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## Differential attrition in health behaviour change trials: A systematic review and meta-analysis

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**Objective:** Attrition is a common problem in health behaviour change (HBC) trials. When the degree of attrition differs between treatment conditions, then this is called differential attrition and is regarded as a major threat to internal validity. The primary research question of this study was: how often and to what degree does differential attrition occur in HBC trials?

**Design:** A systematic review and meta-analysis of a random selection of HBC trials ( $k = 60$ ). We meta-analysed the relative attrition rates using a random-effects model and examined the relationship between the relative attrition rates and the potential moderators: the amount of human contact in delivery and the intensity of the intervention/control condition, the type of control condition, and the follow-up intensity and duration.

**Main outcome measures:** Relative attrition rates.

**Results:** The average attrition rate was 18% ( $SD = .15$ ;  $M = .15$ ) in the intervention and 17% ( $SD = .13$ ;  $M = .13$ ) in the control conditions. The estimated average relative attrition rate was 1.10 (95% CI: 1.01–1.20,  $p = .02$ ), suggesting an overall higher attrition rate of 10% in the intervention conditions. This relative attrition rate was not related to any of the potential moderators.

**Conclusion:** There is indication of a slightly higher amount of attrition on average in the intervention conditions of HBC trials.

**Keywords:** differential attrition; bias; RCT; health behaviour change; internal validity

Attrition or loss to follow-up after randomisation is a common problem in randomised controlled trials (RCTs) (de Bruin, McCambridge, & Prins, 2015; Dumville, Torgerson, & Hewitt, 2006; Schulz & Grimes, 2002). It complicates the statistical analyses and can lead to bias in the findings (Molenberghs & Kenward, 2007). When the degree of attrition differs between the intervention and control conditions in an RCT, then this is typically called ‘differential attrition’. Participants in these conditions are, if random allocation is undertaken properly, comparable at baseline (e.g. in terms of prognostic factors). Therefore, differential attrition can be assumed to be a consequence of differences that arose at some point after randomisation and can be related to perceived efficacy or tolerability of

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the intervention (Stein, Ipser, & Seedat, 2006). It might also be, for example, that participants who are 'treated' in the intervention condition feel a general obligation to complete follow-up measurements (Wortman, 1978) compared to those on a waiting list.

Differential attrition is usually regarded as a major threat to the internal validity of a study (i.e. whether the intervention really did cause behaviour change) (Shadish, Cook, & Campbell, 2002), but insight into the degree of differential attrition occurring in RCTs is limited. Previous meta-analyses focusing on a single clinical area found no differential attrition in trials regarding interventions aimed at self-monitoring of blood glucose in type two diabetes (Heneghan et al., 2007) and the use of serotonin specific reuptake inhibitors in the treatment of posttraumatic stress disorder (Lurie & Levine, 2010), but did find significant differential attrition when comparing atypical and typical antipsychotic medications (Rabinowitz, Levine, Barkai, & Davidov, 2009). In a convenience sample of 10 trials evaluating interventions for the treatment of musculoskeletal disorders, all trials showed some level of differential attrition, ranging from 1 to 14% (Hewitt, Kumaravel, Dumville, & Torgerson, 2010). Our group recently published a meta-analysis of a random sample of RCTs published in general medical journals, but we observed no evidence that differential attrition generally occurred (Crutzen, Viechtbauer, Kotz, & Spigt, 2013).

