IMAGINATION OR REPETITION?

THE POSSIBLE ROLE OF THE REFERENTIAL VALIDITY EFFECT

IN AUTOBIOGRAPHICAL MEMORY DISTORTION

by

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Recently a number of researchers have found that asking participants to imagine engaging in unlikely childhood events can increase their estimation that those events actually took place. This "imagination inflation" effect has been assumed to result from the process of imagination. However, this conclusion may be premature since prior studies investigating imagination have been confounded with repetition; i.e., participants receive more exposure to the "target" items (i.e., those items which are imagined) than to the "non-target" items (i.e., those items which are not imagined). Research on the referential validity effect reveals that estimations of the truth of propositions is increased for repeated items relative to items that are presented for the first time. Based on unpublished work investigating the potency of imagination inflation, and on the inherent similarities between the multiple exposures to target items in the imagination inflation paradigm and repetition utilized in studies investigating the validity effect, the current study tested the effects of imagining an item against those of simply being exposed to multiple repetitions of it. Results provided initial support for the hypothesis that repetition is the driving force between differences between targets and non-targets, and that they do not depend on the act of extensive imagination. However, a limitation in the design of this study (whereby control subjects engaged in some imagination, albeit far less that imagination subjects) suggests that the present findings must be viewed with caution. The implications of this limitation are discussed, and potential ways of honing the experimental design are offered.

Preface

I would like to extend my sincere thanks and appreciation to the following people:

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- The University of Pittsburgh Department of Psychology: Thank you for providing me with various administrative assistance, which was invaluable to the completion of this project.

- (Last, but of course not least) My family and friends: Thank for you giving me support and love during my graduate years up to this point, and for trusting in me to make the right decisions, whatever they might be.

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Introduction

"There's no place like home. There's no place like home. There's no place like home!"
With the repetition of this simple phrase (in addition to some fancy footwear), Dorothy in *The Wizard of Oz* suddenly *was* home. A fiction became reality after she said the sentence over and over again. Wouldn't it be wonderful if we could make something real simply by stating it time after time?

The present study aims to show that it is possible to make an event seem real through repetition. At the very least, the goal is to show that it is possible to make an event seem more real than had previously been thought. While the story of Dorothy and the repetition of her famous statement is fiction, back here in everyday reality, there exist cases in which people – over time – come to believe that certain events really did happen to them, even when there is corroboration that they once said the very same events had *not* in fact occurred. For example, various studies (Hyman, Husband, & Billings, 1995; Loftus & Pickrell, 1995) have demonstrated that people can come to believe that certain events (such as being lost in the mall) happened to them at a young age, even when information from parents and relatives indicates otherwise. Although researchers have proposed a variety of mechanisms as sources of false autobiographical memories (such as increasing familiarity, source dissociation, or the integration of misinformation), they have ignored one very simple and potentially powerful mechanism, namely the effects of simple repetition.

The research conducted here attempts to show that (innocuous) memory distortion can be achieved through repeatedly querying individuals about a low probability event. In recent years, various researchers have utilized imagination inflation as a popular approach to autobiographical memory distortion, the basic tenet of which is that items that are imagined show a greater change

in confidence scores (that the event did occur before the age of ten) from pre-test to post-test than items that are not imagined (cf. Garry, Manning, Loftus, & Sherman, 1996). The interpretation of this finding is that the act of imagination causes non-experienced or low-probability events to become more familiar, which in turns makes them seem as if they actually happened. In contrast, perhaps imagination per se does not yield these results, but rather it is multiple exposures to statements about the event that lead to increases in confidence scores. This expectation is based on research on the validity effect, which states that the judged validity of statements will increase for those statements that are repeated, but not for those that are not repeated (Hasher, Goldstein, & Toppino, 1977). A possibility exists that the validity effect might account for the increased memory ratings seen in studies utilizing the imagination inflation paradigm.

