It must be my favorite brand: Using retroactive brand replacements in doctored photographs to influence brand preferences

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Abstract

We examined whether memories for personally chosen brands could be altered by retroactive exposure to less liked competitor brands embedded in manipulated photographs. In addition, we investigated whether memory errors would lead to preference change for falsely remembered brands. Fifty participants were asked to compile their personal 'brand lifestyle basket' which was then captured in a photo showing the basket and participant. After one week, participants were exposed to the photograph in which some of the originally chosen brands were replaced by different brands of the same category. Results of a memory test revealed a robust misinformation effect. The analysis of pre- and post-manipulation preference ratings indicated a positive shift in attitude and behavior toward falsely accepted misinformation brands. Our findings contribute to what we know about the behavioral consequences of false memories and extend the generalizability of false memory effects to what might be considered a futuristic advertising measure.

Keywords: consequences false memories, misinformation effects, advertising, retroactive placements, preference changes

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Aza Raskin, the former creative lead of Firefox, predicted that marketers might start to apply a new form of brand communication: Social network platforms could be used to replace existing brands in personal photos uploaded by their users. This retroactive product placement might create false memories for brand preferences (Raskin, 2010). Because in marketing, consumers' individual experiences are considered to be key for future purchasing behavior, in one form or another such a technique might find its way into the marketer's toolbox. But could advertisers really use this method to direct how past experiences with a brand are remembered and even be able to go so far as to change a brand preference?

Support for the effectiveness of retroactive product placement comes from more than four decades of extensive false memory research in which thousands of studies have provided evidence that individuals create false recollections of events that never actually occurred (see Gallo, 2010). By using a variety of false memory paradigms, researchers have repeatedly and reliably triggered spontaneously generated false memories for non-presented materials (e.g., the Deese/Roediger–McDermott paradigm [DRM; Roediger & McDermott, 1995]), implanted rich false memories for entire events (e.g., Bernstein, Scoboria, & Arnold, 2015), or implanted explicitly suggested false event details (e.g., the misinformation paradigm; Loftus, 2005). Indeed, we have already seen these effects in the world of consumer behavior and advertising (e.g., Braun-LaTour, LaTour, Pickrell, & Loftus, 2004; Braun, Ellis, & Loftus, 2002; Sherman, Follows, Mushore, Hampson-Jones, & Wright-Bevans, 2014).

But what about the consequences of false memories? Much has been learned about the false memory phenomenon over its' history, but fewer studies have considered it's 'after effects' (Laney, Morris, Bernstein, Wakefield, & Loftus, 2008). This question is of particular

interest for applied areas such as advertising. A few studies have shown that false memories for attributes of experienced brands can be linked to consumer judgments and choice. For example, by using suggestive advertisement, Braun and Loftus (1998) misled participants on the color of a previously seen chocolate bar wrapper, a finding the researchers called the 'advertising misinformation effect'. When the misleading color information was linked to a positive message (e.g., greater safety), misled participants expressed more favorable feelings towards the brand and more willingness to buy the product relative to non-misled participants (see also Braun-LaTour, LaTour, & Loftus, 2006; Braun, 1999, for similar findings in different advertising contexts).

More recently, several researchers found that implanted autobiographical memories could have attitudinal and behavioral effects downstream. Rajagopal and Montgomery (2011) demonstrated this effect using high imagery advertisements for a fictitious brand that made some participants falsely believe that this product had been experienced in the past. Akin to brands that were actually experienced in the study, this 'false experience effect' led to more favorable feelings for the fictitious product. Other researchers tracked participants' preference changes from pre- to post-manipulation. For example, Laney et al. (2008) implanted the memory that participants had a positive food experience in their childhood. Relative to a control group, misled participants increased their general liking and willingness to pay more for the food from pre- to post-manipulation (see also Berkowitz, Laney, Morris, & Loftus, 2008).