The current study focuses purely on behavioural/educational trials, which are aimed at health behaviour change (HBC). In other words, changes in behaviour that are directly related to improvement in health and/or quality of life. HBC trials are generally thought to be at higher risk of bias than pharmaceutical/medical trials (Crocetti, Amin, & Scherer, 2010). This increased risk is due to bias-reducing measures (e.g. blinding) being more difficult to implement in HBC trials (Boutron et al., 2007). A previous meta-analysis found differential attrition in HBC trials aimed at HIV-prevention, favouring the control condition, which suggests that attrition per condition may be 'unique and different from general participation in research' (Noguchi, Albarracín, Durantini, & Glasman, 2007). In other words, these differences in attrition might be the result of perceived differences in treatment administered (e.g. intervention vs. control) that are more likely to occur if participants cannot be blinded – as is mostly deemed to be the case in HBC trials (Boutron, Tubach, Giraudeau, & Ravaud, 2004). Within the area of hip and knee osteoarthritis therapy, for example, non-pharmacological studies (i.e. including HBC trials) less often described blinding of participants in comparison with pharmacological studies (26.0% vs. 96.7%) (Boutron, Tubach, Giraudeau, & Ravaud, 2003). There might also be differences between HBC trials concerning certain behaviours (e.g. less attrition if the behaviour is related to a more serious health problem) or certain types of interventions. For example, Internet-delivered interventions are thought to be more prone to attrition (Kohl, Crutzen, & De Vries, 2013). However, this concerns differences between trials in absolute attrition rates and not necessarily differential attrition.

To our knowledge, no study has examined the amount of differential attrition in HBC trials in general (i.e. not focused on a single clinical area or behaviour). This makes it impossible to draw general conclusions about the degree of differential attrition in HBC trials. We therefore conducted the present study to address the following primary research question: How often and to what degree does differential attrition occur in HBC trials? The secondary research question was: which factors may be related to the degree to which differential attrition occurs in such studies?

## Methods

We conducted a systematic review and meta-analysis of a random selection of RCTs published in 10 major health psychology/behavioural medicine journals over the last decade (2003–2012). We would like to acknowledge a recently made plea for full disclosure to maximise scrutiny, foster accurate replication and facilitate future data syntheses (Crutzen, Peters, & Abraham, 2012; Peters, Abraham, & Crutzen, 2012). Therefore, the search results, screening list, and the list of included and excluded studies, as well as data, syntax and output of the analyses are available at [www.sciencerep.org/11](http://www.sciencerep.org/11).

### *Search strategy*

To identify the RCTs, we searched PubMed for articles with the following query: (*'Psychol Health' [JO] OR 'Health Psychol' [JO] OR 'Br J Health Psychol' [JO] OR 'J Health Psychol' [JO] OR 'Health Educ Res' [JO] OR 'Health Educ Behav' [JO] OR 'Ann Behav Med' [JO] OR 'J Behav Med' [JO] OR 'Int J Behav Med' [JO] OR 'Behav Med' [JO]*) AND (*'Randomised Controlled Trial' [PT] AND humans [MeSH Terms] AND English [LA] AND 2003:2012 [DP] AND random\* [TIAB]*). This resulted in 624 hits of studies, which received an identification number.

### *Selection of studies*

Studies were randomly assigned to one of the investigators, using a screening list containing identification numbers, who examined whether they met the inclusion criteria. In particular, the study had to be the primary publication of an RCT with an identifiable intervention and control/comparison condition. The primary outcome of the trial had to be behaviour change (and not only changes in determinants of behaviour such as attitude or self-efficacy) or biomedical measures indicating this (e.g. changes in HbA1C resulting from increased physical activity). Furthermore, the trial had to contain human participants and one or multiple follow-up measurements whose availability was participant dependent. For example, a trial in which health care utilisation was automatically assessed by patient records was excluded because of the limited risk of loss to follow-up. If in doubt, inclusion of the study was discussed with the rest of the research team until a unanimous decision was reached. If a study did not meet the inclusion criteria, a new study was randomly selected based on the screening list. This process was continued until the desired number of studies was selected (i.e. approximately 10% of the studies on the screening list). Accordingly, we continued the process until 60 studies were selected (Figure 1).