Background

A Popular Method of Distortion: Imagination Inflation

As stated above, imagination inflation has been a popular method for investigating autobiographical memory distortion (cf. Garry, Manning, Loftus, & Sherman, 1996). By comparing pre- and post-experiment answers on a Life Events Inventory (LEI), researchers found evidence that the percentage of events that show a positive change in confidence scores (from Time 1 to Time 2) was greater when the events were imagined, compared to when they were not imagined (a copy of the LEI can be found in Appendix A). The experimenters chose eight items as "target" items, in the sense that their data analysis would be conducted on these chosen items. Four of these items were imagined, and the other four served as controls; this was counterbalanced across participants. The researchers reported data analyses on those cases where a participant answered between 1 and 4 on the pre-test LEI. When considering these "low-end"

items, the experimenters found that the percentage of items that showed an increase from Time 1 to Time 2 was greater when the items were imagined, compared to when they were not imagined.

The researchers interpreted these results as showing that the act of imagining an item can lead to an increase in one's confidence that it occurred. They in turn conclude that the act of imagining appears to be a viable method for distorting autobiographical memory. They propose that the increased familiarity that results from the act of imagining is the most likely explanation for their results.

Reframing the Issue: The Referential Validity Effect

Consideration of the potential import of simple repetition arose from work conducted by Schooler and Halpern (unpublished data, 2000) testing the effects of imagination inflation against other paradigms (namely, thought suppression; see Wegner, Schneider, Carter, and White, 1987). They utilized the following basic design: a few weeks after completing the LEI, participants returned to the laboratory and were presented with an item (target) from the LEI and asked to answer a few questions about it. Following this, one of several (between-subjects) experimental manipulations occurred, followed by a series of questions about the item, one of which served as the LEI post-test question. Participants also then received the same set of questions about an item that had not been mentioned since the original pre-test (non-target). This series of events (presentation/short questions – same experimental manipulation – target questions – non-target questions) was presented for three more items. Targets and non-targets were counterbalanced across participants, and order of presentation remained constant.

Schooler and Halpern discovered that regardless of the actual experimental manipulation, target items showed more of an increase from Time 1 to Time 2 than non-target items. The relevance of this finding is that there ostensibly is something important about re-presenting the

target items that leads to increases in confidence, and that the specific manipulations in and of themselves may not account for the change in confidence ratings. Furthermore, when differences did exist between conditions (trends only), the driving difference was the degree to which participants were required to be involved with the target item, a point which becomes important later for the current design. Overall, then, perhaps the results obtained by researchers using the imagination inflation paradigm depend not on any particular manipulation, but rather on the effects of familiarity misattribution resulting from basic repetition. Furthermore, while increased familiarity resultant from the act of imagining remains an important component of the explanations for the effect (cf. Garry & Polaschek, 2000), it also functions as a key component of the referential validity effect.

The validity effect refers to the observation that participants will judge repeated statements as more valid than non-repeated statements (Hasher, Goldstein, & Toppino, 1977). A typical demonstration of this effect involves presenting participants with a number of statements, and asking them to rate the truthfulness of the statements along a Likert scale. After a delay (lasting usually a week or so, although the effect has been found with delays lasting only for minutes, cf. Hawkins & Hoch, 1992; Schwartz, 1982), participants are again asked to rate the validity of a number of statements. Half of the items are new, while the other half appeared on the earlier test. Research shows that participants judge the repeated statements to be more valid than the new statements; this is the basic referential validity effect. Research has also shown this effect to be a very general one. For example, it has been shown to occur regardless of whether the statement is true, false, or a matter of opinion (Arkes, Hackett, & Boehm, 1989). Also, actual repetition does not appear to matter so much as perceived repetition. This has been tested by

asking participants to provide a rating of familiarity/recognition for the statements, as well as a measure of validity. If the item feels familiar, then the effect will be observed.

Garry, Manning, Loftus, and Sherman (1996) reference work on the validity effect, and discuss the role familiarity may play: "[I]f [research] shows that asking twice about a purported fact results in higher validity ratings, it seems plausible that asking twice about a purported event may result in higher likelihood estimates" (p. 213). Unfortunately, they also caution against embracing the possibility of such repetition effects: "[W]e should exercise caution in going too far afield with a discussion of repetition effects; the effects…might actually be explained by regression to the mean, and research is needed to specifically investigate the possibility of repetition effects for past events" (p. 213).[1] Later work does not consider the possibility that repetition itself– the basic tenet of the validity effect – in truth might account for the apparent effect of imagining an item. The pilot research mentioned above, however (Schooler and Halpern, unpublished data, 2000), suggests that this effect might provide an alternative explanation for the imagination inflation results.