Returning to retroactive product placement, the question we are interested in is what would happen if participants were led to falsely believe that a suggested and less preferred competitor brand was experienced instead of an actual preferred brand? Would participants who form false memories increase their favorable ratings for these falsely remembered brands? Although previous studies might suggest that the answers to these questions are yes,

exploring the consequences of retroactive brand replacement differs somewhat from past research. As demonstrated, previous studies either (1) focused on false memories for misleading brand attributes and the consequences for the original brand experience or (2) looked at implanted false memories for the experience of a fictitious brand. In contrast, our study explores the consequences of false memories for misleading and competitive brands that directly challenge a consumer's original brand choice. A positive finding would extend previous research to a personalized 'brand misinformation effect' that might reflect the competitive environment in supermarket shelves as well as participants real brand choice behavior. To clearly address our questions, we tracked preference changes for brands from pre- to post-manipulation.

To test for the potential effects of retroactive product replacement we used the misinformation paradigm with its standard three stages: experience an event, present misinformation about the event, and subsequently elicit memories for the event. By using this technique in one form or another, researchers have repeatedly shown that misinformation can be falsely remembered as being part of the original event (see Loftus, 2005, for a review). The nature of this phenomenon has been explained by various theories (see Steffens & Mecklenbräuker, 2007, for a review). One frequently used theory, source-monitoring, proposes that misleading postevent information is erroneously attributed to the original event because individuals can confuse the sources of information. These source misattributions arise because memory for event information and memory for the sources of information constitute two separate cognitive processes. Indeed, it is well known that information about an event can be better remembered than the information about its source (Johnson, Hashtroudi, & Lindsay, 1993). Hereby, source confusions can originate from the effects of the misleading information on the original memories (e.g., when the original memory trace is completely or partially overwritten by postevent information [memory impairment]) or when

the newly learned information is preferred at output (i.e., retroactive interference; Loftus & Hoffman, 1989).

Research has already shown that manipulated images can be an effective medium to implant misleading information (e.g., Nash & Wade, 2008; Wade, Garry, Read, & Lindsay, 2002). Although this medium has been less well used in the area of advertising, Hellenthal, Knott, and Howe (2015) successfully misled brand information via doctored Facebook photographs. Participants reliably, but falsely, attributed competitor (misinformation) brands from doctored photos to the original event (a previous exposure to these Facebook photos). However, participants neither appeared in these pictures nor were brands in the photos personally chosen or experienced. One question that naturally arises then is how will these effects develop under more personalized circumstances?

With these ideas in mind the following experiment examined whether retroactively 'less-liked' competitor brands in doctored photographs can change memories for personally chosen brands. In addition, we examined whether false memories might lead to attitudinal and behavioral consequences for falsely remembered brands. We invited participants to the lab under the auspices of examining their personal brand lifestyle. Participants constructed a 'brand profile basket' that was then captured in a photo showing participants and their 'shopping' basket. After a delay, participants were exposed to a doctored photograph, in which some of the self-chosen brands were replaced by their 'less-liked' competitor brands. Memory tests for the original event as well as pre- and post-manipulation brand preference ratings were used to examine the uptake of misleading information as well as potential preference changes for falsely remembered brands. In line with previous research, we expected that misinformation would interfere with participants' original memories and consequently lead to the false acceptance of misinformation items. In addition, we predicted that participants would rely on their false memories in the post-manipulation preference

rating task and rate falsely remembered misinformation brands more favorably from pre- to post-test.

Method

Participants

Fifty university students and staff (mean age = 23.54 years, *SD* = 7.03; 26% male) took individually part in this experiment over three sessions for either course credit or remuneration.

Stimuli and Design

Figure 1 shows the design of this study. Stimulus selection for pre- and post-manipulation brand rating task, brand compiling task, as well as memory tests was based on a norming task used by Hellenthal et al. (2015) in which familiarity of brands belonging to 24 different product categories was recorded. Of these, 12 product groups belonging to the food category were chosen and the six most popular brands per category were selected. Of these, three brands per category were used as actual study stimuli and the remaining three brands were used as filler items in the memory and brand rating tasks. In order to limit the influence of characteristic product packaging on memory performance, packaging of categorically related brands was kept the same (e.g., Coca Cola, Pepsi, Dr Pepper, Fanta, Sprite, and Tango were represented as a 500ml bottle).