### *Analysis of studies*

Each included study was independently scored on the items shown in Table 1 by two of the four investigators in the research team (the one who initially examined the article for suitability and one additional randomly chosen investigator). So, each investigator scored 30 studies. Besides descriptive characteristics, these items concerned study characteristics potentially related to differential attrition (e.g. intensity of treatment per condition, type of control; see Table 1). These items were selected based on a previous study concerning differential attrition (Crutzen et al., 2013) and their face validity in

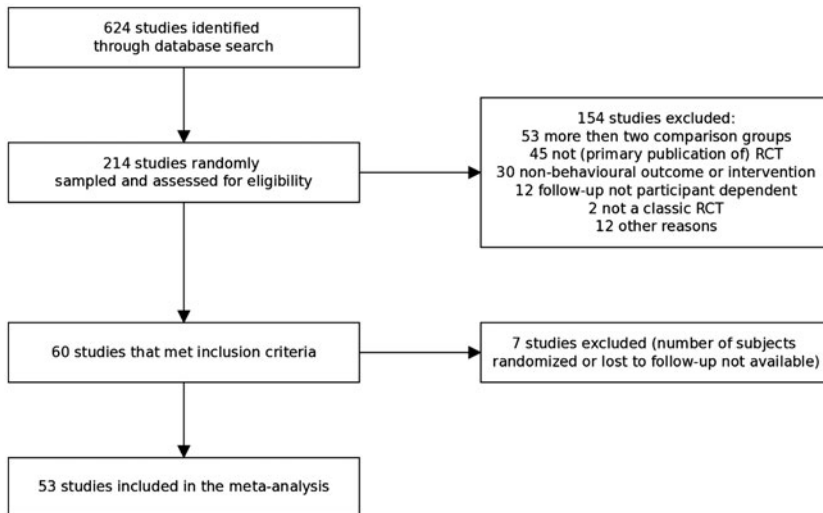


Figure 1. Flowchart of study selection.

line with the study aim, determined upon by all investigators prior to starting data collection. Concordance between scorers was high (e.g. no difference or just a 1-point difference on a 10-point scale). Disagreements between the two scorers were resolved by discussion. The intensity of the intervention condition, the control condition and follow-up procedure was scored on a scale from 1 (lowest intensity; e.g. only an information leaflet or single questionnaire) to 10 (highest intensity; e.g. long-term diary registration and high-frequency counselling sessions). Human contact in delivery of the intervention and control condition was classified as ‘no/low’, ‘intermediate’ or ‘high’. Human contact was added in this study because of its possible relationship with attrition (David, Alati, Ware, & Kinner, 2013). The number of participants that remained in the study in the intervention condition and control condition was determined based on the primary outcome indicated by the authors. When multiple follow-up measurements were taken, we recorded the number that remained at the last time point that contributed to the primary analysis (e.g. at 12 months, for a study with measurements at baseline and at 3, 6, 9, 12 and 24 months post-treatment, where the primary analysis is a repeated-measures analysis for the data up to month 12). If insufficient information was reported to reconstruct these counts (e.g. only the total number that remained was given), we contacted study authors in an attempt to obtain this information. For each study, we therefore attempted to obtain a  $2 \times 2$  table with the number of participants that were lost to follow-up and the number that remained in the study in the intervention condition and control condition.

### **Statistical analysis**

Based on the  $2 \times 2$  tables, we calculated the proportion of participants that were lost to follow-up after randomisation in the intervention and control condition ( $p_I$  and  $p_C$ , respectively) in each study. The outcome of interest for the meta-analysis was the

Table 1. Characteristics of selected studies ( $N = 60$ ).

	Total	Intervention	Control	Test for difference
Human contact in delivery, % ( $N$ )				McNemar's test (3, $N = 60$ ) = 27.00, $p < .001$
No/low		23.3 (14)	53.3 (32)	
Intermediate		26.7 (16)	31.7 (19)	
High		50.0 (30)	15.0 (9)	
Intensity (range: 1–10, $N = 59^*$ ), $M$ (SD)		4.5 (2.3)	2.4 (1.9)	$t(58) = 7.62$ , $p < .001$
Type of control condition, % ( $N$ )				
Waiting list/nothing	20.0 (12)			
Usual care	26.7 (16)			
Alternative intervention	51.7 (31)			
Placebo/sham	1.7 (1)			
Follow-up intensity (range: 1–10, $N = 60$ ), $M$ (SD)	2.5 (1.0)			
Follow-up duration (in months, $N = 60$ ), $M$ (SD)	8.1 (8.9)			
Participants randomised ( $N = 57^*$ ), median (25th–75th percentile)		72.0 (40.0–154.5)	66.0 (37.5–135.0)	
Participants retained ( $N = 54^*$ ), median (25th–75th percentile)		64.5 (34.8–138.5)	57.5 (32.0–130.5)	
Participants lost to follow-up ( $N = 53^*$ ), median (25th–75th percentile)		12.0 (4.0–30.0)	9.0 (4.0–26.0)	