Imagination vs. Repetition

Given that a central element of the imagination inflation paradigm is the repetition of the imagined items, and given that repetition of items is known to increase peoples' confidence in their validity, it seems quite possible that the mere repetition of items may account for the imagination inflation findings. Unfortunately, however, previous imagination inflation studies have omitted a critical control condition, namely a condition in which items are represented but not imagined. Thus, while it is true that imagined events show more of a change than non-

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[1] Although Garry et al (1996) mention regression to the mean in their original paper, it should not be assumed that they consider it to be a leading explanation for their results. Indeed, as has been documented, they consider increasing familiarity to be a leading explanation.

imagined events, the two conditions are not entirely comparable because in the imagine condition, the participants are presented with the item before taking part in the manipulation, something that does not occur for non-imagined items (participants are simply asked about them again in a second LEI, and they are not represented in the experiment itself). To test if simple repetition truly can account for these results, a more appropriate experiment would allow for items both to be imagined and not imagined, but also to be represented in an initial exposure before the experimental manipulation occurs. When conceptualized in this light, the results from the work done by Schooler and Halpern (unpublished data, 2000) become clearer; all target items – regardless of experimental manipulation – showed more of an increase than non-target items. The common element of these items was not any one kind of manipulation, but rather that they all had been represented in the experiment before the manipulation took place. This element of representation is not shared by all items in the imagination inflation paradigm.

Changes to the Imagination Inflation Paradigm

The current study rectifies this potential confound by slightly reorganizing the order of events. Presently, in the imagination inflation paradigm, participants are presented with an item, are asked to imagine it, are then asked to answer some brief questions, and then are moved on to the next item. At the very end of the study, participants receive the LEI as a post-test, including items that are tested (not-imagined control items), but which have not been seen since the original LEI. The present study design corrects this confound of imagination with exposure by first presenting the items/short questions, and *then* introducing the imagination manipulation or the control manipulation. This reorganization then elucidates the potential role that repetition plays in the standard imagination inflation paradigm: When the validity effect is conceptualized in terms of the imagination inflation paradigm, one can see that the initial experimental

presentation of the item to be imagined may serve as the initial exposure (Time 1 in the validity effect), and the subsequent presentation via the post-test LEI may serve as the repetition (Time 2 in the validity effect). When the experimental design is reframed as thus, the possibility becomes clearer that the not-imagined items in the Garry, Manning, Loftus, and Sherman (1996) study might have already been at a disadvantage because they were not given the initial exposure; the Time 2 post-test LEI was therefore not a repetition. With this variation of the original paradigm, each item, regardless of subsequent manipulation, is afforded equal exposure at the outset.

An additional note about the differences between the Schooler and Halpern design and the standard imagination inflation design is warranted. In the former design, participants receive the post-test immediately following the imagination manipulation. In the imagination inflation paradigm used by Garry, Manning, Loftus, and Sherman (1996), all four sets of imagination trials occur in sequence, followed by the mass post-test (the second LEI). Since increased familiarity forms an integral part of the proposed study, it is important to determine if differences in the immediacy of the post-test questioning yield differences in participant confidence ratings. It is possible that shorter delays between the imagination manipulation and post-manipulation questioning (i.e., delays similar to the ones encountered in the Schooler and Halpern design) will have a smaller effect on pre-post test differences in confidence ratings since participants might be more sensitive to the fact that the items have just been represented. With longer delays (i.e., the traditional imagination inflation paradigm), this fact may become less salient, and participants may be more susceptible to confusing their familiarity with actual remembering.

The current project therefore is a 2 x 2 x 2 mixed design, with between-subjects factors of 1) imagination manipulation (imagination or control) tested against 2) the immediacy of the post-test (immediately following the imagination manipulation or delayed until the end). The within-

subjects factor is whether the item is a target (presented and manipulated in some way - imagination or control) or a non-target (not presented again except for the post-test questioning). As outlined below, these manipulations aim to test whether repetition can account for the results in the work done by Garry, Manning, Loftus, and Sherman (1996), and whether the effects of the resultant familiarity can be used to explain the results found by Schooler and Halpern.