[INSERT FIGURE 1 ABOUT HERE]

Brands provided for creating the brand basket (offered items) were the three most popular brands of each of the 12 brand categories. Of these, participants chose one brand per category for basket inclusion so that each personal brand basket contained 12 products in total. Participants were misled on four of these originally chosen items in the manipulated photograph (misled condition). More specifically, misinformation was presented for four

brand categories using a competitor brand that had not been picked from the offered items in the construction of the personalized brand basket (out of two remaining brands of a category). Details of the remaining eight brands remained consistent in the photograph (control condition). Unused offered basket items served as foil items in the final memory test.

Assignment of the brands to the three item types (original item, misinformation item, and foil item) and to the condition (misled and control) was counterbalanced across participants as best as possible. However, given that we used a personal preference task, counterbalancing was partially out of our hands. Nevertheless, we did our best to assure that: (1) originally chosen brands appeared in the misled and in the control condition in a balanced manner and (2) brands appeared about the same number of times as a misinformation or a foil item. Furthermore, misinformation selection was subject to the constraint that the item's preference lay somewhere between the most (the brand that was originally chosen for the basket) and the least preferred brand of the three offered basket items. Thus, a misinformation brand was always less liked than its original counterpart but it was not the least liked brand, given that the least preferred brand of a category might have been too distinctive relative to the originally chosen item.

Finally, we also included some measures that were aimed at reducing the risk of the influence of any confounding variables. Specifically, we made sure that the misinformation categories chosen for each participant varied in overall favorability for a brand category as well as in how often brands of the categories were purchased by a participant in general. These measures were included to avoid behaviors driven by the prominence of certain types of misinformation categories (e.g., high false memory rates driven by categories much liked or frequently purchased).

The memory test as well as the pre- and post-manipulation preference-rating tasks contained all 72 brands that were selected from the norming study. These included all 36

offered brand items classified in three item types of interest: original items, misinformation items, and foil items, as well as the 36 categorically-related filler items.

Procedure

Session 1. On arrival in the lab on Day 1, we informed participants that the purpose of the study was to create different brand lifestyle profiles as part of a wider consumer research study. They were also told that their profile would include a picture showing them and a personal brand profile basket that they would compile during the session. Participants first completed the brand preference questionnaire and evaluated 72 brands on two 10 point Likert-type scales. Based on their personal and current attitudes, participants rated the brands for (1) how much they liked these brands (1 = Absolutely hate the brand - 10 = Absolutely love the brand) and (2) the likelihood of buying these brands (1 = Absolutely not - 10 = Absolutely yes). In order to measure participants' experience with the different product categories, participants also rated how often products from these 12 categories were purchased.

Next, participants were shown the brands for the basket-compiling task. They were told that they would now get the chance to compile their personal brand lifestyle basket by choosing one brand per brand category that they preferred the most and to hand it to the experimenter. In total, 12 brands were chosen and were placed into the brand basket by the researcher. In order to ensure encoding of the selected brands, participants were asked to specify in written form approximately how much they had paid for each individual product in the past. After all the brands had been selected and placed in their basket, a photograph was taken showing the participant and basket.

Session 2. On Day 8, participants returned to the lab for a short session and were exposed to their doctored brand profile picture that included the misleading information. They were asked to confirm whether they were comfortable with this picture being included

into their brand profile. In order to ensure that misinformation was processed, participants completed a short questionnaire about the brands in which they indicated for each of the specific brands the maximum price that they would pay for that product in a shop.

