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Cognitive Psychology

"I Remember Having Chicken Pox at Age 3": How Can Age Manipulations Affect One's Earliest Childhood Memories?

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Adults' reports of earliest autobiographical memories from before the age of 3 are typically scarce. However, recent research suggests that the age range of this childhood amnesia is flexible when participant instructions provide a context in which earlier or later ages of childhood events are plausible (Kingo et al., 2013). This age manipulation may be more or less effective depending on the type of memories, i.e., event memories that describe a specific event, or fragments / snapshots that describe elements of a past event. One earlier study showed that after reading an early age example, the age in event memories was younger than after a late age example, whereas this difference was less pronounced in snapshot memories (Wessel et al., 2019). The present aim was to examine the malleability of the age in earliest childhood memories and replicate this age manipulation by memory type interaction as well as the overall effects of the age manipulation demonstrated earlier (Wessel et al., 2019). Three studies varied the age in example event memories and fragments (early, late, or no age). Overall, the results suggested that age information affects the reported age, but not necessarily more for event memories than for fragments. That is, all present studies failed to replicate the age manipulation by memory type interaction reflecting that relative to the early condition, event memories in the late condition were older than snapshot memories (Wessel et al., 2019). Even though we cannot conclude that a true effect does not exist, the original finding may be taken as reflecting a false positive.

If you ask an adult to retrieve their earliest childhood memory, they will typically report an autobiographical memory of an event that happened when they were between three and four years old. The phenomenon of not being able to remember memories from before the age of three is referred to as 'childhood amnesia' (or infantile amnesia), and has been examined extensively, typically rendering robust and reliable results (Bauer, 2007; K. Nelson & Fivush, 2004). There is debate about the validity of memories of situations from before the age of 2 in adults. According to some authors (Akhtar et al., 2018) such early memories are improbable and result from reconstructive memory processes. Other authors (Bauer et al., 2019) disagree and point to the evidence coming from studies in young children. For example, Bauer & Larkina (2014) found that 5 – 6 year-old children reported less complete narratives but at the same time remembered events from a younger age than 8 - 9 year-old children. Such findings suggest that young children (age 5 - 6) do have access to early memories but lack the narrative skills to generate a complete report that is rehearsed from time to time. Childhood amnesia may start to develop then the moment children lose access to those early memories.

Another possibility is that older children do not forget the memories from a very young age, but that they misdate them such that they appear to refer to a later age. There are some studies (C. Peterson et al., 2011; Wang & Peterson, 2014) suggesting that when children report their earliest memories at multiple time points, the age of the first reported earliest memory may shift upwards at the later assessments. Other indications that the estimated age of earliest childhood memories is malleable comes from studies in adults. For example, coaxing adult participants with visualization and suggestive interviewing resulted in having a majority of them (78%) report memories from before their second birthday after they initially came up with memories from the time they were about 3.7 years of age (Malinoski & Lynn, 1999). Having confederates in an experimental session who produce very early memories (first steps or second birthday party), resulted in the retrieval of earliest memories of events that happened a year earlier compared to participants who were not exposed to the early memories of a confederate (T. Peterson et al., 2009). These results suggest

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that participants are sensitive to social influences when trying to retrieve their earliest memories, either by being encouraged to come up with earlier memories (Malinoski & Lynn, 1999) or by simply listening to or discussing early memories brought up by a confederate (T. Peterson et al., 2009).

The age of earliest childhood memories can be manipulated in other ways that do not reflect social influences but simply contain information prior to the instruction to retrieve the earliest childhood memory. Kingo and colleagues (2013) used warm-up questions with different target ages for high school students retrieving their earliest childhood memories. Warm-up questions using the age of 3 to provide a childhood event memory before retrieving the earliest resulted in earliest childhood memories that applied to younger ages (mean age = 2.4) than warm-up questions using the age of 6 (mean age = 3.16). Another study used childhood age as an anchor to prime participants to retrieve memories from an earlier or later age (Greenberg et al., 2017). Participants had to indicate whether their earliest memory was before or after the age of 6 or 1. The results revealed that in the low anchor condition (anchor was 1), the reported age of the earliest childhood memories was earlier (mean age = 3.6) than in the high anchor condition (anchor was 6, mean age = 4.2).

Why is it possible to manipulate the age of the earliest childhood memories? Anchoring refers to introducing bias in judgments for which people lack relevant information, such as a time period or distance. Age information is typically not stored in memory. Using such information as a reference point in a question, like Greenberg et al did, would invite people to relate their judgment to that reference point. Thus, anchoring would pertain to nothing more than arriving at a number and would not have much to do with memory retrieval. Alternatively, providing age information, such as Kingo et al.'s warm up questions, may also provide a retrieval context that facilitates access to autobiographical memories. Dijkstra & Kaup (2005) demonstrated that young adult and older participants were faster retrieving autobiographical memories after first generating a lifetime period within which the specific memories were retrieved compared to a condition in which no such retrieval context was available. Providing a time-based retrieval context within which earliest childhood memories are generated, may work the same way. Once an early childhood memory has been retrieved, the participant has tapped into a pool of available early childhood memories which in turn may facilitate the retrieval of another memory that may have occurred at an even earlier stage (Kingo et al., 2013). This pool of earliest childhood memories would become available after seeing an example of a typical childhood event or after thinking about one's early childhood. The difference in retrieved memories between very young and older children may be partly due to a different or lack of retrieval context from which they choose their memories (Bauer & Larkina, 2014). It should be noted that typically, the design of studies in this field does not allow conclusions as to whether an anchoring or retrieval context mechanism provides the most likely explanation for the malleability of the reported age of earliest memories.

A different issue regarding the malleability of earliest childhood memories concerns their content. As discussed above, earliest childhood memories narrated by younger children were less detailed, complete (Bauer & Larkina, 2014), coherent, and shorter than memories narrated by older children (Bauer & Larkina, 2019). The same appears to be the case for adults' earliest memories from young ages (Mullen, 1994). Bruce and colleagues (2005) asked participants to retrieve personal event memories (autobiographical memories) and memory fragments (an isolated memory moment having no event context and remembered, perhaps as an image, a behavior or emotion). Personal event memories reflect events that are remembered as a single event retrieved against some background or setting and is reported in the form of a narrative. Memory fragments on the other hand, lack such characteristics. They reflect knowing rather than remembering of an isolated moment in time that contains a detail or visual image of a scene. Bruce et al. (2005) found that age estimates for earliest fragment memories were younger (3.52 years) than those for earliest event memories (4.36 years). This was particularly the case when fragments were described first.