Predictions

Based on the findings that repeated items show more of an increase in truth rating relative to non-repeated items (the validity effect), the present study aims to show that targets and non-targets will differ from one another, regardless of the imagination condition to which a participant is assigned. Target/non-target differences should stand, regardless of whether a participant imagines the target or not.[2] What is critical is that targets will be repeated, while non-targets will not be, and it is this manipulation – and not that of imagination – which should yield increased confidence ratings from Time 1 to Time 2 for targets relative to non-targets. In other words, this study will show that validity inflation is the source of the so-called imagination inflation effect; repetition influences target/non-target differences, not the act of imagination.

If the imagination inflation effect is in fact due to a misattribution of familiarity due to repetition effects, then differences between targets and non-targets may also change as a function of delay. When tested immediately, misattributions of familiarity may be reduced because participants can readily attribute the sense of familiarity to the fact that they had been just been exposed to the item for the past few minutes. However, following a delay, participants may have

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[2] Indeed, if confusion of the source of an item's familiarity is crucial (the act of imagining the item might increase its salience), and if someone is aware of the source of familiarity (i.e., realizing that one has just imagined an item), then confidence in its past occurrence should, at the very least, not change, and at the most, decrease. Once this appropriate control has been introduced, imagination per se as an explanation for memory distortion may be ruled out in favor of the validity effect.

a greater chance of confusing the source of their familiarity with actual remembering. Therefore, another prediction in the present study states that short delays may yield less of an effect on target/non-target difference scores from Time 1 to Time 2 than longer delays, based on the fact that in the longer delays, there will be a greater chance for participants to become confused about the source of their familiarity. It is also important to note that the inclusion of the delay condition allows for an approximation of the paradigm used by Garry et al. (1996), in which all items were presented before Time 2 testing occurred. The import of this aspect of the current design is such that it considers the original design Garry et al. used, and therefore allows for cleaner conclusions.

Methods

Participants

      The participants in this study were drawn both from the Introductory Psychology classes at the University of Pittsburgh and from students seeking to participate in paid studies in the University area (encompassing the University of Pittsburgh and Carnegie Melon University). The majority of the students received course credit for completing the entire experiment, while those drawn from the larger University area received $7 upon completion of the study. Participants were not eligible for the experiment proper (presentation and manipulation of items) unless they had completed an initial Life Events Inventory at least two weeks prior to the experimental session itself. The study was advertised as being concerned with how people answer questions about possible past experiences that are presented to them. The anticipated number of participants per cell was at least 40, for an anticipated total of at least 160 participants.

<u>Design</u>

The majority of this study was conducted with paper and pencil, and consisted of two testing periods no less than two weeks apart. The pre-test portion of the study involved potential participants completing the Time 1 LEI, submitted via electronic mail or a paper-and-pencil form returned to the experimenter. The remainder of the study involved a mixed design, testing the within-subjects factor of item type (target vs. non-target) against two between-subject factors, each consisting of two levels: imagination (imagination vs. control) and immediacy of the post-test/Time 2 LEI (immediately following the imagination manipulation or delayed until the end). Testing for this part of the experiment was done in groups. The dependent variables of interest were the computed LEI difference scores from Time 1 to Time 2 for each item, and the percentage of items that increased from Time 1 to Time 2.

<u>Materials</u>

The Life Events Inventory is a 20-item questionnaire consisting of questions regarding possible events that may have been experienced before the age of 10 (see Appendix A). Participants are asked to indicate their confidence that an event occurred to them along an eight-point scale (1: Definitely did not happen, 8: Definitely did happen). The events chosen to be included in the data analysis are indicated with * in Appendix A; these are the same as those used by Garry, Manning, Loftus, and Sherman (1996).

Crossword puzzles used in the control conditions were taken from a book of standard easy crossword puzzles.