Session 3. On Day 9, we invited participants back to the lab under the pretense that some final follow up questionnaires had to be filled out. Instead, participants were confronted with a surprise recognition test for the original event. Under the explicit instruction that the test strictly referred to the original brand-packing task carried out nine days earlier, participants were asked to press the 'yes' key for brands that were included in the original personal brand basket and to press the 'no' key if they were not. Only if 'yes' was clicked were participants asked to make a remember, know (Tulving, 1985), or guess judgment (see also Braun & Loftus, 1998). Participants were asked to press 'remember' when they were able to vividly remember choosing a particular brand for the basket; to press 'know' when the judgment was based more on a feeling of familiarity (i.e., they had the sense that they included the brand into the basket but they could not really remember it); and finally to press 'guess' when they guessed the brand was included but they were not really sure.

Following the recognition test, a final source-monitoring task was carried out to measure what is referred to as participants' robust memory performance. Five options were provided for each of the items endorsed in the recognition test: (1) included in brand basket on Day 1 only, (2) seen in the photo on Day 8 only, (3) included in brand basket and seen in photo, (4) item differed between the actual basket and the photograph, (5) just guessed. We used this procedure in order to further affirm whether participants believed that they had chosen the misleading detail themselves during the original event (see Zhu et al., 2012). We chose this practice over an explicit 'misinformation-warning' procedure because we did not want bias participants' behavior for the post-manipulation preference rating task that would

follow. This last preference rating task was completed after 10 – 15 minutes of filler activity (short working memory tasks) and was the same task as used on Day 1.

Results

Misinformation effect data

To address our first question concerning whether participants would create false memories for misinformation items, we analyzed their overall memory performance on the recognition test. Table 1 shows the proportion of yes-responses for the three item types (original details, misinformation details, and foil details) correctly and incorrectly accepted as being part of the original event as a function of condition (i.e., whether participants were or were not misled on that item).[1]

[INSERT TABLE 1 ABOUT HERE]

To see whether participants were misinformed by the retroactively inserted brands in the photos, we first compared their overall endorsement rates of original items, misinformation items, as well as foil items in the misled condition. A one-way repeated measures ANOVA revealed a significant main effect of item type, $F(2, 98) = 51.57$, $p < .001$, $\eta_p^2 = .51$. Bonfferoni pairwise comparisons showed that participants falsely accepted more misinformation items ($M = .69$, $SD = .33$) than they correctly accepted original items ($M = .30$, $SD = .32$, $p < .001$). Results also showed that both the misinformation and the original items were more often accepted than were the related foil items ($M = .05$, $SD = .11$, p's $< .001$). Next, we examined participants' responses to the item types across condition (misled vs. control). For original items, analysis showed that participants correctly accepted more original details in the control condition ($M = .95$, $SD = .09$) than in the misled condition, $t(49) = 13.14$, $p < .001$, $d = 2.39$. We also compared misinformation false alarms in the misled

[1] Responses to filler items were not considered in this report in order to reduce the complexity of the analysis. Please note that false alarm rates for fillers ($M = .02$, $SD = .05$) did not statistically differ from that for foils ($M = .05$, $SD = .11$), $t(49) = 1.57$, $p > .05$.

condition with foil false alarms in the control condition. This was done because foil items in the control condition referred to a detail that would have been used to contradict the original item as misinformation in the misled condition and thus, retrieval of these items served as an additional baseline. Here, analysis revealed more misinformation acceptances in the misled than foil false alarms in the control condition (M = .03, SD = .04), $t(49) = 13.75$, $p < .001$, d = 2.66. Hence, our findings suggest that participants were misled by the retroactively changed brands in the photographs.

Most of these effects were confirmed when participants' recollective experiences only (remember responses; see Table 1) and participants' robust memory performances (source-monitoring answers; see Table 2) were considered (all ps < .001). Only one exception was found in the source-monitoring data where the misinformation acceptance rate did not significantly differ from the acceptances of original items in the misled condition (p = .052). Here misinformation endorsement dropped, indicating that participants were able to correct some of their source errors from the recognition test in this task.

[INSERT TABLE 2 ABOUT HERE]

Preference rating data.