Bruce et al.'s results suggest that studies on earliest childhood memories should take the distinction between event memories and memory fragments into account. If not specified, reported earliest childhood memories may in fact be fragments and originate in a different manner and contain different information than event memories. Wessel et al. (2019, experiment 1) explored the malleability of the estimated age in adults' earliest autobiographical memories for event memories and memory fragments. Participants reported their earliest childhood memories after reading examples of memories referring to either an event early (2 years) or later (6 years) in childhood. The reported age in the early condition was lower than in the late condition. Furthermore, following Bruce et al. (2005), Wessel and colleagues (2019) explored the content of the memories, but could not identify any fragment memories. Instead, they identified 'snapshot memories', i.e., mental pictures without a temporal sequence, but possibly containing some context information (e.g., a shelf with comics on it.). The age in snapshots was significantly younger than the age in event memories. In addition, a condition by memory type interaction suggested that the content manipulation (event vs. snapshot memories) was less effective in the early condition than in the late condition, where event memories were older than snapshot memories.

It should be noted that the event versus snapshot distinction in this earlier study was based on post-hoc coding by the experimenter. Moreover, the analysis revealing the differential effect of the age-manipulation on memory types had not been planned and might reflect a spurious finding (see for example Gelman & Loken, 2014). In line with such an interpretation, the differential effect of the age manipulation on snapshot and event memories was not statistically significant in a follow-up experiment examining the role of self-relevant knowledge in age estimation (Wessel et al., 2019, experiment 2). However, the design of this follow up study differed substantially and that might have resulted in an inconclusive finding. Moreover, a more direct replication study in a community sample (ages between 20 and 59; Klusmann & Wessel, 2019) also failed to replicate the interaction effect. Importantly, both studies did yield an effect of the early age manipulation relative to a no-age control group (but not the late age manipulation in Klusmann & Wessel, 2019). The general problem with non-replications is, however, that statistical non-significance provides insufficient evidence that an effect does not exist. Therefore, in this paper we present the findings of three experiments further examining the replicability of Wessel et al.'s (2019) findings that age estimates of earliest memories are sensitive to age manipulation and that this sensitivity differs for event and fragment/snapshot memories. The designs of the three experiments differed in the following ways.

In experiment 1, we explicitly instructed the participants to retrieve either event memories or fragment/snapshot memories. In addition, they were exposed to examples containing information about either an older age (age 6) or no age. Experiment 2 had a similar set up, but used a younger (age 2) rather than the older age example. Experiment 3 was a more direct replication of Wessel et al. (2019) including an older and younger age condition and the post-hoc categorizing of memories into event and fragment/snapshot types. Different from Wessel et al., a no-age control group was added. Because of possible age differences in earliest memories between Western and Eastern cultures (see Wang, 2013), we restricted our sample to participants with a Western background as much as possible. We expected that a late age example would yield memories of a later age compared to a control condition (experiment 1 and 3) or an early example (experiment 3), whereas an early age example should yield earlier memories than a control condition (experiments 2 and 3). Furthermore, we predicted that fragment/snapshot memories should result in reports of an earlier age than event memories (experiments 1 - 3). Finally, we expected to observe a statistically significant age by memory type interaction such that the effect of the younger/older age manipulation would be stronger for event memories than for fragment/snapshot memories.

Experiment 1

Method

Statement of Transparency

The materials and pseudonymized data can be found at https://osf.io/uygjq/. The project was not preregistered formally, although an a priori power analysis was conducted as part of the procedure to obtain ethics approval. We collected the data of 464 eligible participants. For the age comparisons that are the interest of the present paper, we excluded participants who relied on others to generate the age in their first memory or who did not report a memory from childhood.

For the sake of comparability with earlier studies (Klusmann & Wessel, 2019; Wessel et al., 2019) we explored differences in memory characteristics (e.g., vividness, coherence) of event and fragment/snapshot memories that were coded by the experimenter, regardless of the explicit instruction to retrieve a fragment or event memory that depended on the participants' experimental condition. As these post-hoc comparisons are outside the scope of the present paper, we report on them separately at https://osf.io/tg7ux/.

Design and Power Analysis

We used a 2 (Age: late age vs no-age control) by 2 (Memory Type Instruction: fragment vs event) between participants design. An a priori power analysis was based on Wessel et al.'s (2019, study 1) findings. Because the critical Age by Memory Type interaction in that study resulted from an exploratory analysis, we adjusted the reported effect size of $\eta_p^2 = 0.039$ (corresponding to $f = 0.201$) downwards. A G*Power analysis (Faul et al., 2009) with $f = .15$, power = .80 and $\alpha = .05$, indicated that a minimum of 351 participants would be required. In addition, approximately 7% of the participants in the earlier study were excluded because of dishonest answering and asking their parents' help for estimating their age. Therefore, we added a similar percentage and set out to recruit N = 376 first year undergraduate students.

Participants

Eligible participants were students at a German or Dutch institution and had a Western cultural background. They were recruited and reimbursed with course credit through an online participant management system (SONA, see https://www.sona-systems.com/default.aspx) or were offered to enter a lottery to win one of five 15 euro gift vouchers. The Ethics Committee of the Department of Psychology at the University of Groningen approved the study.

In total, N = 464 eligible participants were included. For a detailed overview of the recruitment and selection procedures, see https://osf.io/bfyvt/. Of the eligible participants, $n = 107$ reported the age that someone else had told them. They were excluded from the analyses because age estimates generated by others may be less sensitive to the Age manipulation. In addition, the earlier, to-be-replicated study (Wessel et al., 2019) excluded participants reporting that they estimated their age with help of their parents. Inspection of the distribution of the age variable revealed that another participant was an extreme outlier (i.e., 137 months). As their report suggested that their memory did not refer to a childhood experience (“I just remember, that this was an example and about university”), this participant was excluded from all further analyses (cf. Greenberg et al., 2017, who used age older than 10 as a cut-off). Thus, the sample for the age comparisons was $n = 356$ (82 men). Their mean current age was 20.9 years ($SD = 2.7$; range 17 – 36 years).