<u>Procedure</u>

After giving informed consent for the initial pre-test questionnaire, potential participants filled out the pre-test LEI (which takes at most ten minutes to complete), and in doing so

provided a Time 1 measure of their confidence that certain events did or did not occur to them before the age of ten. These Time 1 scores were obtained via both mass testing or via individual e-mail messages to people who expressed interest in participating. If there were no complications or problems with the LEI, participants were invited back to participate in the experiment itself. Potential participants were not allowed to attend an experimental session less than two weeks from the time they filled out the Time 1 LEI. This length of time is standard for imagination inflation research, and was used by Garry, Manning, Loftus, and Sherman (1996).

Before participating in the experiment proper, participants were once again asked to provide informed consent, this time for the experiment itself. Testing was done in groups, ranging in size from as small as four people to as large as fifteen. The experimenter told participants that over the next hour, they would be presented with a number of past experiences, and that they would be asked to answer some questions about them. The imagination manipulation and immediacy manipulation were both between subjects, so the procedure and order of events were the same for everyone within any one group.

Once the initial instructions above were given, the experimenter informed participants that over the next five minutes, they would be asked to imagine an event occurring to them from before the age of ten, and that they were to answer some questions about the event. The actual target item was never identified out loud, and was only indicated to the participants in their booklets. Participants were given a short description of the first item which they read silently to themselves, and they then answered a few short questions about the item, such as "Who might you have been with?", "What was the order for events?", and other such questions designed to establish the situation. Participants were given up to five minutes to do this, though if less time was required, the group was moved on to the next part of the experiment.

At this point, the experimental manipulation testing the effects of imagination occurred. In the imagine condition, participants were told that for the next two minutes, they were to sit quietly and to do their best to imagine the event with which they were just presented. In the control condition, participants were asked to work on a crossword puzzle (from a book of "easy" crosswords) for two minutes. In either case, the full two minutes was allowed to elapse. For each two-minute period in the control condition, the same crossword puzzle was used.

The sequence of events that followed this two minute period depended on the immediacy manipulation to which the group had been assigned. Participants in the "immediate" group received a series of questions about their memory for the target item that was presented, one of which was the LEI question (and which thus served as the Time 2 measure of confidence). Following these questions, participants then answered the same series of questions for a yolked non-target item, again providing a Time 2 measure for that item. Finally, the participants answered a short set of manipulation checks for the target and non-target items. There was no time limit on answering these questions. Once everyone answered all of the questions, the group was then given the instructions regarding the next five minute presentation/short questions period. The two minute period followed this five minute period, followed by the set of memory questions and manipulation checks for this second target/non-target pair. This procedure (target presentation – experimental manipulation – target questions – non-target questions) was repeated two more times, for a total of four "trials" of target item manipulation.

The participants in the "delayed" condition, instead of answering the memory questions after the first two-minute period, were moved on to the second target item to be presented. The experimenter repeated to the participants the same instructions that over the next five minutes, they would be asked to imagine an event occurring to them before the age of ten, and that they

would be asked to answer some questions about it. The two minute imagination manipulation followed, and the entire procedure (target item presentation – experimental manipulation) repeated for the next two target items. Once the fourth imagination manipulation occurred, participants were asked to work on a different crossword puzzle for fifteen minutes, in order to guard against recency effects for the final target item. After the full time period had passed, all of the memory questions (in order from the first presented target and its yolked non-target) were presented, with no time constraint placed on them. After answering the final set of target/non-target memory questions and manipulation checks, participants were debriefed.

A total of four pairs of targets/non-targets was used, with the same items paired together across the experiment. Two lists were constructed so that those items that were targets for some participants were be non-targets for others, and vice versa. In order to guard against an effect of recency and primacy, the lists were also counterbalanced across participants for order of presentation. The first item that was presented to one participant might have been the fourth and last item that was presented to another participant; the order was only forwards or backwards. Since the only parts of the protocol that the experimenter actually read out loud were the instructions regarding the full hour of the experiment, and each generalized introduction to the five minute presentation/short questions period, the actual lists (A or B) and list orders (forwards or backwards) were able to be counterbalanced across participants within the same experimental session, since the experimenter never verbally indicated what the actual item in question was.

Results

A total of 174 participants took part in this experiment. However, four of them had to be removed for failing to provide a Time 1 LEI or failing to follow directions, leaving a total of 170 participants to be included in the data analyses, 60 male and 110 female.