Note:  $M$  = Mean, SD = Standard deviation.

\*Number of studies deviates from  $N = 60$  because of missing data.

relative attrition rate (i.e.  $p_I/p_C$ ), where a value  $>1$  indicates a higher attrition rate in the intervention condition and a value  $<1$  indicates a higher attrition rate in the control condition. As is typically done for rate ratios (Fleiss & Berlin, 2009), the actual analysis was carried out with the log-transformed relative rates (i.e.  $\log(p_I/p_C)$ ), as this yields values that are symmetric around 0 and whose sampling distribution is better approximated by a normal distribution. Studies with no loss to follow-up in either the intervention or the control condition were handled by adding  $\frac{1}{2}$  to each cell in the  $2 \times 2$  table, which not only makes the computation of the log relative attrition rate possible, but more generally acts as a bias correction (Higgins & Green, 2008). Studies with no loss to follow-up in both conditions were excluded from the analysis (Higgins & Green, 2008). The sampling variance of the log-transformed relative rates was estimated with  $\text{Var}[\log(p_I/p_C)] = (1 - p_I)/(n_I p_I) + (1 - p_C)/(n_C p_C)$ , where  $n_I$  and  $n_C$  are the number of individuals randomised to the intervention and control condition, respectively (Fleiss & Berlin, 2009).

Using a random-effects model with restricted maximum-likelihood (REML) estimation for the amount of heterogeneity, we then meta-analysed the log-transformed relative attrition rates (Raudenbush, 2009). We report the (back-transformed) estimated average relative attrition rate and corresponding 95% confidence interval, in addition to the results from the  $Q$ -test for heterogeneity and the  $I^2$  statistic. The funnel plot was visually examined for asymmetry (as a potential indicator of publication bias), by means of the rank correlation and regression test (Sterne & Egger, 2005), and via the trim and fill method (Duval & Tweedie, 2000a, 2000b). We present a so-called contour-enhanced funnel plot, which more clearly indicates suppression of non-significant values (Peters, Sutton, Jones, Abrams, & Rushton, 2008).

Next, meta-regression analyses using mixed-effects models and REML estimation (Raudenbush, 2009) were carried out to examine the relationship between the (log-transformed) relative attrition rates and various study characteristics that may be related to the degree of differential attrition: the amount of human contact in delivery and the intensity of the intervention/control condition, the type of control condition, and the follow-up intensity and duration. Variables coded separately for each condition (i.e. the amount of human contact in delivery and the intensity) were included in the model as the difference between the intervention and control condition (with the amount of human contact initially coded as 0, 1 or 2 for no/low, intermediate and high amounts in each condition). Type of control condition was examined as a (dummy-coded) factor (with 'waiting list/nothing' as the reference level). The various variables were examined univariately, jointly in a single meta-regression model, and via backwards elimination. We report the regression coefficients from the models with corresponding standard errors,  $z$ -values and  $p$ -values. In addition, we report the estimated (average) relative attrition rates (with 95% confidence intervals) based on the models for each level of the categorical moderator (type of control condition) and for representative values of the quantitative moderators. For all models, we also checked the data for outliers and influential cases using residuals and various case and deletion diagnostics (Viechtbauer & Cheung, 2010). All analyses were conducted in R (R Development Core Team, 2010) using the *metafor* package (Viechtbauer, 2010).