Materials and Procedure

The study was conducted online, in the period of November 2015 - February 2016, using Qualtrics Survey Software (Version October 2015, Qualtrics, Provo, UT). The questionnaire consisted of multiple parts that are described below in the order of presentation. For details, see the questionnaires at https://osf.io/uygjq/.

Informed Consent and Demographic Information was obtained from all eligible participants (see https://osf.io/

Table 1. ***Means and Standard Errors for Reported Age in Memories (Uncontrolled and Controlled for Belief in Memories under Age 2) and Percentages of Experimenter-rated Memory Types (n = 356).***

	No-age control		Late age	
	Fragment (*n* = 93)	Event (*n* = 87)	Fragment (*n* = 89)	Event (*n* = 87)
Age in months (*M* (*SE*))				
Uncorrected	41.3 (1.78)	48.0 (1.84)	50.4 (1.82)	51.8 (1.84)
Corrected for belief in age < 2	40.7 (1.69)	48.8 (1.75)	49.9 (1.73)	51.9 (1.74)
Experimenter-rated Type (% (n))				
Fragment	3.2 (3)	1.1 (1)	3.4 (3)	2.3 (2)
Snapshot	59.1 (55)	21.8 (19)	53.9 (48)	27.6 (24)
Event	32.3 (30)	72.4 (63)	41.6 (37)	67.8 (59)
Other[2]	5.4 (5)	4.6 (4)	1.1 (1)	2.3 (2)
Belief in earliest memories < 2 y (% (n))	44.1 (41)	57.5 (50)	44.9 (40)	50.6 (44)

[2] Other = Repetitive events; General/extended time-period or Autobiographical fact / Association.

8eauf/).

Memory Type and Age Manipulations. The block randomization option in the Qualtrics Survey Software randomly allocated participants into one of four conditions. The conditions differed in the instructions for retrieving the earliest memory participants could think of. The participants read a description of either an event or fragment memory. The event memory description included key characteristics such as a narrative structure, a beginning and an end and a temporal sequence of events. The fragment memory description mentioned the decontextualized nature of the memory and the absence of a clear storyline and temporal sequence. The memory type descriptions were followed by two specific examples of the memory type in question, including information about a relatively late age for earliest memories (i.e., 6 - 8 years old) or no indication of age (no-age control). The themes in the memories were a birthday party and a visit to a swimming pool. Care was taken that the examples matched as closely as possible, except for the age information.

Memory Retrieval and Age Estimation. Participants were instructed to provide their earliest event or fragment memory in either Dutch, English or German. Importantly, they estimated the age at which the event in their memory happened by selecting the number of years (0, 1, 2,... – 11 years or older) and number of months (0 – 11) separately. In addition, they used slider scales (0 - 100) to indicate their confidence in these estimates of the number of years and months, and described how they arrived at these estimates.

Memory Characteristics. Participants completed the Short form of the Memory Experiences Questionnaire (MEQ-SF, Luchetti & Sutin, 2015), that assesses the phenomenology of autobiographical memory on ten dimensions (e.g., vividness, coherence, emotional intensity) as well as five additional items from Bruce et al. (2005). See https://osf.io/tg7ux/ for details.

Final Questions. An open question asking for participants' ideas about the purpose of the study served as a manipulation check. In addition, participants completed a number of "yes/no" questions asking whether: i) they believed it possible to have memories from age 2 or earlier; ii) someone else had told them their age in their earliest memory and if yes, whether they had reported that age; iii) there were any videos or photos of the target event[1] and iv) they had answered the questions truthfully.

Analyses

The main dependent variable was the reported age of the earliest memory in months. This estimate was calculated by multiplying the number of years by 12 and adding the months.

The age in months of *n* = 356 participants was subjected to a 2 (Age) x 2 (Memory Type Instruction) ANOVA. In addition, because Wessel et al. (2019) had controlled for the belief that memories before the age of two are possible, we repeated the ANOVA controlling for this dichotomous Belief variable by including it as a fixed factor. In addition, 2 (Age) x 2 (Memory Type Instruction) ANOVAs examined differences in confidence ratings of the age estimates in years and months. One value for confidence in years was missing. The percentages of participants who reported to have guessed or who used a strategy for coming up with different memories are reported in table 1 for the sake of completeness (see also Wessel et al., 2019) but were not further analyzed.

Irrespective of Memory Type Instruction, the nature of each memory (e.g., fragment, event) was coded independently by two raters following a standardized coding manual (see https://osf.io/489z6/). See for results and interrater

1 Given that only 12% of the sample responded affirmatively, these data were not considered further.

reliability https://osf.io/tg7ux/.

Within families of related tests, we corrected for inflated Type I error due to multiple testing using the Holm-Bonferroni procedure (Cramer et al., 2016). That is, we set the criterion for the smallest p-value to the conventional alpha level (α = .05) divided by the total number of tests. If the p-value was smaller than the adjusted alpha, testing was continued by setting the second-smallest p-value to alpha of .05 divided by the total number of comparisons minus 1; for the third-smallest p-value alpha of .05 divided by the total number of comparisons minus 2; and so on. Testing was discontinued after the first instance of $p >$ adjusted alpha.

Results

Table 1 summarizes the means and SEs of the age estimates in the four conditions.

Together, the uncontrolled ANOVA and the ANOVA controlling for belief in memories under age two contained seven comparisons. Because the comparisons between experimenter rated fragment/snapshots and event memories (see https://osf.io/tg7ux/) also included age, we corrected the alpha for a total of 8 comparisons (see https://osf.io/8vusg/). The uncontrolled ANOVA yielded a statistically significant main effect of Age, $F(1, 352) = 12.7$, $p = .0004$, $\alpha_{adj} = .007$, $\eta_p^2 = .035$ with the mean reported age being statistically significantly older in the late condition relative to the control condition. The Memory Type Instruction main effect was not statistically significant, $F(1, 352) = 4.94$, $p = .027$, $\alpha_{adj} = .0167$, $\eta_p^2 = .014$. This was the first instance of $p > \alpha_{adj}$ in the ranking, and thus all effects with p-values > .0167 were considered non-significant. The interaction was not statistically significant, $F(1, 352) = 2.07$, $p = .151$, $\eta_p^2 = .006$.

