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The effect of events between waves on panel attrition

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Panel surveys suffer from attrition. Most panel studies use propensity models or weighting class approaches to correct for non-random dropout. These models draw on variables measured in a previous wave or from paradata of the study. While it is plausible that they affect contactability and cooperativeness, panel studies usually cannot assess the impact of events between waves on attrition. The amount of change in the population could be seriously underestimated if such events had an effect on participation in subsequent waves. The panel study PASS is a novel dataset for labour market and poverty research. In PASS, survey data on (un)employment histories, income and education of participants are linked to corresponding data from respondents' administrative records. Thus, change can be observed for attritors as well as for continued participants. These data are used to demonstrate that changes in household composition, employment status or receipt of benefits have an influence on contact and cooperation rates in the following wave. A large part of the effect is due to lower contactability of households who moved. Nevertheless, this effect can lead to biased estimates for the amount of change. After applying the survey's longitudinal weights, this bias is reduced but not entirely eliminated.

1 Introduction and Research Question

All panel surveys face the problem that units, in most cases households or persons, that initially took part in the study drop out in later waves. If this attrition process does not occur completely at random (MCAR according to Rubin, 1976), there is a threat that estimates for longitudinal populations and cross-sectional populations at later waves will be biased.

There is a vast literature on the correlates of panel attrition that proves that dropout usually does not occur completely at random and that attrition due to non-contact (including failure to locate panel members) and attrition due to refusals can be explained by partly different observed variables (cf. N. Watson and Wooden, 2009 for a recent overview). Variables that affect panel attrition can be classified into survey design features (e. g., number of call attempts, interviewer workload, and interviewer continuity), aspects of the previous wave interview situation (e. g., amount of item nonresponse and interview length), respondent characteristics (e. g., gender, age, education, country of birth, labour force status, number of children and house ownership) and area characteristics (e. g., municipal size and distance from the nearest city).

State of the art methods for the correction of this type of unit-nonresponse in panel studies attempt to model this two-stage dropout process (given participation in the previous wave) using exactly these variables. Note that an additional requirement for a variable to be useful for weighting adjustments is that it is correlated with survey outcomes. Otherwise, its inclusion might increase variance without reducing bias (Little & Vartivarian, 2005). In propensity weighting that is widely used to adjust for attrition in panel surveys, e. g., in the Household, Income and Labour Dynamics in Australia Survey (HILDA; N. Watson and Wooden, 2009) and the German Socio-Economic Panel Study (GSOEP; Kroh, 2010), logit or probit models are used to predict the probabilities of contact and cooperation separately or in a bivariate model. The reciprocal value of the re-participation probability can then be used as a weight to correct for the attrition process. Alternatively, weighting cell approaches can be used in much the same way if the number of categories and variables used to predict participation is small; see the Panel Study of Income Dynamics (PSID, Gouskova, Heeringa, McGonagle, and Schoeni, 2008) and the Survey of Income and Program Participation (SIPP, Westat, 2008) for examples of usage.

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Propensity weighting can correct for attrition only if the dropout process is a missing at random (MAR according to Rubin, 1976) process[1]. That is, conditional on observed covariates, it is random. For attrition in panel studies, the adequacy of this assumption is often doubted. Some authors are concerned (Couper & Ofstedal, 2009), and several studies have indicated (see the literature review below) that change in attributes since the previous wave and events that happened since then could have a strong additional effect on attrition that cannot be accounted for by attributes of the persons or households in the previous wave. Lemay (2009) distinguishes between a sociodemographic and a psychosocial explanation for this: the sociodemographic explanation argues that the individual status has changed and that the current status (which is not measured) is thus what drives participation. The psychosocial explanation, in contrast, places emphasis on the event itself: former participants have to deal with the "shock" exerted by the event and will thus be less likely to participate (Lemay, 2009, 51f.). In addition, many events (such as divorce, childbirth, and new employment) will be accompanied by a change of address and thus make location in a panel survey more difficult. Following this line of argument, it is not people with bad health that are most likely to drop out of the panel but people whose health status has deteriorated since the last interview (Heller & Schnell, 2000). Of course, this would have implications especially on longitudinal estimates as change could be dramatically underestimated if that were true.

2 Previous Research and Contribution of this Article

So far, there have been very few studies that investigate the effect of changes or events between waves on panel attrition. This is because, in panel studies, events or changes between waves are usually observed for those who re-participate in later waves only, while for attritiors, these variables are usually missing.[2] In the past, different paths have been taken to investigate the problem in spite of missing information on changes or events between waves for nonrespondents. Note that we will not discuss the studies here that approach the problem by comparisons of proportions of events estimated from a panel survey to external statistics. One shortcoming of this approach is that it cannot distinguish between underreporting of the event and an association between the event and panel attrition (e.g., Rendtel, 1995, 2002).

Many studies use information on past events or changes, either between the last two waves before dropout or across the whole panel history, to investigate whether change is related to dropout (Fitzgerald, Gottschalk, & Moffitt, 1998; Short & McArthur, 1986; Voorpostel & Lipps, 2011). These studies found higher rates of past fluctuation for attritiors in household size (Short & McArthur, 1986), marital or partner status (Fitzgerald et al., 1998; Voorpostel & Lipps, 2011), employment status (Short & McArthur, 1986; Voorpostel & Lipps, 2011), earnings (Fitzgerald et al., 1998) and household income (Short & McArthur, 1986), and even in soft measures such as satisfaction with one's living arrangements or financial situation (Voorpostel & Lipps, 2011).[3] On the one hand, one might assume that these studies are more likely to underestimate than overestimate effects of change on attrition because they are based on those cases alone that responded at least once after the event or change under scrutiny. However, there is a convincing alternative explanation to the conclusion that true change is related to attrition. It might be that the higher variation in survey answers found for attritiors is a result of lower reliability of their measurements due to a lower motivation, which subsequently results in dropout (cf. Bollinger & David, 2001).