Results for the misinformation items. To answer our second question concerning whether false memories would have any consequences for individuals, we examined attitudinal and behavioral changes for falsely remembered misinformation items from pre- to post-manipulation. First, we analyzed participants' mean rating on the liking as well as the likelihood of buying scales for endorsed misinformation items at time 1 and time 2. Concerning the liking scale, as predicted, results of paired samples t-tests showed that participants rated endorsed misinformation items higher at time 2 (M = 6.87, SD = 1.45) than at time 1 (M = 6.26, SD = 1.42), $t(44) = -3.28$, p = .002, d = .49. This trend was also found for the likelihood of buying scale in which endorsed misinformation brands were rated higher at

time 2 (M = 6.75, SD = 1.58) than at time 1 (M = 6.13, SD = 1.45), $t(44) = -2.89$, $p = .006$, $d = .43$.

To create a more comprehensive test, we added a variable where we compared attitude changes between endorsed (falsely accepted) and non-endorsed (correctly rejected) misinformation items. In this way each participant served as their own control. To do this this, we conducted 2(Endorsement: endorsed vs. non-endorsed) x 2(Time: time 1 vs. time 2) repeated measures ANOVAs. For the liking scale the analysis revealed a significant main effect of Endorsement, $F(1, 25) = 5.78$, $p = .024$, $\eta_p^2 = .19$, where participants rated endorsed misinformation items higher than non-endorsed misinformation items. However, there was no significant interaction of Endorsement and Time, $F(1, 25) = 1.65$, $p = .21$, $\eta_p^2 = .06$. Thus, although the data showed a trend in the expected direction (i.e., that there was an increase in liking from time 1 to time 2 for endorsed misinformation items but not for non-endorsed misinformation items), the trend was not statistically significant.[2]

The likelihood of buying scale analysis yielded a significant main effect for Endorsement, $F(1, 25) = 6.56$, $p = .017$, $\eta_p^2 = .21$, as well, showing that participants rated endorsed misinformation items higher than non-endorsed misinformation items. In addition, there was a significant Endorsement X Time interaction, $F(1, 25) = 4.80$, $p = .038$, $\eta_p^2 = .16$. Analyzing the simple main effects with Bonferroni using adjusted alpha levels of .0125 per

[2] A prior power computation with a medium effect size $f = 0.25$ and power = 0.95, suggested that the sample size needed would be N = 36 [G*Power 3.1.7 (Faul, Erdfelder, Lang, & Buchner, 2009)]. Thus, a sample size of 50 was considered adequate for revealing the expected effect. However, since out study manipulation turned out to be very effective, analysis reported was based on only 25 participants who created both false alarms as well as correct rejections for misinformation items (endorsed vs. non-endorsed misinformation items). Forty per cent of our participants falsely endorsed all misinformation items and 10% correctly rejected all misleading items. Hence, these participants were not included into the analysis due to missing data points, which reduced statistical power by 50%. This was supported when we used the Expectation-Maximization (EM) method to impute the missing values for our liking data (SPSS 22.0). When we ran the same two-way ANOVA with the new and complete data set, results revealed not only a significant main effect for Endorsement $F(1, 49) = 11.17$, $p = .002$, $\eta_p^2 = .19$, but also a significant Time and Endorsement interaction, $F(1, 49) = 4.90$, $p = .032$, $\eta_p^2 = .09$.

test (.05/4), confirmed that participants rated endorsed misinformation higher at time 2 than at time 1 (see test statistics for this *t*-test at the beginning of this section) but this was not the case for non-endorsed misinformation items. In addition, they rated endorsed misinformation items (M = 7.10, SD = 1.53) higher at time 2 than non-endorsed items (M = 5.57, SD = 1.92), $t(25) = 3.39$, $p = .002$, $d = .67$, whereas no difference was found at time 1.