The ANOVA corrected for believing in memories of under the age of two yielded a statistically significant effect of this covariate, $F(1, 351) = 40.2$, $p < .00001$, $\alpha_{adj} = .006$, $\eta_p^2 = .103$. In this controlled ANOVA statistical significance was obtained for the main effects of Age, $F(1, 351) = 12.7$, $p = .0004$, $\alpha_{adj} = .008$, $\eta_p^2 = .035$, and Memory Type, $F(1, 352) = 8.61$, $p = .004$, $\alpha_{adj} = .01$, $\eta_p^2 = .024$. The interaction did not reach statistical significance, $F(1, 351) = 3.11$, $p = .079$, $\eta_p^2 = .009$.

As for confidence in the number of years and months in the reported memory, the Age by Memory Type Instruction ANOVAs yielded six comparisons in total. Alphas were Holm-Bonferroni corrected accordingly. None of the tests was statistically significant according to this criterion (see https://osf.io/uysj3/ for details.)

Discussion

The purpose of this experiment was to replicate the finding of Wessel et al.'s (2019) first experiment that providing an older age example yielded older estimates of the age of the earliest childhood memory than a younger age example. We included a control condition rather than an early age condition, and again showed higher age estimates after reading an older age example. However, the evidence for younger ages in participants who were instructed to retrieve memory fragments compared to participants who retrieved event memories was weak: the effect size was small and the difference only reached statistical significance in the analysis controlling for the belief that memories from an age under 2 years old are possible. In contrast to the original study (Wessel et al., 2019), there was no statistically significant example by type of memory interaction. Consistent with the previous study (Wessel et al., 2019), the results regarding confidence level in the age estimates were inconclusive. Thus, it remains unclear whether confidence ratings are affected by any of the manipulations.

Taken together, although examples of an earliest memory from a relatively late age (6 – 8 years old) yielded later memories than examples without age information, the present results were not in line with the idea that fragment/snapshot memories are less sensitive to such an age manipulation than event memories. This could be due to a less extreme age comparison (late versus control as compared to late versus early in the original study). Alternatively, perhaps the differential effect of age information is restricted to younger age examples, as suggested by Klusmann and Wessel's (2019) findings.

Experiment 2

Experiment 2 aimed to replicate the earlier findings (Wessel et al., 2019) now comparing an earlier age of the memory examples (event memories and fragments) with an example that does not mention an age. In Wessel et al. (2019) the comparison was with a late example. Here, the comparison was with a control version without an age indication, similar to Experiment 1. We expected participants to report earlier childhood memories in the early than in the neutral group and to a greater extent in fragment memories than in event memories.

Method

Statement of Transparency

The materials and pseudonymized data can be found at https://osf.io/kz2r4/. The project was not pre-registered. In contrast to study 1, we did not subject the memories to post-hoc coding of memory type (i.e., fragment/snapshot vs event).

Design

We used a 2 (Age: early example vs no-age control) by 2 (Memory Type Instruction: fragment vs event) between participants design.

Participants and Screening

In total, data were obtained from 200 first-year students from the Erasmus University Rotterdam. The data were collected online in the Fall of 2016. Participants were recruited through the Erasmus University student research pool. Those who signed up for the study received an e-mail containing a link to a questionnaire in Qualtrics. Participants' ethnicity was not part of the selection procedure. Similar to Experiment 1, we excluded n = 52 participants who said

Table 2. ***Means and Standard Errors for Reported Age in Memories (Uncorrected and Corrected for Belief in Memories under Age 2).***

	No-age control		Early age	
	Fragment (*n* = 35)	Event (*n* = 38)	Fragment (*n* = 37)	Event (*n* = 37)
Age in months (*M* (*SE*))				
Uncorrected	43.9 (2.44)	52.6 (2.44)	42.9 (2.47)	48.5 (2.47)
Corrected for belief in age < 2	44.4 (2.44)	52.5 (2.33)	42.9 (2.37)	48.0 (2.37)
Belief in earliest memories < 2 y (% (n))	51.4 (18)	44.7 (17)	45.9 (17)	40.5 (15)

[1] Alpha adjusted for multiple testing within the set of age comparisons (Holm Bonferroni)

to have reported the age that someone else had told them. In addition, we excluded one participant, whose report suggested that their memory did not refer to a childhood experience (age >= 12 years). All in all, the final sample consisted of n = 147 participants. Participants were randomly assigned to one of four conditions. Their mean age was 20.52 (*SD* = 2.72, Range = 17-34), female students comprised 86% of the sample, and international students comprised 50% of the sample. Participants were reimbursed with research credit. The research complied with the principles of the Helsinki Declaration for using human subjects (approval from the Ethics Committee of the Department of Psychology was not required).

Materials and Procedure

The materials and procedure were the same as in experiment 1, with the following exceptions:

1. Rather than a late age example, an early age example was used.
2. The lay-out and description of event memories and fragments in the instructions differed somewhat from those in experiment 1 to make them more specific. The examples were a bit more elaborate to make sure the participants understood the difference between event and fragment memories (see osf.io/kz2r4/)
3. The lay-out and answer options regarding nationality, first language, how participants arrived at their estimate of the age of their memory/fragment, and whether there were pictures or videos of the experiments were slightly different in the sense that some questions were phrased as open questions to get more information on the participant population
4. The strategy for coming up with the number of months was not asked.
5. The MEQ items were presented in a fixed rather than a random order as a choice by the experimenter (see https://osf.io/pfzrv/ for an example that applies to all conditions).

Analyses

All analyses were the same as in experiment 1, with the exception that the post hoc memory type analyses were not carried out.