A different approach was chosen by Heller and Schnell (2000), who used data from a study that identified for each attritior from the GSOEP study by register search whether he or she was still alive. They included a dummy variable for death three years later in the propensity models used in the GSOEP and found a highly significant effect even when including additional indicators for health from the previous wave. They could almost double the pseudo-r-square of the GSOEP propensity model. Given that death is in many cases preceded by a deterioration of health, this appears to be evidence that deteriorating health causes dropout.

To our knowledge, the only previous study that made use of external indicators of change for respondents and attritiors is the study by Neukirch (2002). He used data from the 1996 to 1998 waves of the Finnish subsample of the European Community Household Panel (ECHP), which were linked to data on the respondents from various registers[4]. His results confirm many of the findings from the studies reported above that use change in the past as a proxy. Disregarding direction

[1] MAR is a necessary condition, not a sufficient one. Ultimately, propensity models will remove attrition bias only if they are correctly specified, i. e., the functional form of all predictors including interaction effects between predictors is adequately modeled and no relevant predictors are left out.

[2] One exception is household moves. In many panel studies, these can be derived from population registers or from the fieldwork itself. Evidence that household moves are associated with higher panel attrition is overwhelming (e.g., Kroh, 2010; Short & McArthur, 1986; Uhrig, 2008; D. Watson, 2005; N. Watson & Wooden, 2009).

[3] Fitzgerald et al. found these effects only for men. The study by Voorpostel and Lipps is the only one that distinguishes between attrition due to noncontact and attrition due to noncooperation and between temporary and final dropout.

[4] Neukirch used "[. . .] data from the employment statistics, the ministry of labour's register on job seekers, the population statistics and various income data registers" (Neukirch, 2002, p. 5)). Unfortunately, he gives no further information on the register data or the linkage process, nor does he give any references to literature describing the data or linkage.

of change, he found increased attrition for respondents with changes in labour market status. For income, his results are more differentiated. While increases in income go along with higher attrition rates, decreases even correspond to lower attrition rates. As Neukirch notes himself, this might be a disguised effect of working hours (which are not controlled for) on attrition. The strongest effect could be demonstrated for changes of marital status. Even after controlling for household moves, divorces correspond to highly increased attrition. Neukirch examines two transitions separately (1996 to 1997 and 1997 to 1998). For the latter transition, only the effects of income increase and divorce are significant.

The aim of this article is to go beyond this state of research in three respects. First, we will investigate the effect of a variety of events available from external records for respondents and nonrespondents on panel attrition. Among them are indicators that have not been investigated in past research (such as benefit receipt or benefit sanctions) or for which the direction of change has been neglected so far (such as changes in employment status or household composition changes). Second, we will distinguish between the effect those events have on contact and on cooperation conditional on a successful contact. Third, we will investigate whether a biased composition of later wave samples with respect to events experienced between waves still prevails after applying the propensity weights of the PASS survey. Only then would event occurrence be underestimated by survey users.

The remainder of this article is organised as follows. In section three, the data are introduced. In section four, we derive hypotheses on how specific events affect attrition. In section five, we analyse the effect of events between waves and indicate whether propensity models effectively eliminate any resulting biases. In section six, we discuss our results.

3 Data

The panel study PASS (Trappmann, Gundert, Wenzig, and Gebhardt, 2010; Trappmann, Beste, Bethmann, and Müller, 2013) is a novel dataset in the field of labour market, welfare state and poverty research in Germany. With initially nearly 19,000 interviewed persons in more than 12,500 households, PASS is currently one of the most comprehensive panel surveys in Germany. The PASS sample consists of two subsamples. The first subsample is a random sample of households containing at least one recipient of basic income support (called "Unemployment Benefit II"; UB II) in July 2006. This sample is refreshed every year using a random sample of new recipients since the last reference date, while people who overcome recipiency remain in the panel. The second subsample is an area probability sample of households residing in Germany. In PASS, survey data on the employment and unemployment history, income and education of participants can be linked to corresponding data from respondents' administrative records if the participants agreed to the linkage during the interview. For the following analyses, we use data from waves 1 and 2 and field-work protocols of wave 2 and 3 of PASS that have been linked to administrative datasets of the Federal Employment Agency.

The PASS panel is implemented in a sequential mixed-mode design. In the initial wave, CATI was used as the default mode, and noncontacts as well as target persons who expressed preference for the CAPI mode were followed up by CAPI interviews. From wave 2, the previous wave mode became the default mode for the next wave.

PASS applies wide following rules. People who move into PASS households are targeted for the panel as well as respondents who move out of PASS households and additional members of their new households. Temporary dropouts, who did not participate in one panel wave, are followed up in the next wave, unless they are classified as "'hard refusals", in which case German data protection legislation does not allow any further follow-up.

In CATI mode, at least 30 contact attempts under variation of day of the week and time of the day had to be performed. In CAPI, this was limited to 6 attempts under variation of day of the week and time of the day due to the increased costs of contact attempts.