## Results

Of the 624 studies that were obtained from the PubMed search, a total of 214 studies had to be screened to identify 60 eligible studies (28%; Figure 1). Thus, a total of 154 studies were not eligible and excluded: 53 (34%) because there were more than two comparison conditions, 45 (29%) because they were not (a primary publication of) an RCT, 30 (19%) because they contained a non-behavioural outcome or intervention, 12 (8%) because follow-up was not participant-dependent, 2 (1%) because they were not a classic RCT, and 12 (8%) for other reasons.

Thirteen studies (22%) did not provide sufficient information to reconstruct the number of participants randomised and/or retained within each condition. Contacting the corresponding authors of these studies resulted in the requested information for six of these 13 studies (46%). Hence, seven studies (54%) had to be excluded because of insufficient information about the outcome of interest (Figure 1). In the remaining 53 studies, the attrition rates ranged from 0 to .62 (0–62%) both in the intervention and in the control conditions. The average attrition rate was .18 (18%;  $SD = .15$ ;  $M = .15$ ) in



the intervention and .17 (17%;  $SD = .13$ ;  $M = .13$ ) in the control conditions. As shown in Figure 2, the distribution of the attrition rates was heavily right-skewed in both conditions.

In one out of the 53 studies, there was no loss to follow-up in both the intervention and the control condition. This study was therefore removed from the analysis (a sensitivity analysis including this study led to unchanged conclusions). In the remaining 52 studies, the relative attrition rates ranged from .14 to 4.80, with the distribution centred visibly at a relative rate of 1 (see Figure 3). However, the distribution was not entirely balanced. In 30 out of 51 cases (59%), the attrition rate was larger in the intervention condition (and one study had a relative attrition rate of exactly equal to 1).

In four out of the 52 studies (8%), the relative attrition rate differed significantly from 1 at  $\alpha = .05$  (two-sided), which is quite close to the number of significant findings one would expect to obtain if the true relative attrition rate was equal to 1 in all of the studies. However, the average relative attrition rate as estimated based on the random-effects model was 1.10 (95% CI: 1.01–1.20), which was just significantly different from 1 ( $p = .02$ ) and implies a 10% higher attrition rate on average in the intervention conditions. The data also suggest the presence of a small ( $I^2 = 29.3\%$ ) albeit just non-significant ( $Q(df = 51) = 66.66$ ,  $p = .07$ ) amount of heterogeneity.

The funnel plot for the 52 studies included in the analysis is shown in Figure 4. Neither the regression test ( $p = .37$ ) nor the rank correlation test ( $p = .29$ ) indicated asymmetry in the plot. The trim and fill method suggested three potentially missing studies on the lower right-hand side of the funnel plot (which are small studies with higher attrition in the intervention condition), but their imputation did not lead to any substantially changed results (i.e. an estimated average relative attrition rate of 1.11 with 95% CI: 1.02–1.21).

An examination of the studentised residuals, Cook's distances and weights suggested the presence of five potential outliers or overly influential studies. Removing those studies as part of a sensitivity analysis yielded an essentially unchanged estimated average relative attrition rate of 1.11 (95% CI: 1.02–1.22), but a notable reduction in heterogeneity ( $I^2 = 0\%$ ,  $Q(df = 46) = 36.59$ ,  $p = .84$ ).