Comparison across item types. We also conducted an analysis that would compare preference changes over time between endorsed misinformation items and other endorsed item types (original items and the foils). We run 2(Endorsed Item Type: misinformation item vs. other item type) x 2(Time: time 1 vs. time 2) repeated measures ANOVAs separately for original items and foil items in the misled and control conditions. However, because foil items were rarely endorsed in the misled condition (N = 7), we collapsed the foil responses across conditions (misled vs. control; the preference change trend in both conditions was the same). To reduce complexity, we focused on the matter of interest: the interaction between Endorsed Item type and Time that would show whether or not potential preference change effects were more pronounced for one item type or the other[3].

For the analysis of original items in the misled condition, there was a significant Endorsed Item Type X Time interaction for liking, $F(1, 25) = 4.53$, $p = .043$, $\eta_p^2 = .15$, as well as one for likelihood of buying, $F(1, 25) = 5.55$, $p = .027$, $\eta_p^2 = .18$. Further analysis of the simple main effects using paired sample *t*-tests showed that the preference change effect found for misinformation items (see earlier) was not present for originally chosen items on which participants were misled on (for both scales *p*s $> .05$). Conducting the analysis with original items in the control condition, the Endorsed Item Type X Time interaction did not

[3] Please note that the analysis revealed significant main effects of item type when comparing misinformation items with original items in misled and control condition. Here, results showed that endorsed original items were rated higher on the liking and likelihood of buying scale than misinformation items. Considering how misinformation items were chosen in this study (less-liked competitor brands) this is outcome is not surprising. No main effects were found for the foil items.

turn out to be significant (liking: $F(1, 44) = 3.29$, $p = .070$; likelihood of buying: $F(1, 44) = 3.70$, $p = .061$). Nevertheless, we used the low *p*-values as justification to do a further analysis of the simple main effects. Results showed that only on the liking scale did participants rate endorsed original items higher at time 2 ($M = 7.89$, $SD = 1.29$) than at time 1 ($M = 7.63$, $SD = 1.43$), $t(48) = 2.85$, $p = .006$, $d = .42$. Thus, the preference change trend found for misinformation items was also present for participants' attitudes towards brands that had appeared in a consistent manner in both basket and photo. Last, we found significant Endorsed Item Type X Time delay interactions for the foil items as well, with $F(1, 19) = 11.00$, $p = .004$, $\eta_p^2 = .37$ for liking and $F(1, 19) = 4.23$, $p = .039$ for likelihood of buying. Further analysis showed that the preference change effect for misinformation items was not present for the foil items either (for both scales *p*s $> .05$).

Results for the other item types. For completion, we also conducted separate 2(Endorsement: endorsed vs. non-endorsed) x 2(Time: time 1 vs. time 2) repeated measures ANOVAs for the original and the foil items. The only significant effect we found was for foil items (foils collapsed across condition). Analysis revealed a significant Item Endorsement main effect for liking, $F(1, 22) = 22.65$, $p < .001$, $\eta_p^2 = .51$, as well as for likelihood of buying, $F(1, 22) = 20.31$, $p < .001$, $\eta_p^2 = .48$. That is, endorsed foil items were rated higher than non-endorsed items on both scales. Neither for original items in the misled nor in the control condition were there any significant main effects or interactions (for both scales all *p*s $> .05$).

Discussion

Our results show a reliable and strong misinformation effect caused by retroactive brand replacements in photographs. On the recognition test, 70% of the non-chosen and less-liked misinformation brands were falsely attributed to the original brand-packing event. Of these, the majority of responses were associated with recollective experiences (Remember

judgments), meaning that in about 80% of the cases, participants were able to vividly remember choosing a particular misinformation brand for the brand basket. Our data also suggest that participants' original memories were strongly affected by the study manipulation. Items that were originally chosen for the basket but were later replaced by a competitor in the photo were correctly remembered in only 30% of the cases and only half of these were vividly remembered. In comparison, we found 95% (85% remembered) correct recognition for control brands that were included in the basket and that also appeared in the photo. When we gave participants the chance to reconsider the sources of their memories in the source-monitoring task these trends, even if in weaker form, were still present. What this suggests is that the misleading postevent information in the pictures affected participants' original event memory and as a result, participants formed the false memory that the less-liked competitors were originally included in the basket.