A comparison of the mean Time 1 LEI scores as a function of whether they were for targets or non-targets revealed that no significant differences existed between the two, t (169) = .747, p = .456 (mean = 3.24 and 3.15, targets and non-targets, respectively). Time 1 confidence ratings also did not differ as a function of experimental condition (imagine vs. control), F (1, 166) = .402, p = .527 (mean = 3.15 and 3.25, respectively), which is to be expected. These results indicate that the different items/groups started at approximately the same place.

The two primary measures of interest include difference scores (Time 2 rating –Time 1 rating) and the percentage of a sub group that moved in a certain direction (for example, increase). In both cases, a single value could be computed a number of ways for each participant. These measures could be analyzed in terms of target/non-target, in which case a single value for "target" was computed simply by averaging (for example) the difference scores for a single participant's set of "target items," and likewise for non-targets. Alternatively, (for example) difference scores could also be analyzed in terms of whether they were based on a low or high Time 1 score, in addition to being connected to a target or non-target. This analysis is useful for determining the effects (if any) of regression to the mean.

Target/Non-target Comparison[3]

The primary data of interest concern how targets and non-targets performed relative to one another within the context of the imagination condition (imagination or control) to which a participant was assigned. Considered were both difference scores (Time 2 – Time 1) and the percentage of items that increased from Time 1 to Time 2. When considering only Low (1 – 4)

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[3] As per the analyses performed by Garry, Manning, Loftus, and Sherman (1996), Time 2 confidence ratings were split according to whether the corresponding Time 1 (LEI) score was "Low" (1 – 4) or "High" (5 – 8). Garry, Sharman, Wade, Hunt, and Smith (2001) also make reference to a theoretical difference in splitting the LEI along the midpoint vs. splitting it along the mean itself (for purposes of explaining away the potential role of regression to the mean, since a midpoint split in their view more accurately corresponds to "did happen" vs. "did not happen"). For the purposes of clarity, the present study uses both "splits" here.

items (split along the midpoint of the LEI scale itself), when difference scores were analyzed as a function of imagination condition, the "imagine" targets/non-target performance did not significantly differ from the "control" target/non-target performance [$F(1, 164) = .008$, $p = $ n.s.]. However, when targets and non-targets were considered overall, regardless of imagination condition, a significant difference in difference scores did exist, $F(1, 164) = 7.04$, $p = .009$ (mean = 1.20 and .82, respectively) (see Figure 1A). Targets showed more of an increase from Time 1 to Time 2 than did non-targets.

Figure 1A. Mean Difference Score for LOW (Midpoint Split) Items.
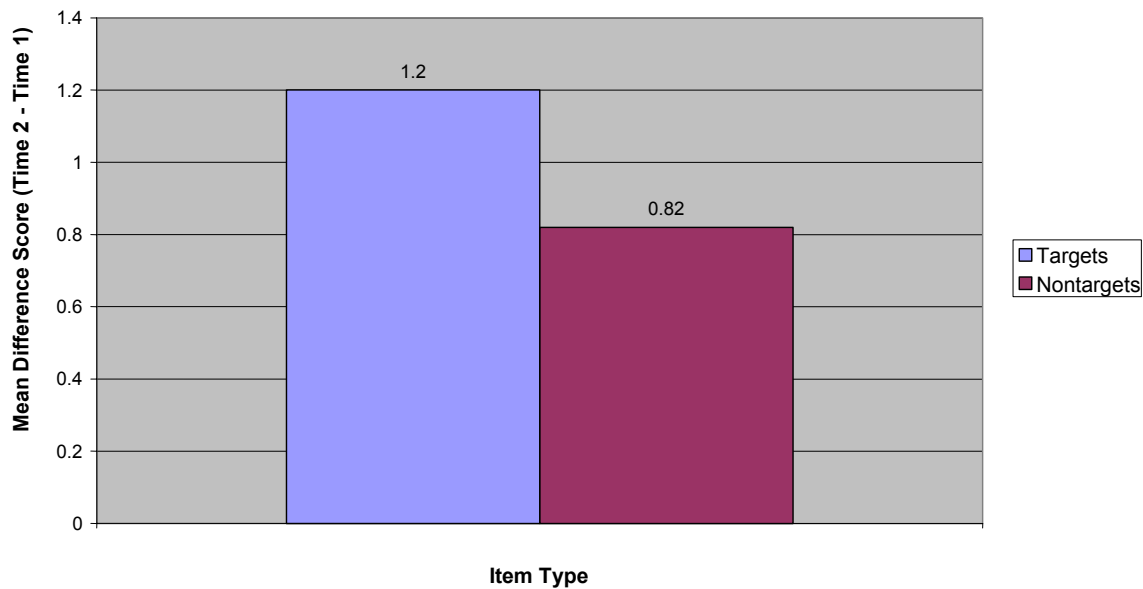
A similar trend was borne out for the percentage of items that increased. There was not a significant difference between the percentage of targets that increased relative to non-targets, as a function of imagination condition [$F(1, 164) = .132$, $p = $ n.s.]. However, when considered