Nevertheless, response rates were rather low and attrition was rather high compared to high-quality studies in other countries. In wave 1, response rates (RR1 according to AAPOR standards; see American Association for Public Opinion Research, 2011) were 29.1 percent in subsample 1 and 23.8 percent in subsample 2. Panel attrition in the PASS panel was particularly high between waves 1 and 2 (see Table 1). Although the higher attrition rate after the first wave is partly due to a less matured panel, this can hardly explain the whole difference. Thus, we can compare one transition with a less than optimal fieldwork with a transition with a much improved fieldwork.

Panel attrition was higher in both years for the UB II recipient sample than for the population sample. This reflects well-known difficulties in surveying welfare populations (Ver Ploeg, Moffitt, & Citro, 2002). They are harder to locate and contact as they move more often and leave fewer traces of where they can be found (Weiss & Bailar, 2002). Whether they are also less likely to cooperate given contact depends very much on the survey topic. In PASS that addresses social security in Germany, they are more likely to cooperate in the initial wave but less likely to cooperate in subsequent waves.

The administrative datasets used in the analysis are from different sources. Information on employment is from the Employment Histories (BEH). This dataset is based on regular notifications by employers to the German labour administration. This notification is compulsory for all legal employment, except for self-employment and employment as a civil servant (German: "Beamte"). It contains information

Table 1
PASS Attrition Rates by Sample and Wave

	Attrition rate on person level (in %)	
Sample	Wave 1 to 2	Wave 2 to 3
Population sample	39.0	22.3
UB II sample	54.4	29.1
Whole sample	46.6	25.2

on the individual employment histories, including the exact beginning and end of all employment episodes.

Additionally, we linked the survey data and paradata to two datasets on UB II-recipiency (called LHG and XLHG). Both datasets are drawn from administrative systems used by local employment agencies to administer the payment of unemployment benefits. As two different standards of data entry exist in Germany, this results in two different datasets. Together, these two datasets cover all recipients of UB II. They include information on all episodes of receipt and, during times of receipt, additional information on place of residence and household composition. For an overview of the administrative datasets used in this article, see (Dorner, Heining, Jacobebbinghaus, & Seth, 2010a).

The administrative data contain numerous variables for which we suspect that changes between waves affect panel attrition. In the subsequent analyses, we will use employment status, UB II receipt, sanctions within the UB II system, place of residence, family status and household size. All indicators for change between waves are constructed from the administrative datasets. Household related information from the administrative data (family status, household size) is only available for UB II recipients and can thus only be analysed for respondents who receive benefits at the time of both waves. Therefore, two analytic subpopulations will have to be constructed in the analysis section. Changes in employment status and benefit receipt can be analysed for the whole analysis sample, but changes related to family status, household size and moves will be analysed for continuous benefit recipients only.

The administrative datasets are highly reliable with respect to the variables used in this analysis because they are not only collected for statistical purposes but are the basis for payment of social security transfers. Data on employment are used by the German Statutory Pension Insurance (Deutsche Rentenversicherung) to calculate the amount of pension-receipt (Dorner, Heining, Jacobebbinghaus, & Seth, 2010b). An employment spell that is not included in the data is equivalent to undeclared work. Information on household composition, family status and place of residence is essential for the payment of unemployment benefits. False statements can lead to serious sanctions and prosecution for benefit recipients and employers.

Note that for the subsequent analyses, not all cases from the PASS panel can be used. First, we exclude all respondents who are 65 years old or older because usually no administrative data are available after that age. To use the combined dataset of PASS data and administrative data, the sample has to be restricted to cases that agreed to the linkage and could successfully be linked. Linkage consent was given by 79.8 percent of wave 1 respondents; 92.0 percent of consenters could actually be linked using probabilistic record linkage procedures based on Jaro (1989). Sakshaug and Kreuter (2012), who compare nonconsent bias in this survey to other error sources, find that it is smaller than nonresponse and measurement error bias. They find small bias for age and foreign nationality and no bias for substantive variables. Furthermore, many variables are only available for recipients of UB II. We exclude the wave 2 refreshment sample from our analysis and treat temporary dropouts in wave 2 who returned to the sample in wave 3 as permanent dropouts.[5] All analyses are on the person level. Table 2 displays the selection steps for the subsequent analyses.

4 Hypotheses

We assume transitions from non-employment to employment to affect contact rates as well as cooperation rates. People with such transitions should be more difficult to locate as they may have moved as a consequence of finding a job, more difficult to contact as they should spend more time outside their homes, and exhibit less cooperation due to stronger time constraints. This effect should increase with working hours of the job.

A similar argument holds for transitions out of UB II receipt.[6] We expect that people who have overcome UB II are less likely to participate as they should be more difficult to locate and contact (e. g., due to someone in their household gaining employment). We also expect them to be less cooperative as two potential arguments for participation, an increased interest in the topics of the survey (i. e., the system of social security in Germany) and a perceived obligation to

[5] For many applications of the panel data that require a balanced panel (i. e., the subset of cases that participated in all waves under consideration), this is the set of cases that can be used for analyses. Furthermore, in section 6, we will analyze whether any biasing effects of attrition due to events between waves can be corrected using propensity weights. The PASS propensity weights only refer to the balanced panel (Trappmann, 2011).

[6] UB II receipt and unemployment, though positively correlated, are far from being identical. Approximately one-quarter of recipients are employed (so-called supplementary recipients; cf. Dietz, Müller, and Trappmann, 2009) and a majority of people who are not employed (i. e., unemployed or not in the labor force) do not depend on UB II as their household income comes from other sources such as unemployment insurance, pensions or other household members' job-related income.