In line with previous research, our data also suggest that false brand memories can lead to attitudinal and behavioral consequences for individuals. Specifically, after participants created false memories for retroactively inserted brands they rated these brands as being more positive on the liking and likelihood of buying scales. Also, misinformation brands that were falsely accepted for basket inclusion were more positively rated on both scales than misinformation brands that were correctly rejected. In addition, we have seen that this preference change effect was more pronounced for misinformation items than for any of the other items types. Indeed, for these other items, the effect was either completely absent or weaker. This positive shift in ratings for misinformation items might be explained by fluency misattribution processes that acted at the time of the post-brand rating. When participants saw the misinformation brands in our post-rating test, product icons might have been processed more fluently because of the previous encounter in the photograph. If this fluency was

interpreted as brand familiarity, this might be what caused the shift in favorable ratings (Laney et al., 2008).

Some might argue that this effect was simply a mere exposure effect rather than being associated with false memories. We argue that this is unlikely due to several factors. First, our analysis was based on comparing endorsed versus non-endorsed misinformation items. A mere exposure effect would have driven a positive shift in attitudes for non-endorsed items as well (all misinformation items appeared equally often and we made sure that all misinformation items were processed during the misinformation phase). Second, the shift in preferences was not found for the other item types to this extent. For example, if we compare original control items (packed in basket and seen in photo) with misinformation items, then we are comparing item types that were seen the same amount of time (in pre-test, on the table, in the photo, in the post-test). In fact, control items may have been processed even more intensively as they were chosen for basket inclusion. However, although we did measure an increase of preference for these control brands as well, the effect was weaker and was only present for the attitudinal measure, not the behavioral scale.

Although it is reasonable to suggest the involvement of demand characteristics, we used a plausible cover story for this study. At the end of the study participants were also asked if they were aware of the purpose of the study. Only six participants were suspicious that something was wrong with the picture. Of these, only three participants were positive that the picture was manipulated. When debriefed on the purpose of our study, most participants appeared surprised. Also, five of our six suspicious participants produced zero misinformation false alarms. If demand characteristics had been the driving force of our effects we would assume high false alarm rates in these cases as well. Last, it should be mentioned that our target items were embedded in a substantial number of filler items which

would have made it hard for participants to apply any kind of strategy (see Laney et al., 2008, for a similar argument on this matter).

It would seem reasonable to conclude that the effects we obtained are largely driven by the fact that participants relied on their false memories when post-rating the brands. Together, these findings contribute to the false memory literature in two ways. First, we extend the applicability of the classical misinformation paradigm by showing reliable misinformation effects in a new and ecologically relevant context – retroactively changed brands in personal photos. Second, following from this, our research builds on previous studies and shows the additional consequences of false memories for highly competitive stimuli that aimed to overwrite or suppress participants' original brand choice. However, these results go beyond showing the after effects of misinformation-based false memories. Interestingly, we did not find any indication that preferences for originally chosen but replaced items changed once they were 'missed' and potentially overwritten by misleading information. Hence, rather than completely restructuring participants' brand attitudes and behavior, these changes only occurred (to this extent) for the misinformation brands (see Braun-LaTour et al., 2006 for a similar finding in a different context).