Results

Table 2 summarizes the means and SEs of the age estimates in the four conditions as well as the confidence in the estimates and strategies used. Together, the uncontrolled ANOVA and the ANOVA controlling for belief in memories under age two contained seven comparisons. Alphas were adjusted accordingly, using the Holm-Bonferroni method. The uncorrected ANOVA yielded a statistically significant main effect of Memory Type Instruction, $F(1, 143) = 8.20$, $p = .005$, $\eta_p^2 = .054$ with fragments having a younger mean reported age than event memories. The main effect for Age was not statistically significant, $F(1, 143) = 1.06$, $p = .304$, $\eta_p^2 = .007$), nor was the Age by Memory Type interaction, $F(1, 143) = 0.41$, $p = .325$, $\eta_p^2 = .003$. The ANOVA corrected for believing in memories of under the age of two yielded a statistically significant effect of the covariate, $F(1, 142) = 13.87$, $p < .00001$, $\eta_p^2 = .089$. Again, the controlled ANOVA rendered statistical significance for the main effect of Memory Type Instruction only, $F(1, 142) = 7.61$, $p = .007$, $\eta_p^2 = .051$. The main effect of Age did not reach statistical significance, $F(1,142) = 1.58$, $p = .211$, $\eta_p^2 = .011$), nor did the Age by Memory Type interaction $F(1, 142) = 0.42$, $p = .520$, $\eta_p^2 = .003$).

As for confidence in the number of years and months in the reported memory, the Age by Memory Type Instruction ANOVAs yielded six comparisons in total. None of the tests was statistically significant (see https://osf.io/kz2r4/).

Discussion

The goal of Experiment 2 was to repeat Experiment 1 contrasting early rather than late age examples with no-age control examples. However, the hypothesis that providing early age examples would result in earliest childhood memories of a younger age than a neutral instruction was not supported. Controlling for Belief in memories before the age of 2 did not change the pattern of results and the main effect for Memory Type Instruction remained statistically significant. Similar to Experiment 1, confidence levels did not vary across conditions.

All in all, it seems that in line with the analysis controlling for beliefs in memories under the age of 2 in experiment 1, in experiment 2 the instruction to retrieve fragment rather than event memories yielded earlier childhood memories overall. Contrary to the findings in experiment 1, the evidence was inconclusive that providing early age examples affects the estimated age of earliest memories. This might mean that there is a limit to the malleability of memories to be manipulated with examples of an early age, although an earlier replication study (Klusmann & Wessel, 2019) found the effect for early and not late age examples. Alternatively, the final sample size in this study was modest. A sensitivity analysis using a Holm-Bonferroni corrected $\alpha = .007$ and power = .80 in G*Power (Faul et al., 2007) showed that $n = 147$ would suit detecting a medium to large effect size of $f = .30$. Thus, the current sample size would have lacked sensitivity to pick up smaller effect sizes. Nevertheless, similar to experiment 1, the results were not in line with the idea that the age manipulation is more effective for event than fragment memories.

Experiment 3

Taken together, Experiments 1 and 2 yielded mixed evidence for the effect of an age manipulation relative to a no-age control. The combined results suggested that a vignette referring to age 6 yielded later memories than a control vignette, whereas the evidence for the idea that an example referring to age 2 would yield younger ages remained inconclusive. In addition, there was no evidence that the effect of an Age manipulation varies with Memory Type. However, a prominent difference between the earlier study (Wessel et al., 2019) and the current experiments is that in the latter we instructed participants to come up with either a fragment or an event memory. Perhaps these instructions and examples were not clear enough to sufficiently distinguish between the two types of experiences. Note that this was a between subjects design in which participants received instructions for only one type of memory.

Therefore, in experiment 3 we replicated the original setup (Wessel et al., 2019, study 1) as closely as possible and relied on the memory types that were spontaneously generated by the participants. Differences with the original study were that we included a neutral condition (with a no-age example) as comparison for both an early as well as a late age examples condition (i.e., age of 2 years versus 6 years) and that participants were tested at the university rather than online. We hypothesized that compared to participants who received an example that was silent about age, participants provided with early age examples would recall memories from an earlier age and that participants in the late age example condition would estimate their age to be older. In addition, we expected for all three conditions that fragment/snapshot memories would be of an earlier age than event memories. Finally, we expected an age by memory type interaction such that age estimates in the late age condition would be higher for event memories than fragment/snapshot memories whereas the difference between age estimates of fragment/snapshot memories and event memories would be less prominent in the early age condition.

Method

Statement of Transparency

The method and analysis plan for this study were preregistered at https://osf.io/yfnru/. The materials and pseudonymized data can be found at https://osf.io/dz89e/.

We deviated from the preregistered plan for the following reasons. First, the sample size was N = 305 rather than the anticipated N = 315. By the end of January 2018, our resources were running out (i.e., participants did not sign up for the experiment anymore, presumably due to exams, and the test leaders approached the deadline for finishing their thesis projects). The pre-registered plan was to continue data collection in the new academic year. Yet, recruiting participants from a new cohort of students as well as training new test leaders might affect comparability. Because we were only 10 participants short and an N = 305 would still yield enough power to detect an ES of Cohen’s $d = 0.51$ for a one-tailed t-test between the smallest groups ($n = 102$ vs $n = 101$), we refrained from continuing the data collection.

Second, the preregistration anticipated the replication of all analyses in Wessel et al., 2019, exp 1). However, because the present focus is on the effect of the Age manipulation and on whether this effect varies with Memory Type, we report all other analyses on https://osf.io/dz89e/. These analyses involve associations with strategy (i.e., guessing vs using autobiographical knowledge) and comparisons of the characteristics of snapshot and event memories.

Third, minor deviations from the preregistration were that we i) included participants ($n = 7$) whose earliest memory description was coded as “no memory” because they wrote down the language they wanted to respond in rather than the content of the memory, but judging from the other ratings apparently did have a memory in mind; ii) parents’ ethnicity was not a prescreen selection criterion; and iii) we used Fleiss’ kappa for calculating the interrater reliability of the Memory Type scoring across three raters rather than separate Cohen’s kappa’s for pairs of raters.

Design and Power Analysis

We used a 3 (Age: early vs late example vs no-age control) by 2 (Memory Type: snapshot vs event) between participants design. The dependent variable was the reported age of the earliest memory. Participants were allocated randomly to one of the Age conditions. The Memory Type factor was experimenter-coded and was thus determined after data-collection.