Table 2
Sample Selection

	n	Percent of previous row
Transitions wave 1 to wave 2		
# participants in wave 1 (15-64 years)	17,249	
Linked to administrative data	12,659	73.4
Linked UB II Recipients in wave 1	6,049	47.8
Linked UB II Recipients in wave 1 and 2	4,932	81.5
Transitions wave 2 to wave 3		
# participants in wave 2 (15-64 years)	9,927	
Linked to administrative data	6,949	70.0
Linked UB II Recipients in wave 2	2,722	39.2
Linked UB II Recipients in wave 2 and 3	2,195	80.6

participate in exchange for receiving benefits, no longer apply.

For people who received a sanction within the UB II system, i. e., their benefits were cut due to misbehaviour, we expect no effect on the contact rate but a decreased cooperation rate due to a reduced sense of obligation towards the sponsor of the study, the Ministry of Labour and Social Affairs.

In agreement with most previous studies (compare footnote 2), we assume that having moved between two waves decreases the probability of achieving contact but has no effect on cooperation once contact is achieved.

For people who lived with a partner in the previous wave but no longer live with a partner in the subsequent wave, we expect lower contact rates and lower cooperation rates. A separation or death of a partner increases the probability of moving cf., Rendtel (1995, 2002)) and also increases the probability that the former participation decision that was associated to the old household and partner will be re-evaluated.

Respondents who have started cohabiting with a partner since the previous wave are more likely to have moved than those who did not experience changes in cohabitation status. We assume that this decreases contactability in spite of the additional adult in the household. In addition, as PASS is a household panel, the new couple might re-evaluate the participation decision so that there is a higher than average probability to withdraw the previous cooperation.

The birth of a child is also likely to affect participation in a panel survey. On the one hand we assume that people with new born children are more often at home and thus easier to contact (although they may also demonstrate an increased propensity to move). On the other hand, new-borns impose severe time-constraints on their parents, which might be associated with refusals due to busyness.

5 Results: Events between waves and panel attrition

This section is subdivided into three parts. In the first subsection, we will investigate bivariate associations between change or events between waves and panel attrition and determine which events lead to increased panel attrition. In the second part, we will estimate regression models on contact and cooperation including all events simultaneously. In the third part, we will investigate whether associations between change and attrition found in the first part still lead to biased estimates of the amount of change after the propensity weights of the survey have been applied. All analyses are performed disregarding initial wave PASS weights. It is important to note that while we can analyse changes in employment and benefit status for all respondents, we can evaluate all other events only for those people who remained on benefits between two successive waves.

5.1 Bivariate Results

The analysis in this section follows a straightforward strategy. In Table 3 and Table 4, attrition rates are presented for persons with and without the above defined events between two subsequent waves according to the administrative data. We will differentiate between attrition due to noncontact and attrition due to noncooperation (given contact). Table 3 gives the results for all events that can be identified in the administrative data across the two waves. Table 4 displays the same rates separately for the two transitions. For efficient presentation of the results, we will focus on Table 3 and resort to Table 4 only when the results differ between the two transitions.

Note that there are some strong main effects of the previous wave status (rows in italics). For example, from Table 3 one can derive that contact rates are 6.3 percentage points lower for respondents who were not employed in the prior wave (77.9%) than for those who were employed (84.2%). This main effect can easily be adjusted for in propensity models and should not be focused on here. We will thus only compare respondents with a certain event to those who were at risk of experiencing the same event but did not experience it. For example, the benchmark for those who experienced a transition off benefits is only those who remained on benefits. Thus, Table 3 and 4 are organised as follows. The rows in italics are subtotals of those who are at risk of experiencing a certain event. Significance tests compare the proportion in a given row to the proportion in the subtotal in italics above using chi-squared tests of independence. The chi-squared statistic is corrected for the complex survey design with the second-order correction of (Rao & Scott, 1984) and is converted into an F statistic using the Stata 12 "svy: tabulate twoway" command.

Employment. We find lower contact and cooperation rates for those who experienced changes of employment status than for those whose employment status remained unchanged. Previously, not employed people were less likely to be contacted and less likely to cooperate when they became employed than when they remained unemployed (−3.1 percentage points for contact rates and −2.0 percentage points for cooperation rates). Only the first of these two differences is significant. However, if we focus on only those respondents who took full-time employment, the effects become much stronger and both differences (−7.9 percentage points for contact, −5.5 percentage points for cooperation) are significant. Thus, the data support hypothesis 1a and, for full-time employment, 1b (in that we can reject the associated null-hypothesis on a 5 percent level in a two-sided test). Table 4 indicates that the sign of the association is stable across waves, but with a better fieldwork in wave 3, the differences in contact rates are not as pronounced[7].

Welfare benefit receipt. The effect of welfare benefit receipt exhibits a similar pattern that is, as expected, even more pronounced. Respondents with changes off benefits exhibit the lowest contact and cooperation rates. In effect, they are more than 10 percentage points less likely to participate in the following wave than people who stayed on benefits. The result is significant in both waves for both cooperation and contact. Respondents who newly pick up benefits are on the other hand slightly less likely to be contacted (4.5 percentage points) but exhibit similar cooperation rates as respondents who remained off benefits. Hypotheses 2a and 2b gain support from the data.

Sanctions for benefit recipients. The following events can only be analysed for respondents who remain on benefits for two subsequent waves. The number of cases for analysis drops from 19,608 to 7,127. We find no consistent effects for people whose benefits were cut by the authorities. Contact and cooperation rates are somewhat lower in the sum of the two transitions (−1.9 percentage points for contact, −3.6 percentage points for cooperation) than for other recipients. The sign for cooperation changes, however, across the two transitions as observed in Table 4. We cannot reject the null hypothesis associated with hypothesis 3. As there are only 145 cases who experienced sanctions in the sample, this might be due to low statistical power.