Our findings have implications for marketers including insights into the effectiveness of a new, albeit futuristic, advertising measure. Traditional advertisements serve to 'remind' consumers to purchase a product again. However, advertisements are often thought of in disdain, designed to manipulate buying habits. As a result, consumers develop advertising avoidance strategies. Retroactive implicit brand placement could lead consumers to reinterpret and reconstruct their past experiences (believed that they consumed one particular brand over the other) (Braun, 1999; Braun & Loftus, 1998; Loftus & Pickrell, 1995). This then becomes part of their own decision-making experiences. Our study provides evidence that brand attitudes and buying behavior might be influenced as a result. But even if the

manipulation of personal photos on social network platforms fails on legal grounds, our findings can be translated into other marketing techniques. For example, in ambush marketing, advertisers try to associate their own brand with a sponsoring event (without paying for official sponsoring rights) at the cost of a true and official sponsor (Cornwell & Humphreys, 2013). If marketers manage to create false positive associations between brands and the sponsored event, attitudes and purchasing behavior for these brands might change (see also Braun-LaTour et al., 2006 for potential areas of application of the advertising misinformation effect and its' consequences). A last point to mention is the importance of protecting consumers from such implicit advertising techniques. This is particularly important for young consumers who create sophisticated 'online personalities' and spend many hours on unregulated internet sites and thus might be particularly vulnerable to these manipulations. Our results will also help policy makers evaluate the effects of deceptive advertising techniques and develop programs for stopping them at an early stage.

Although our study provides evidence to suggest that retroactively replaced brands in photos have the potential to influence one's past experience with a brand, the way in which participants in our study encoded the misinformation stimuli (focusing the attention to each brand by answering a question) does not accurately reflect how peripheral details in photos would be normally processed. We can assume that picture details are more implicitly processed in real life situations. For example obtained Hellenthal et al. (2015) who examined the effects of retroactive brand replacements in doctored Facebook photos lower hit- and false alarm rates in an incidental brand-learning task. Nevertheless, a reliable misinformation effect was achieved in that study. However, since Hellenthal et al. (2015) did not use personalized brand stimuli and photos, results of the studies are only partially comparable. Future research should close this gap and address whether preference change effects would still occur under these circumstances. The next step in this research would be to examine

whether false brand memories affect participants' real purchasing behavior. An increase of preferences on a scale might not necessarily mean that particular brands will actually be purchased. Finally, the constraints that constituted the selection process of misinformation items in our study should be mentioned. Although we do not believe that these constraints were the driving force of the misinformation effect uncovered here, the misinformation selection process might have influenced our results.

Even with these limitations in mind our study has given insight into a creative, albeit controversial, advertising method. Today marketers encounter a fierce advertising environment that demands creative and effective measures that specifically target the potential consumer. This study shows that retroactive product replacement might be a powerful tool in an advertiser's armament.

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Table 1. Mean proportion of 'Yes' responses with proportion of Remember, Know, and Guess responses (SE) for each item type as a function of condition.

Condition	Response type	*Item type*					
		Original items (hits)		*Misinformation items (false alarms)*		*Foil items (false alarms)*	
Mislead	**Total**	**.30**	**(.046)**	**.69**	**(.047)**	**.05**	**(.016)**
	Remember	.17	(.034)	.54	(.046)	.01	(.007)
	Know	.11	(.028)	.13	(.029)	.03	(.012)
	Guess	.02	(.010)	.02	(.010)	.01	(.007)
Control	**Total**	**.95**	**(.013)**	--		**.03**	**(.006)**
	Remember	.80	(.032)	--		.01	(.003)
	Know	.12	(.024)	--		.01	(.003)
	Guess	.02	(.009)	--		.02	(.005)

Table 2. Robust memory performance rates of the source memory task and distribution of responses broken down by response.

	Original item		Misinformation item		Foil item	
	Misled	Control	Misled	Control	Misled	Control
Robust	18%	68%	32%	--	2%	1%
(1) Chosen 1	12%	8%	3%	--	0%	0%
(2) Saw2	1%	10%	28%	--	2%	0%
(3) Both	9%	68%	29%	--	0%	1%
(4) Conflicted	6%	7%	7%	--	3%	0%
(5) Guessed	3%	2%	3%	--	1%	1%

Note. Item types were coded robust when one of the following options were ticked by a participant: Original item: misled (1) or (4), control (3); misinformation item: (1) or (3), foil item: misled and control (1), or (2), or (3).