To calculate the sample size, we focused on two comparisons of interest: the late age versus no-age control conditions and the early age versus no-age control conditions. Alpha was adjusted accordingly ($\alpha = .025$). Because the Cohen’s d effect size of the difference between the late versus early conditions in the original study was 0.61, we chose a Cohen’s d of 0.5 (medium ES) for comparisons with a no-age control. As study 3 was set up as a direct replication of Wessel et al. (2019) study 1, we set the power to .95. A power analysis (G*Power; Faul et al., 2007); see https://osf.io/s7vqp/) for a one-tailed t-test with these parameters yielded a sample size of 105 participants in each group, and thus a total N of 315.

Participants

Eligible participants were students from the University of Groningen (n = 205) and the Erasmus University Rotterdam (n = 104). In Groningen, participants were preselected based on their native language (Dutch, German or English) before they were invited to participate. In Rotterdam, the invitation to participate was accessible for all students in the local participant pool and Dutch, German or native English speaking students were specifically invited to participate. In total N = 309 participants were tested. The data of one participant were not recorded because of a defective keyboard. Three participants were excluded in accordance with pre-specified criteria (https://osf.io/yfnru/). They either indicated that they did not answer truthfully (n = 2) or recalled a memory from 11 years or older (cf. Greenberg et al., 2017; n = 1).

The final sample consisted of 305 students (54 men) with a mean age of 20.32 years (SD = 3.69; range 17 - 53). Participants were reimbursed with course credit (n = 284) or money (5 euros, n = 21). The Ethics Committee of the Department of Psychology at the University of Groningen approved the study, which applied to recruiting at the Erasmus University as well.

Materials and Procedure

The data were collected between November 2017 and February 2018. The materials and procedure were identical to those of study 1 from the Wessel et al. (2019) study, with some exceptions (see https://osf.io/yfnru/).

For the present purpose, it is important to note that the materials and procedure were similar to those used in study 1 of the present paper, with the following differences:

1. Participants who signed up for the study came to the lab and completed a questionnaire in Qualtrics. In Groningen, they were tested individually at desks separated by folding screens, with a maximum of 3 participants at the same time. In Rotterdam, participants were tested individually in one-person cubicles.
2. From all participants, informed consent was obtained on paper before the online questionnaire was started.
3. The questions about demographics included questions about the country of birth of both biological parents.
4. Participants were randomized into one of the three Age conditions (control, late, and early).
5. The instructions consisted of a brief explanation of both memory event and fragment types and a specific example of an event memory (birthday party) and a fragment memory (first steps, first bicycle ride, and a ride on the back of a bicycle, for the early, late and neutral Age condition, respectively).
6. Participants completed the Autobiographical Memory Characteristics Questionnaire (Boyacioglu & Akfirat, 2015) and items by Bruce et al. (2005) rather than the MEQ (Luchetti & Sutin, 2015)
7. The manipulation check did not ask for whether someone else had told the participants the age in their memory or whether there were any videos or photos of the target event.

Data & Analysis

Following the preregistered analysis plan, the mean reported ages in the earliest memory (in months) were subjected to two independent Welch t-tests comparing the no-age condition with the early age and late age conditions, respectively. Alpha was adjusted to .025. Likewise, we used Welch t-tests to test differences in confidence ratings of the age estimates in years and months. Alphas were Holm-Bonferroni corrected (4 tests). Additionally, a Welch t-test was conducted to test the hypothesis that the reported age in snapshot memories was lower than that of event memories.

Memory Type was coded independently by three raters who were blind to condition. Interrater reliability was good (Fleiss’ kappa = .85). See https://osf.io/s2brn/ for the coding manual and https://osf.io/5qhda/ for detailed information about interrater reliability. A 2 (memory type: snapshot vs event) x 3 (condition) ANOVA, controlled for belief in memories under age 2, tested whether age reports for the event memories were more sensitive to the manipulation than snapshots. Again, the alphas were Holm-Bonferroni adjusted to account for multiple testing (2 main effects and 1 interaction effect).

Results

Table 3 shows the means and standard errors of the reported age across conditions. The results indicated that the mean reported age in the control condition was statistically significantly older than in the early age condition, $t_{Welch}(200)$= 2.86, p=.005, Cohen’s d = 0.40. Likewise, the mean reported age was statistically significantly older in the late condition relative to the control condition, $t_{Welch}(192.8)$= 3.24, p=.001, Cohen’s d = 0.45. [2]

For the ratings of confidence in the years and months of the estimated age in the earliest memories, none of the comparisons between the conditions was statistically significant (see https://osf.io/qup7a/)

As can be seen in table 3, there were relatively few fragment memories. Overall, snapshot or event memories were the most common, although fewer snapshot memories (n = 120) were generated than event memories (n = 161). A comparison of the reported age in snapshot memories (m = 45.0, SE = 1.59) and event memories (m = 46.9, SE = 1.30) indicated that the difference was not statistically significant, t_{Welch} (248.2) = 0.941, p = .347, Cohen’s d = 0.11 [3]

To test whether the effect of age manipulation depended

2 The preregistered plan included additional non-parametric tests if skewness or kurtosis > 1. This was the case for the early condition. An additional Mann-Whitney U test (Z = -3.13, p = .002) yielded a similar conclusion as the t-test.