Change of address for benefit recipients. Benefit recipients who moved are much less likely to be contacted. We find this difference to be strongly affected by the quality of the fieldwork as indicated by the overall contact rate. While they are only 8.8 percentage points less likely to be contacted in wave 3, they are 24.6 percentage points less likely to be contacted in wave 2. Thorough fieldwork can thus decrease the amount of bias introduced by relocations but not eliminate it. There is overwhelming support for hypothesis 4. In addition, movers are also somewhat less likely to cooperate (−5.0 percentage points, significant only between waves 2 and 3), which might be due to confounding with the above-mentioned changes in benefit receipt or employment.

Changes in household composition for benefit recipients. We find stronger effects for changes in partnership status than for new children in the household. Persons who separated from their previous wave partners are the group that is least often contacted. In the sum of the two waves, the chance of contacting such a person is less than 50%. This is 28.4 percentage points less than other recipients and cannot entirely be explained by an increased chance of moving as the contact probability is even less than that of people who moved. The differences in cooperation rates for both waves point in the expected direction (−3.9 percentage points) but are not significant (on the 5 percent level for a two-sided test). Thus, the data support hypothesis 5a but not 5b. The latter might be due to a low statistical power as only 146 separations are observed.

Living with a new partner also decreases the probability of contact dramatically (−20.7 percent). There is no difference in cooperation rates though (−1.0 percentage points) for persons who live with a new partner. Thus, there is support for hypothesis 6a but not for 6b.

Both effects (new partner, separation) on contact rates are much more pronounced in the transition from wave 1 to 2 with higher overall attrition rates. They are both not significant for the transition from wave 2 to 3, although this might be due to a very small number of events (29 separations, 25 new partners) between waves 2 and 3.

Finally, a new child in the households corresponds to (insignificantly) lower contact rates (−3.3 percentage points), contrary to our expectations. Cooperation is significantly decreased by 6.5 percentage points in the presence of a new child. The results are very similar in size across waves. However, the effect on cooperation is insignificant when testing it

[7] As we acknowledged in section 3, we are only able to analyze changes with respect to employment subject to social insurance contributions, which excludes the self-employed as well as civil servants and undeclared work.

Table 3
Contact and cooperation rates for persons by event (averaged over waves 2 and 3)

Event	n	Contact rate (%)	Cooperation rate (%)	Response rate (%)
Events defined for whole sample				
Total	19608	80.5	79.4	62.4
All not employed in previous wave	*11388*	*77.9*	*78.5*	*59.7*
No employment → employment	1595	75.2**	76.7	56.2**
No employment → full time employment	809	70.4**	73.2**	50.0**
In both waves no employment	9793	78.3**	78.7	60.2**
All employed in previous wave	*8220*	*84.2*	*80.6*	*66.3*
In both waves employment	7007	85.2**	80.9	67.5**
Change to no employment	1213	78.1**	78.3	59.3**
All UB II recipients in previous wave	*8771*	*74.9*	*78.5*	*57.4*
UB II exit	1644	67.3**	72.9**	47.9**
UB II in both waves	7127	76.7**	79.6**	59.6**
All non-recipients in previous wave	*10837*	*85.1*	*80.0*	*66.5*
UB II entry	429	80.7*	79.6	62.7
No UB II in both waves	10408	85.2*	80.1	66.6
Events defined only for UB II continuous				
Total	7127	76.7	79.6	59.6
Partner left household	146	48.3**	75.7	36.3**
New partner in household	105	56.0**	78.6	41.9**
New child in household	395	73.4	73.1*	52.2**
UB II sanction	145	74.8	76.0	54.5
Moved to other district	358	56.6**	74.6	41.1**

Two-tailed chi-square tests compared to the subtotal above (in italics).
* $p < 0.05$ ** $p < 0.01$

for each wave separately. Hypothesis 7a thus has no support, but hypothesis 7b is supported.

5.2 Multivariate results

The purpose of the previous subsection was to demonstrate how events affect contact and cooperation rates of respondents who experience them. That section focused on a gross effect of the event, i. e., the average effect of a separation of a couple on contactability includes any indirect effects via a higher propensity to move after a separation. This gross effect is important as it informs us about possible biases in longitudinal estimates of these events. However, as already mentioned, the occurrence of some of the events is highly correlated, and readers might be interested in the effects of one event controlling for other events. For example, any changes in household composition, employment status or benefit receipt may lead to moves. Thus, moves might be the underlying cause for increased attrition after any of these events. We used logit regression models explaining contact and cooperation given contact in which we included all events simultaneously and controlled for previous wave status. The analyses were performed using Stata 12's "svy: logit" command, taking into account clustering and stratification of the complex survey design of PASS.

Again, not all variables can be included simultaneously. As in the previous section, sanctions, moves and family-related events can only be included for continuous welfare benefit recipients. Thus, in these models, the sample is reduced, and the effect of dynamics in benefit receipt cannot be estimated. Thus, two models are estimated for each wave transition and each nonresponse stage, leading to eight models altogether. All model coefficients and p-values of corresponding t-tests are presented in Table 5. For the convenience of the reader, coefficients and p-values referring to events are in italics.