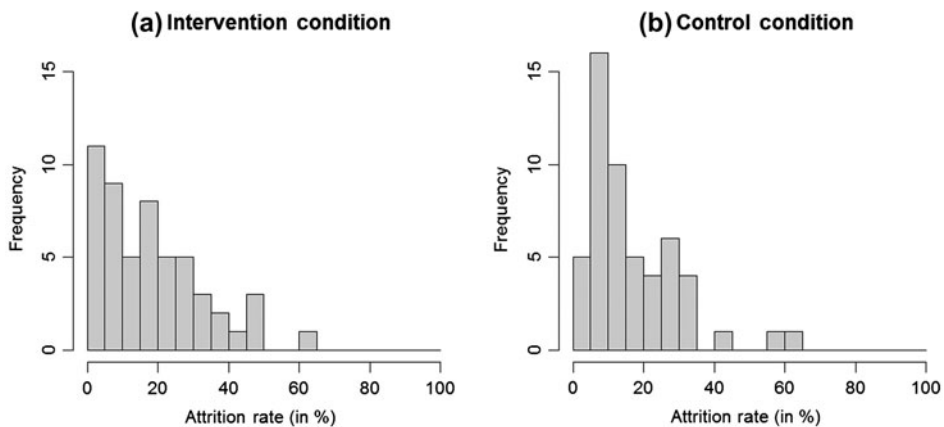


Figure 2. Distribution of the attrition rates in the intervention and control conditions.

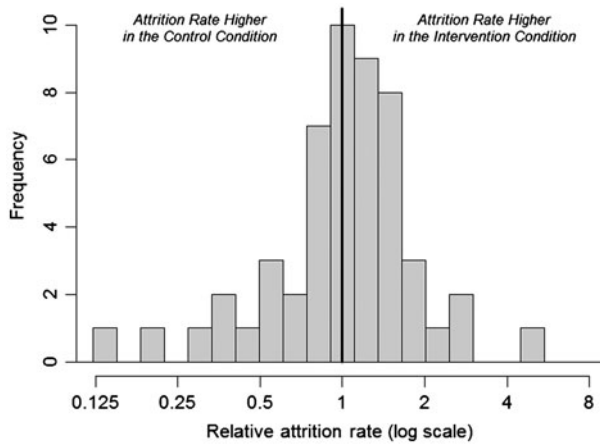


Figure 3. Distribution of the relative attrition rates.

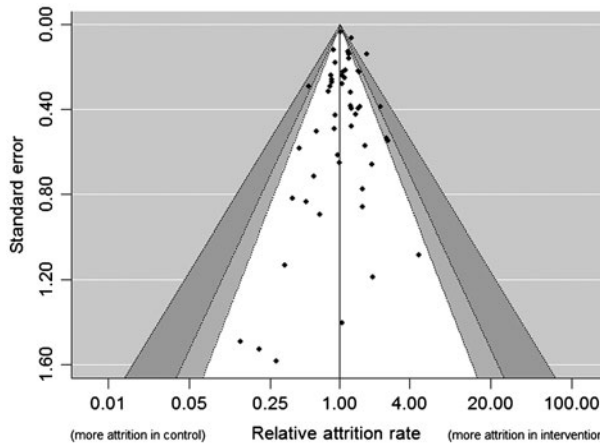


Figure 4. Contour-enhanced funnel plot of the relative attrition rates (>1 indicates higher attrition rate in intervention condition). Note: white area:  $p \geq .1$ ; light grey area:  $.05 \leq p < .1$ ; dark grey area:  $.01 \leq p < .05$ ; area outside funnel:  $p < .01$ .

None of the potential moderators examined in the meta-regression analyses was found to be related to the relative attrition rates (see Table 2 for univariate analysis results). For the type of control condition, all pairwise comparisons between the four levels were also not significant (all pairwise  $p > .36$ ). The meta-regression model including all variables simultaneously (or using backward elimination starting with this model) did not lead to altered conclusions, suggesting that none of the potential moderators was related to the relative attrition rates in a significant manner. Again, removal of the potential outliers of overly influential studies identified earlier did not lead to any changed conclusions with respect to the moderator analyses.

Table 2. Meta-regression analysis results on the relationship between the (log) relative attrition rate and a number of potential moderators ( $N = 52$ ).