Table 3. ***Means and Standard Errors for Reported Age in Memories, Percentages (n) of Experimenter-rated Memory Types, and Belief in Memories under Age 2 for the Control, Early Age and Late Age Conditions (N = 305).***

	Age Condition		
	Control (*n* = 102)	Early (*n* = 101)	Late (*n* = 102)
Age in Memory, months (*M* (*SE*))	45.4 (1.48)	39.6 (1.37)	53.1 (1.85)
Memory type			
Fragment (% (n))	2 (2)	1 (1)	3.9 (4)
Snapshot (% (n))	37.3 (38)	44.6 (45)	36.3 (37)
Event (% (n))	55.9 (57)	46.5 (47)	55.9 (57)
Other[1] (% (n))	2.9 (3)	5.9 (6)	1 (1)
Error in reporting (% (n))	2 (2)	2 (2)	2.9 (3)
Belief in earliest memories < 2 y (% (n)	46.1 (47)	50.5 (51)	37.3 (38)

[1] Other = Repetitive events; General/extended time-period or Autobiographical fact / Association

Table 4. ***Corrected and Uncorrected Means and SEs of the Reported Age in Months for Snapshot and Event Memories within the Control, Early and Late conditions. The Corrections pertained to Belief in Memories < Age 2 (N = 281).***

	Age Condition		
	Control (*n* = 95)	Early (*n* = 92)	Late (*n* = 94)
Snapshot memories			
Corrected for belief in memories < Age 2	42.8 (2.40)	38.0 (2.21)	52.2 (2.45)
Uncorrected	43.8 (1.63)	38.4 (1.27)	54.05 (2.14)
Event memories			
Corrected for belief in memories < Age 2	47.1 (1.96)	41.1 (2.16)	50.5 (1.98)
Uncorrected	47.0 (1.46)	40.2 (1.65)	52.4 (1.77)

on memory type, a 3 (early age, late age, control) by memory type (snapshot versus event memory) ANOVA was conducted controlling for belief in early memories. The corrected means and SEs of the reported age in snapshot and event memories are given in table 4. There was a statistically significant main effect of Condition, $F(2, 274) = 14.2$, $p < .001$, $\eta_p^2 = .094$, such that numerically, the late condition reported the highest and the early condition the lowest avegares ages. The effect of memory type was not statistically significant, $F(1, 274) = 0.11$, $p = .300$, $\eta_p^2 = .004$. Importantly, the memory type by condition interaction effect was not statistically significant, $F(2, 274) = 1.0$, $p = .364$, $\eta_p^2 = .007$.

Discussion

The primary purpose of this experiment was to replicate the findings of Wessel et al.'s (2019) first experiment that age examples affect the estimated age of the earliest childhood memory, and that this effect varies with memory type. The results of the present study, that included a control condition and both an early and late age manipulation, demonstrated an effect of the age manipulation. An early age example resulted in an earlier age of the memories compared to the control condition as expected. Moreover, a late age example resulted in a later age of the memories relative to the control condition. Contrary to the expectation, evidence was inconclusive for the interaction between the age manipulation and the type of memory reported (event or snapshot).

General discussion

In three studies, we examined the malleability of age estimates of earliest childhood memories and varied the

3 Kurtosis was > 1 for the mean reported age in snapshot memories. A Mann-Whitney U test (Z = -1.38, p = .168) was also not statistically significant.

age in example memories (early, late, or no age), aiming to replicate the findings of Wessel et al. (2019, Study 1). Experiment 1 contrasted a late age instruction with a no-age control; experiment 2 included an early age manipulation and no-age control; and Experiment 3 included both age conditions and a control condition. Moreover, experiments 1 and 2 included a memory type manipulation, instructing participants to either retrieve a fragment or an event memory, whereas in experiment 3 we relied on an experimenter-coded distinction between snapshot and event memories. Overall, the experiments demonstrated effects of the age manipulation (Experiments 1 and 3), type of memory reported (Experiments 1 and 2) but not the age by memory type interaction that was reported in the original study (Wessel et al., 2019, Study 1).

The effects of the age manipulation are in line with earlier findings (Greenberg et al., 2017; Kingo et al., 2013; Klusmann & Wessel, 2019; Wessel et al., 2019) and support the idea that the reported age in earliest childhood memories is malleable. There are at least two types of explanations for these results. To begin with, providing age information in experimental instructions may induce anchoring, that is, a bias resulting from taking the provided age as a starting point for estimating (Greenberg et al., 2017). Although Greenberg et al. (2017) explicitly invited their participants to relate the age in their earliest memory to a reference age ("Did this happen before or after age 6?"), it may be that a more subtle manipulation like ours (including an age in examples of earliest memories in the experimental instructions) exerts a similar effect. Importantly, an explanation in terms of anchoring does not necessarily involve memory retrieval but reflects merely placing a time stamp on a memory.

Alternatively, the effects of an age manipulation may be explained with a retrieval context account that claims a heightened availability of memories within a certain time frame when this frame is activated (Dijkstra & Kaup, 2005). Accordingly, providing an example of an event that happened at a certain age should make memories from that time period more salient and accessible than simply mentioning age as a reference. In contrast to anchoring (Greenberg et al., 2017), in the retrieval context account the example of a memory in which the age is mentioned sets the stage for initiating the memory search within the pool of memories around this age. This means that the age information is utilized for the actual retrieval of the memory. Thus, the difference with anchoring (Greenberg et al., 2017) is that the age of the example here functions as a cue for retrieving a specific event memory or fragment rather than as a reference point (anchor) for assigning an age *after* the memory retrieval process has been completed.

It should be noted that with the present set of experiments, we cannot determine which account is more likely. For these types of studies, the underlying mechanism is difficult to ascertain. However, the main purpose of the present paper was to replicate earlier findings on the effect of age manipulations and memory type, as well as the interaction between the two, in three separate experiments Overall, the results of these experiments provide more convincing support for the age manipulation than for the memory type manipulation. However, we did not find support for the age manipulation by memory type interaction emerging from exploratory analyses in the Wessel et al. (2019) paper. There are several explanations for the current pattern of results.

First of all, the inconclusive finding regarding the age manipulation in experiment 2 may reflect a lack of sensitivity. That is, the sample size was adequate for detecting a relatively large effect size, whereas the results from experiments 1 and 3 suggested effect sizes in the medium range. The latter is more in line with small to medium effect sizes found in our earlier studies using the same method (Klusmann & Wessel, 2019; Wessel et al., 2019). It is also possible that an effect of an early age manipulation is harder to detect, although this seems less likely in light of the findings in experiment 3, and those of Klusmann & Wessel (2019) that the manipulation was effective for early but not the late manipulation.