Models (1) to (4) include changes in benefit receipt and employment status simultaneously. The results indicate that welfare benefit exits still have a significantly negative effect on contact and cooperation after controlling for changes in

Table 4
Contact and cooperation rates for persons by event and wave

	n	Contact rate (%)	Cooperation rate (%)	Response rate (%)
Events wave 1 → 2, whole sample				
Total	12659	74.0	76.5	54.9
All Not employed in wave 1	*7604*	*71.4*	*75.5*	*52.2*
No employment → employment	1132	69.3	74.3	49.9
No employment → full time employment	539	64.2**	70.1*	43.4**
In both waves no employment	6472	71.8**	75.7	52.6**
All employed in wave 1	*5055*	*78.0*	*77.0*	*58.9*
In both waves employment	4306	79.5**	78.2	60.4**
Change to no employment	749	68.9**	76.5	50.5**
All UB II recipients in wave 1	*6049*	*68.6*	*74.8*	*49.7*
UB II exit	1117	59.1**	69.1**	39.8**
UB II in both waves	4932	70.7**	75.8**	52.0**
All non-recipients in wave 1	*6610*	*79.1*	*78.0*	*59.6*
UB II entry	273	73.8	81.4	57.9
No UB II in both waves	6337	79.3	77.8	59.7
Events wave 1 → 2, UB II in both waves only				
Total	4932	70.7	75.8	52.0
Partner left household	117	39.7**	76.1	29.9**
New partner in household	80	46.1**	77.1	33.8**
New child in household	302	68.9	69.8	46.7
UB II sanction	104	73.5	72.2	50.0
Moved to other district	253	46.1**	74.1	32.8**
Events wave 2 → 3, whole sample				
Total	6949	92.1	83.5	76.1
All not employed in wave 2	*3784*	*90.6*	*83.0*	*74.6*
No employment → employment	463	89.1	81.2	71.7
No employment → full time employment	270	88.2	79.8	68.8
In both waves non-employment	3321	90.8	83.3	75.0
All employed in wave 2	*3165*	*93.9*	*84.0*	*78.0*
In both waves employment	2701	94.1	84.6*	78.8
Change to non-employment	464	92.6	80.4*	73.5
All UB II recipients in wave 2	*2722*	*88.8*	*84.7*	*74.5*
UB II exit	527	84.5**	78.5**	65.1**
UB II in both waves	2195	89.8**	86.1**	76.8**
All non-recipients in wave 2	*4227*	*94.2*	*82.7*	*77.2*
UB II entry	156	92.3	77.1	71.2
No UB II in both waves	4071	94.3	82.9	77.4
Events wave 2 → 3, UB II in both waves only				
Total	2195	89.8	86.1	76.8
Partner left household	29	82.8	75.0	62.1*
New partner in household	25	87.5	81.0	68.0
New child in household	93	87.9	81.3	69.9
UB II sanction	41	78.1*	84.4	65.9
Moved to other district	105	81.0**	75.3**	61.0**

Two-tailed chi-square tests compared to the subtotal above (in italics)
* $p < 0.05$ ** $p < 0.01$

Table 5
Logit-Models explaining contact and cooperation

	(1) Wave 1/2 contact		(2) Wave 1/2 cooperation		(3) Wave 2/3 contact		(4) Wave 2/3 cooperation	
	Coef.	$P > \|t\|$	Coef.	$P > \|t\|$	Coef.	$P > \|t\|$	Coef.	$P > \|t\|$
In both waves non-employment (Ref. category)								
Change to employment	*0.003*	*0.970*	*-0.008*	*0.928*	*-0.070*	*0.659*	*-0.071*	*0.617*
In both waves employment	0.265	0.000	0.104	0.093	0.282	0.012	0.181	0.014
Change to non-employment	−0.201	0.034	0.017	0.872	0.158	0.418	−0.137	0.259
No UB II in both waves (Ref. category)								
UB II entry	*-0.193*	*0.309*	*0.256*	*0.180*	*0.264*	*0.425*	*-0.304*	*0.172*
UB II in both waves	−0.368	0.000	−0.074	0.200	−0.527	0.000	0.310	0.001
UB II exit	−0.900	0.000	−0.415	0.000	−1.012	0.000	−0.223	0.088
Linear combinations								
Chg. to non-empl. − both waves empl.	*-0.466*	*0.000*	*-0.087*	*0.427*	*-0.124*	*0.568*	*-0.318*	*0.007*
UB II exit − UB II both waves	*-0.533*	*0.000*	*-0.341*	*0.001*	*-0.486*	*0.004*	*-0.533*	*0.000*

	(5) Wave 1/2 contact		(6) Wave 1/2 cooperation		(7) Wave 2/3 contact		(8) Wave 2/3 cooperation	
	Coef.	$P > \|t\|$	Coef.	$P > \|t\|$	Coef.	$P > \|t\|$	Coef.	$P > \|t\|$
In both waves non-employment (Ref. category)								
Change to employment	*0.128*	*0.269*	*0.199*	*0.173*	*-0.072*	*0.795*	*-0.126*	*0.613*
In both waves employment	0.186	0.054	0.092	0.421	0.083	0.687	−0.093	0.596
Change to non-employment	−0.074	0.602	0.133	0.456	0.294	0.351	−0.105	0.686
No partner in both waves (Ref. category)								
New partner in later wave	*-0.913*	*0.001*	*-0.179*	*0.647*	*0.123*	*0.850*	*-0.378*	*0.517*
Partner in both waves	−0.039	0.636	−0.592	0.000	0.019	0.917	−0.583	0.000
Separation	−1.277	0.000	−0.179	0.612	−0.480	0.390	−0.875	0.052
New child in household	*0.891*	*0.577*	*-0.265*	*0.187*	*-0.161*	*0.671*	*-0.269*	*0.336*
UB II sanction	*0.115*	*0.680*	*-0.081*	*0.806*	*-0.986*	*0.017*	*-0.030*	*0.945*
Moved to other district	*-0.964*	*0.000*	*-0.102*	*0.712*	*-0.792*	*0.002*	*-0.677*	*0.027*
Linear combinations								
Chg. to non-empl. − both waves empl.	*-0.260*	*0.108*	*0.041*	*0.841*	*0.210*	*0.583*	*-0.012*	*0.965*
Separation − partner both waves	*-1.238*	*0.000*	*0.413*	*0.267*	*-0.499*	*0.380*	*-0.292*	*0.519*