	Estimate (95% CI) <sup>a</sup>	Coefficient	SE	<i>z</i>	<i>p</i>
Difference in amount of human contact in delivery		.020	.062	.32	.75
At 0 (minimum value)	1.09 (.98–1.21)				
At 1 (3rd quartile value)	1.11 (1.00–1.24)				
At 2 (maximum value)	1.14 (.93–1.39)				
Difference in intensity of the intervention and control condition ( $N = 51^*$ )		.018	.023	.80	.42
At 0 (minimum value)	1.08 (.95–1.22)				
At 1 (median value)	1.10 (1.00–1.21)				
At 3 (3rd quartile value)	1.14 (1.03–1.26)				
At 7 (maximum value)	1.23 (.97–1.56)				
Type of control condition ( $Q_M(df = 3) = 1.27, p = .74$ )					
Waiting list/nothing (reference level)	1.05 (.81–1.35)				
Usual care	1.07 (.87–1.30)	.019	.166	.11	.91
Alternative intervention	1.13 (1.02–1.26)	.081	.142	.57	.57
Placebo/sham	.86 (.48–1.54)	-.198	.326	-.61	.54
Follow-up intensity		-.021	.046	-.46	.64
At 1 (minimum value)	1.14 (.98–1.32)				
At 2 (median value)	1.11 (1.01–1.22)				
At 3 (3rd quartile value)	1.09 (.98–1.21)				
At 5 (maximum value)	1.04 (.81–1.34)				
Follow-up duration		.003	.005	.56	.58
At .75 (minimum value)	1.07 (.95–1.22)				
At 6 (median value)	1.09 (.99–1.20)				
At 12 (3rd quartile value)	1.11 (1.02–1.21)				
At 54 (maximum value)	1.24 (.82–1.87)				

<sup>a</sup>Estimated (average) relative attrition rates based on the meta-regression models (with 95% CIs) for all levels of the categorical moderator (type of control condition) and for representative values of the quantitative moderators.

\*Number of studies deviates from  $N = 52$  because of missing data regarding the intensity variable in one study.

## Discussion

The findings of this meta-analysis demonstrate a slightly higher amount of attrition in the intervention conditions of HBC trials than in their control conditions. That is, the estimated relative attrition rate of 1.10 indicates that on average, the amount of attrition in the intervention conditions was 10% higher compared to the control conditions. There was no significant heterogeneity, which strengthens our confidence to draw general conclusions about the degree of differential attrition in HBC trials. This suggests that the true relative attrition rate is likely to be a fairly fixed constant (i.e. differences being mostly the result of sampling error). In a previous meta-analysis of a random sample of RCTs published in general medical journals, we observed no evidence that differential attrition generally occurred (Crutzen et al., 2013). The ratio of studies in which the relative attrition rate differed significantly from 1, however, was comparable, namely 7.3% (7/96) in the previous study and 7.5% (4/53) in the study at hand.

As a *post-hoc* analysis, we closely inspected the four studies in which the relative attrition rate differed significantly from 1 to identify possible factors related to differential attrition. In three of these studies, the attrition was higher in the intervention condition. Two of these studies tested tailored interventions stressing the individual responsibility of behaviour change (e.g. smoking) by personal goal setting and barrier identification or providing feedback on participants' use of change processes, while the control condition only received the outcomes measures (Glasgow et al., 2008; Prochaska et al., 2004). Participants might feel that they are held responsible for their own behaviour or consequences. If, at the same time, they feel that the intervention is not helping them to change their behaviour, they might decide not to complete follow-up measurements. This is related to the speculative explanation provided in the next paragraph. The third study regarding the effects of a Mediterranean lifestyle intervention conducted an attrition analysis, but did not find any interactions between treatment condition and any of the baseline characteristics: age, weight, waist-to-hip ratio, age diagnosed with diabetes, years taking diabetes medications, years diagnosed with diabetes, smoking, type of medication, income, educational level, living arrangement, ethnicity and co-morbidities (Toobert, Strycker, Glasgow, Barrera, & Angell, 2005). In the one study in which attrition was higher in the control condition (Blom et al., 2009), the intensity of the control condition could not be determined (a common problem, as described by Ayling, Brierley, Johnson, Heller, & Eiser, 2015), while the intensity of the intervention condition was relatively high (i.e. a stress management intervention consisting of 20 2-h sessions).