Second, the hypothesis that on average, fragment/snapshot memories reflect memories from an earlier age than event memories was supported in experiments 1 and 2, although the evidence from experiment 1 was not particularly strong and on the whole, the effect sizes were small. In addition, it should be kept in mind that the memory type factor in these studies reflected instructions for the participants to generate a particular type of memory. We have no way of knowing whether these experimental instructions affect the retrieval process in some way, complicating any comparison with the earlier studies relying on spontaneously occurring memories. Nevertheless, the positive findings are in line with earlier studies showing younger average ages in "snapshots" (Klusmann & Wessel, 2019; Wessel et al., 2019) and fragments (Bruce et al., 2005) than in event memories. Post hoc coding in experiments 1 and 3 in this paper indicated that fragments were generated less frequently than event memories, which suggests a potentially lower availability of those types of experiences in the memory pool of early childhood memories in the first place.

Third, the original study (Wessel et al., 2019, study 1) reported a condition by memory type interaction such that the age manipulation in the early condition appeared to have had less of an effect than in the late condition, where event memories were older than snapshot memories. This interaction effect was considered to be important because if indeed, snapshots represent qualitatively different memories that are generated earlier in life and are relatively resistant to (age) manipulations, this would have implications for the theory of autobiographical memory development and the mechanisms underlying childhood amnesia. The interaction effect, however, failed to reach statistical significance in each of the present experiments. Likewise, a follow-up experiment in the original report (Wessel et al., 2019) as well as a large-scale replication study in a community sample (Klusmann & Wessel, 2019), yielded inconclusive age by memory type interaction effects. Thus, all in all, five different studies with varying designs (e.g., data collection both online and in the lab; including the manipulation or post-hoc coding of memory type; relying on student or community samples) failed to produce the differential effect of age instruction on snapshot and event memories. This, together with the observation that the original finding resulted from exploratory analyses, and thus may well have reflected a false positive finding (see for example Gelman & Loken, 2014), suggests that it may be time to abandon the search to confirm that the estimated age of snapshot memories is relatively impervious to suggestive information.

There are some methodological considerations that deserve attention. To begin with, the results of the three experiments in this paper underscore the need for replication studies. Yet, even though the results may increase our confidence that the age manipulation by memory type interaction is unlikely to be replicated in future studies, the Null Hypothesis Significance Testing (NHST) framework adopted here does not allow us to conclude that the true effect does not exist. In itself, our series of studies into the association between age manipulation and memory type exposes a problem that has been noted before in writings (e,g., Chambers, 2017; Gelman & Loken, 2014; L. D. Nelson et al., 2018) on a confirmation bias in the psychological literature. What happened here is that an initial exploratory analysis – among many other non-significant exploratory analyses - yielded a significant result. Subsequently, multiple failed attempts to replicate that finding accumulated, because each non replication in itself cannot be taken as evidence that the initial finding was wrong. Even now, after five failed attempts (the three present studies; Klusmann & Wessel, 2019; Wessel et al., 2019, study 2), formally the correct conclusion is that the evidence is inconclusive. The problem is that there is no clear decision rule on when to stop. On the whole, searching for non-existing phenomena may have multiple detrimental consequences (e.g., misinform theory, waste of resources, misinform policy making).

Another methodological issue concerns an apparent discrepancy between the type of memory that was generated by the participants and the post-hoc experimenter coding. In study 1, the experimenter classified 59.8% of the memories in the fragment conditions (regardless of the age manipulation) as being either fragment or snapshot memories. In the event conditions, the experimenter coded 70.1% as event memories. It is tempting to regard the experimenter coding as the gold standard and hence, conclude that the participants misunderstood the instructions or failed to generate the appropriate type of memory. However, the post-hoc coding depends on the quality of the description provided by the participant. It may be that the participant had the appropriate type of memory in mind, but that this is not picked up by the experimenter. Thus, based on our data, we cannot say whether participant-generated or experimenter-coded provides the superior method for examining memory type. More research is needed before we can draw conclusions regarding this issue.

Obviously, the generalizability of the findings is limited. Our samples consisted of young, highly educated adults with a Western cultural background. As previous studies indicated that the age of earliest memories may vary with cultural background (see Wang, 2013), it would be interesting to see whether the current results generalize to other samples.

Overall, considering all studies on age manipulations in earliest childhood memories so far, we can conclude that it is possible to manipulate the age of earliest childhood memories by providing information pointing towards an early or late age, but that the age manipulation by memory type interaction is unlikely to be replicated in future studies. The malleability of age estimates in earliest childhood memories not only applies to children being asked to retrieve their earliest memories (Bauer & Larkina, 2014; T. Peterson et al., 2009) but to adults retrieving these earliest memories as well (Greenberg et al., 2017; Kingo et al., 2013; Klusmann & Wessel, 2019; Malinoski & Lynn, 1999; Wessel et al., 2019). More research is needed in order to have deeper insight into the mechanism that underlies the retrieval process of early childhood memories. Comparing response latencies for earliest childhood memories generated with or without a retrieval context, similar to the Dijkstra & Kaup (2005) study, could be one way to get a better understanding of how the retrieval process takes place and whether the retrieval context account may explain the malleability of early childhood memories better than the anchoring account. Another possibility could be to have adults verbalize their memory search for early childhood memories and to categorize the retrieval strategies they share.

The present studies on earliest childhood memory retrieval contribute to improving our understanding of how the retrieval process can be affected by using relatively simple manipulations. Three failures to replicate the earlier finding that the effect of the age manipulation varies with memory type lend more confidence to the idea that this may have been a false positive finding. Future research may focus on the mechanisms underlying the age manipulation effects to gain a deeper understanding of retrieval of autobiographical memories from childhood.

Contributions

Contributed to conception and design: KD, IW

Contributed to acquisition of data: students, KD (Experiments 2 & 3), IW (Experiments 1 & 3)

Contributed to analysis and interpretation of data: KD, IW

Drafted and/or revised the article: KD, IW

Approved the submitted version for publication: KD, IW

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Competing Interests

The authors report no conflict of interest.

Data Accessibility Statement

Data are publicly available, but pseudonymised in the interest of privacy. This means that demographic information and answers to open-ended questions that could possibly identify participants (e.g., descriptions of memories and strategies) were deleted.

The preregistration of experiment 3 and all study materials, data, and code can be found at https://osf.io/h4nj6/

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SUPPLEMENTARY MATERIALS

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