employment status.[8] As in the bivariate models, changes from employment to non-employment lead to lower contactability only in wave 2 and lower cooperation only in wave 3. For the other direction of change (into employment, into benefit receipt) no significant effects are observed, which is in line with the results from the bivariate analysis.

Models (5) to (8) include changes in employment status, moves and family related events simultaneously. They can only be estimated for benefit recipients at two consecutive waves. Thus, they lack power and generalisability. Nevertheless, their merit lies in providing information about the effect of other events after controlling for household moves. Moves have a pronounced and highly significant negative effect on contact in all waves and on cooperation in wave 3 after controlling for other events and changes in employment status. Changes to non-employment no longer have a negative effect (neither on contact nor on cooperation) when controlling for moves and other family related events. This is not due to reduced statistical power alone. The effects are much smaller than without control of these events and in two cases change to a positive sign. The effect of family-related events is clearly reduced by including moves. Separations and a new partner have a negative effect on contact only in wave 2 and

[8] The appropriate test is the test of the linear combination of the effect of welfare benefit exits minus continuous benefit receipt.

no effect on cooperation. A new child in the household has no effect on contact or cooperation. Benefit sanctions have a negative effect on contact only in wave 3. Again, we find no effect for changes to employment. After controlling for moves, no other event in models (5) to (8) has a consistently significant effect for both transitions. This suggests that successfully tracking movers is key to removing event-related bias from panel surveys.

Summarising the results from this section, it appears that a large part of the effect of events between waves on panel attrition can be explained by a higher propensity to move that is associated with nearly every event considered in this article. Unfortunately, this cannot be tested for the strong effect of exiting welfare benefit receipt as moves are only observed in the administrative data for those who remain in welfare receipt.

5.3 Do propensity models eliminate the bias?

In the previous subsections, we have been able to demonstrate that events that happen between panel waves can have a substantial impact on panel attrition in the subsequent wave. One important question now is whether the longitudinal weights of a panel survey can neutralise this effect. One might argue, for example, that household moves will become apparent in the pattern of contact attempts or that deterioration of health is mainly driven by age and that therefore, when controlling for these variables, the effect of events between waves on panel attrition is indirectly captured in propensity weighting models, although the event itself is not observed.

Increased attrition rates for respondents who experienced an event between waves lead to biased estimates of the proportion of respondents who experienced this event in later panel waves, as long as this is not adjusted by weights. In this subsection, we will first calculate this relative bias by comparing the proportion of wave n respondents who experienced a certain event between waves n and $n+1$ to the proportion of wave $n+1$ respondents who experienced the same event between waves n and $n+1$ (Table 6, columns 1-3). In a second step, we weigh respondents from wave $n+1$ according to PASS propensity weights for the transition and then calculate the bias again. Thus, we can evaluate the extent to which bias is reduced by propensity models in PASS.[9]

The construction of the PASS propensity weights for person interviews is described in (Buengeler, Gensicke, Hartmann, Jaeckle, & Tschersich, 2009) for the transition from wave 1 to 2 and in (Berg et al., 2010) for the transition from wave 2 to 3. Propensity models in PASS model the stages of contact and cooperation separately. Person and household attributes from the previous interview (age, gender, nationality, language, education, working hours, income, UB II recipiency, self-rated health, life satisfaction, children in certain age groups in the household, and house ownership), attributes of the regional context (state and municipal size), attributes of the previous wave interview situation (interview mode, length, missing values, completeness, and subsample) and attributes of the fieldwork of the current wave (number of contact attempts in CATI/CAPI) have been used as predictors.

Column 4 in Table 6 indicates the extent to which the propensity weights in PASS reduce attrition bias for the indicators of change. The results indicate that the PASS propensity models are adequate in reducing bias for new full-time employment. The bias is reduced by more than 74 percent. Furthermore, in both waves, the bias in exits from receiving benefits is reduced by more than half. For people giving up employment, bias is still reduced by 20.8 per cent. However, the current propensity models are not good at reducing bias related to household moves (bias is reduced by approximately 10 percent for the first transition but even increased by 2 percent in the second transition) and to new partners in the household (an increase in bias of 3.5 percent). For partners leaving the household, the propensity weights decrease bias in wave 2 by 5 percent. In contrast, in wave 3, there is an overcorrection of bias. The proportion of persons whose partner left the household is too high by 12 percent after applying the weights. Note that case numbers for household related events are very small. Still, the results indicate that the existing weights do a reasonable job at reducing attrition bias, although they contain no information on the events themselves.

6 Conclusions and Discussion

This article uses a rare combination of survey data and administrative records to investigate the effect of events between waves on panel attrition in the subsequent wave. We could demonstrate that events between waves such as changes in labour market status or welfare receipt, changes in household composition and household relocations can have a pronounced effect on panel attrition.