A speculative explanation for the slightly higher amount of attrition in the intervention conditions could be that participants in HBC-trials are not blinded for the treatment. Participants are likely to be aware that they are assigned to the intervention condition and might therefore have higher expectations in terms of perceived treatment efficacy. If these expectations are not met, then participants in the intervention conditions might be less likely to complete follow-up measurements in comparison with participants in the control conditions who might have lower expectations to begin with (e.g. because they are on a waiting list). An intuitive way to test this hypothesis would be to link differential attrition to effect sizes regarding primary outcomes of studies. This was not possible in the current study, because it would be difficult to get a comparable effect size across all studies. Furthermore, the studies concern completely different types of interventions aimed at different behaviours. Effect sizes will differ in size simply because of that. Future work could conduct parallel analyses within particular HBC domains (e.g. smoking cessation, HIV prevention) to control for this and to explore moderators of differential attrition within these domains.

Another explanation could be that HBC interventions can be cumbersome and are often preventive in nature. These interventions might target health risk behaviours that participants do not perceive as being highly problematic, whereas pharmaceutical/medical trials are often curative in nature. Therefore, participants in HBC interventions might be more inclined to stop using the intervention and to refuse follow-up measurements. In other words, the costs of participating in the intervention study do not outweigh the benefits.

The potential moderators tested in this study were not related to the amount of differential attrition. Although these moderators already covered a lot of possible differences between trials (e.g. intensity of treatment, amount of human contact, follow-up duration), it might be that there are other variables that contribute to explaining differential attrition. A possible way for future research to explore such variables would be

to conduct a case-control study in which HBC trials in which the relative attrition rate differed significantly from 1 are matched with comparable trials in terms of behaviour, target group and the moderators included in the study at hand. Subsequently, inspection of these trials in pairs could lead to hypotheses regarding variables that contribute to explaining differential attrition, which could be tested in future meta-analyses. Moreover, including even more trials (e.g. from a larger set of journals) could provide the opportunity to investigate moderators that have not yet been explored.

Furthermore, our analyses concerned relationships between trial-levels factors and differential attrition rather than individual-level factors. As suggested before, future research could systematically study attrition at the individual level with methods that permit detecting biases that a trial-level analysis of studies cannot detect (Noguchi et al., 2007). Such studies would require other methods as well as access to individual-level trial data. We would like to stress, however, that we were ultimately interested in the relative attrition rate. The link to the potentially resultant bias is typically much more direct and plausible for differential attrition in comparison with absolute attrition. Specifically, if there is differential attrition, then a non-random missingness mechanism that operates in the same way for both groups (e.g. people who do poorly have an elevated chance of dropping out, regardless of which condition they are in) will lead to bias.

Another valuable finding of the study at hand is the average overall attrition of 17–18%. This number can be used as a rule of thumb in sample size calculations of future HBC trials. It also shows that HBC trials have a higher attrition in comparison with RCTs published in general medical journals, which had an average overall attrition of 13% (Crutzen et al., 2013). This is in line with one of the main reasons to conduct this study, namely that HBC trials are usually at a higher risk of bias than pharmaceutical/medical trials (Crocetti et al., 2010).

The results of this study are limited to HBC trials published in 10 major health psychology/behavioural medicine journals over the last decade. It is possible that editors and reviewers at major journals are more likely to notice differential attrition as a threat to validity and reject such studies for publication. Similarly, authors may well decide not to submit studies with significant differential attrition to major journals, fearing it is only a waste of time. A strength of the present study, however, is the systematic way in which trials were randomly sampled and independently scored by two investigators. This is in fact one of the rare cases where a meta-analysis can be said with confidence to provide a representative sample of studies because there is a clearly defined population of studies from which a random sample of studies was taken. Our population consisted of 624 studies, of which 214 had to be sampled to result in 53 eligible studies. If we extrapolate this rate, then this would result in  $(53/214) \times 624 = 155$  eligible HBC trials in our population. This would imply that we have included approximately 34% (53/155) of the eligible trials in our meta-analysis. So, our key finding is likely to persist: a slightly higher amount of attrition in the intervention conditions of HBC trials.

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