In our bivariate analysis, we found exits from welfare benefits and new full-time employment between two waves of a panel survey to be significantly negatively associated with contact and cooperation in the subsequent wave, whereas changes from employment to non-employment and the start

[9] Original PASS longitudinal weights that were derived from propensity models for the complete sample are used in the following analyses. In contrast, in the analyses presented here, only cases that could be linked to administrative data are used. However, there are no obvious reasons why patterns of attrition should differ between consenters and nonconsenters. As a test, the authors replicated the PASS weighting procedure once for the full sample and once for the analysis sample only. Full sample weights and analysis sample weights have a Pearson correlation of 0.985. The difference between the two sets of weights is negligible, and the analysis is continued with the official survey weights.

Table 6
Attrition bias before and after propensity weights are applied

	(1) Wave n	(2) Wave $n+1$	(3) Relat. bias	(4) (2) + PASS weights	
	Prop. (%)	Prop. (%)	%	Prop.	Bias red. (%)
Wave 1 → 2					
Events defined for whole sample					
No employment → full time employment	4.26	3.37	−20.9	4.03	74.2
Change to no employment	5.92	5.44	−8.1	5.54	20.8
UB II exit	8.82	6.39	−27.6	7.78	57.2
Events defined only for UB II in both waves					
Moved to other district	5.13	3.24	−36.8	3.44	10.6
Partner left household	2.37	1.37	−42.2	1.42	5.0
New partner in household	1.62	1.05	−35.2	1.03	−3.5
Wave 2 → 3					
Events defined for whole sample					
UB II exit	7.58	6.48	−14.5	7.10	56.4
Events defined only for UB II in both waves					
Moved to other district	4.78	3.80	−20.5	3.78	−2.0
Partner left household	1.32	1.07	−18.9	1.35	112.0

of receiving benefit affect only contact negatively. For family related events, we found negative effects on contact of partners joining or leaving the household and a negative effect of new children in the household on cooperation. Furthermore, household moves significantly reduce the likelihood of contact.

This effect induces a bias in key variables of the survey such as the proportion of benefit recipients who overcame recipiency between two waves. We found that this bias is for the most part reduced but not entirely eliminated when the propensity weights of the survey are applied.

Multivariate models indicated that relocations associated with the events of interest (each event except exits from employment is positively correlated with relocation) might explain a large part of the effects on contact. Thus, the not-too-surprising lesson for fieldwork is that a good sample maintenance and tracking are keys to avoiding bias concerning not only relocations but also events such as changes in employment status, separations and new partners moving in. When data have already been collected and moves between waves can be demonstrated to have an effect on attrition, survey managers may be well advised to include information from paradata about respondents no longer residing at the previous wave address in propensity models.

Furthermore, if one is worried about unobserved events between waves affecting attrition, it can be helpful to collect at least proxy information on those unobservables. Paradata and survey variables collected in the previous wave interview might be predictive of such unobserved events. One direction for future research might be to investigate which variables to include in the propensity models to effectively adjust for attrition bias due to events between waves. Kreuter and Jäckle (2008) suggested focusing on changes in patterns of contactability (as proxies for changes in employment status). In addition, one might include survey information such as job search intensity, satisfaction with dwelling, satisfaction with marriage or similar available variables that are predictive of events and that are not regularly used in propensity models.

In the multivariate models, significant effects on cooperation can only be found for exits from welfare benefits. This effect might be specific to the PASS survey. Although the survey covers miscellaneous labour market and income related topics, there is a focus on welfare receipt for those who report to have received these benefits. Thus, respondents who overcame receipt might perceive that the survey no longer applies to them despite all conversion attempts by the interviewer. Furthermore, their initial participation, at least for some of them, might have been induced by a sense of obligation as a recipient of public money to respond to a survey request on social security topics. Having moved out of benefit receipt, this sense of obligation may have vanished.

This brings us to the limitations of our study. The PASS survey has an exceptionally high attrition rate between waves 1 and 2. Between waves 2 and 3, attrition is lower, but at 25 percent, it is still considerably higher than for most international benchmark panel studies. There are several reasons for the high attrition rate. In addition to mode (approximately 70 percent of wave 1 were performed in CATI mode) and population (approximately half of wave 1 respondents were benefit recipients), some of the reasons are specific to Germany, where the market for scientific face-to-face-surveys is oligopolistic (usually only two survey organisations participate in calls for tenders), and public opinion towards even scientific surveys is low after miscellaneous data protection scandals (not related to scientific surveys) and frequent telephone marketing calls in the disguise of surveys. Comparing effects of events between waves on panel attrition between the first transition (wave 1 to 2) with high attrition and the second transition (wave 2 to 3) with reasonable attrition indicates that effects of events on contactability could be much reduced by the better fieldwork in wave 3. Thus, effects of events between waves might be exaggerated by the use of a panel survey with high attrition.

A second limitation is that family-related events and relocations between waves could only be identified in the administrative data for those respondents who received benefits in both waves. Thus, we cannot be certain we would find the same effects for general population samples. In addition, analyses could only be performed for those respondents who agreed to have their survey data linked to administrative data and who were successfully linked. This might bias the results compared to the full sample. However, replicating the wave 1 longitudinal weighting, we found that the weights estimated from our analysis sample and from the full sample correlate almost perfectly. This indicates that although consenters are more cooperative than non-consenters, the factors driving nonresponse scarcely differ between the analysis sample and the full sample.

The number of surveys linking their data with administrative data is increasing. Thus, it should be possible in the near future to compare the results from PASS to the results from surveys in other countries, with different populations, different modes of data collection, and with widely varying attrition rates.

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