overall, a significant difference existed in the percentage of Low targets that increased from Time 1 to Time 2 relative to the percentage of non-targets that increased from Time 1 to Time 2, $F(1, 164) = 5.25$, $p = .023$ (mean = 39.6% and 32.3%, respectively) (see Figure 1B). A greater percentage of targets increased from Time to Time 2 than did non-targets.
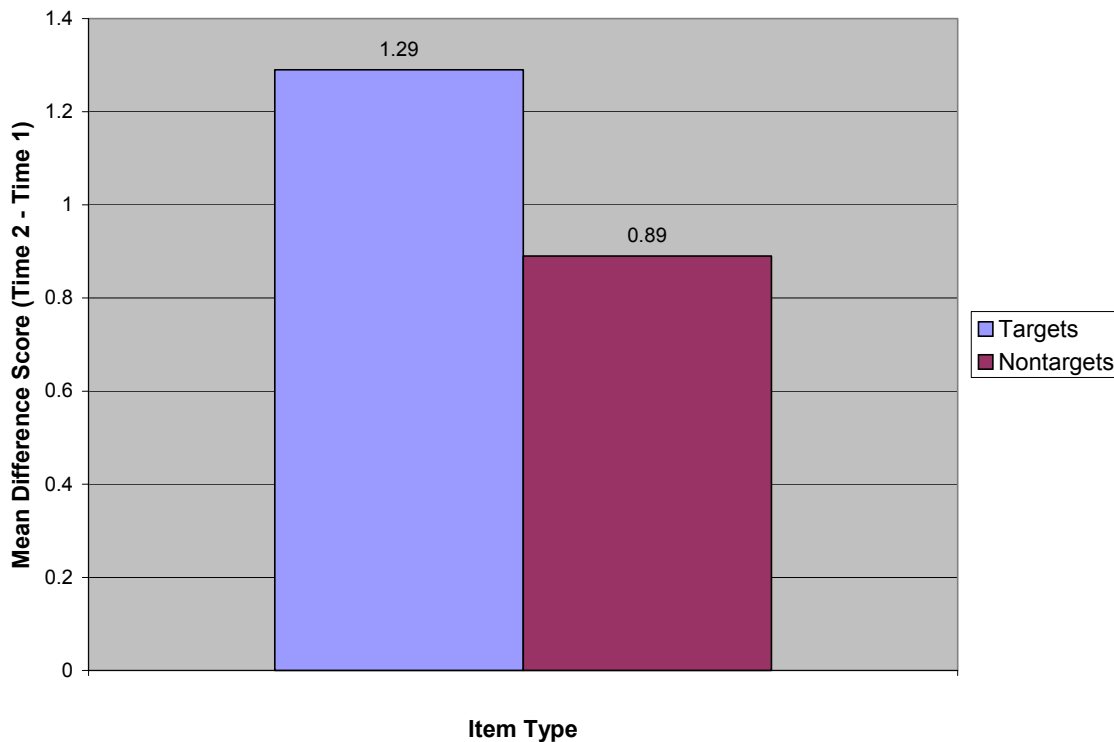
Figure 1B. Percentage of LOW (Midpoint Split) Items that Increased from Time 1 to Time 2.
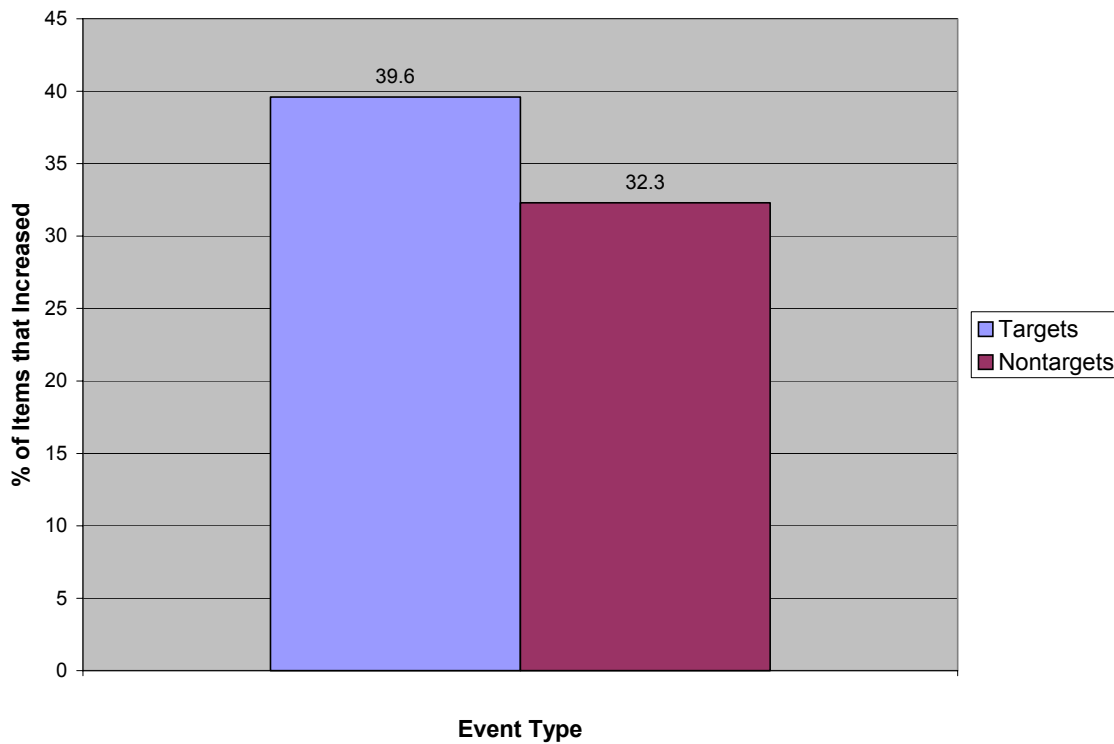
In sum, when considering Low targets and non-targets, as defined by the split of the LEI scale itself $(1 - 4, 5 - 8)$, the two show comparable differences regardless of which experimental condition – imagination or control – the participant happened to be in.

As mentioned, a Low-High split for each item based on its own Time 1 mean could also be utilized. In all cases, this resulted in a Low-High split occurring below 4. Target difference scores did not differ from non-target difference scores as function of imagination condition [F (1, 158) = .000, p = n.s.]. However, when considering Low difference scores of targets and non-targets overall, there was a significant difference between the two, F (1, 158) = 7.38, p = .007 (mean = 1.29 and .89, respectively) (see Figure 2A). Targets showed a greater increase from Time 1 to Time 2 than did non-targets.

Figure 2A. Mean Difference Score for LOW (Item Specific Mean Split) Items.

When considering the percentage of Low (actual item split) items that increased, the percentage of targets that increased did not differ from the percentage of non-targets that increased, as a function of imagination condition [$F (1, 158 = .243), p = $ n.s.]. However, targets and non-targets did exhibit a significant difference, $F (1, 158) = 5.04, p = .026$ (mean = 39.2% and 31.4%, respectively) (see Figure 2B). A greater percentage of targets increased from Time 1 to Time 2 relative to non-targets.

Figure 2B. Percentage of LOW (Item Specific Mean Split) Items that Increased from Time 1 to Time 2.
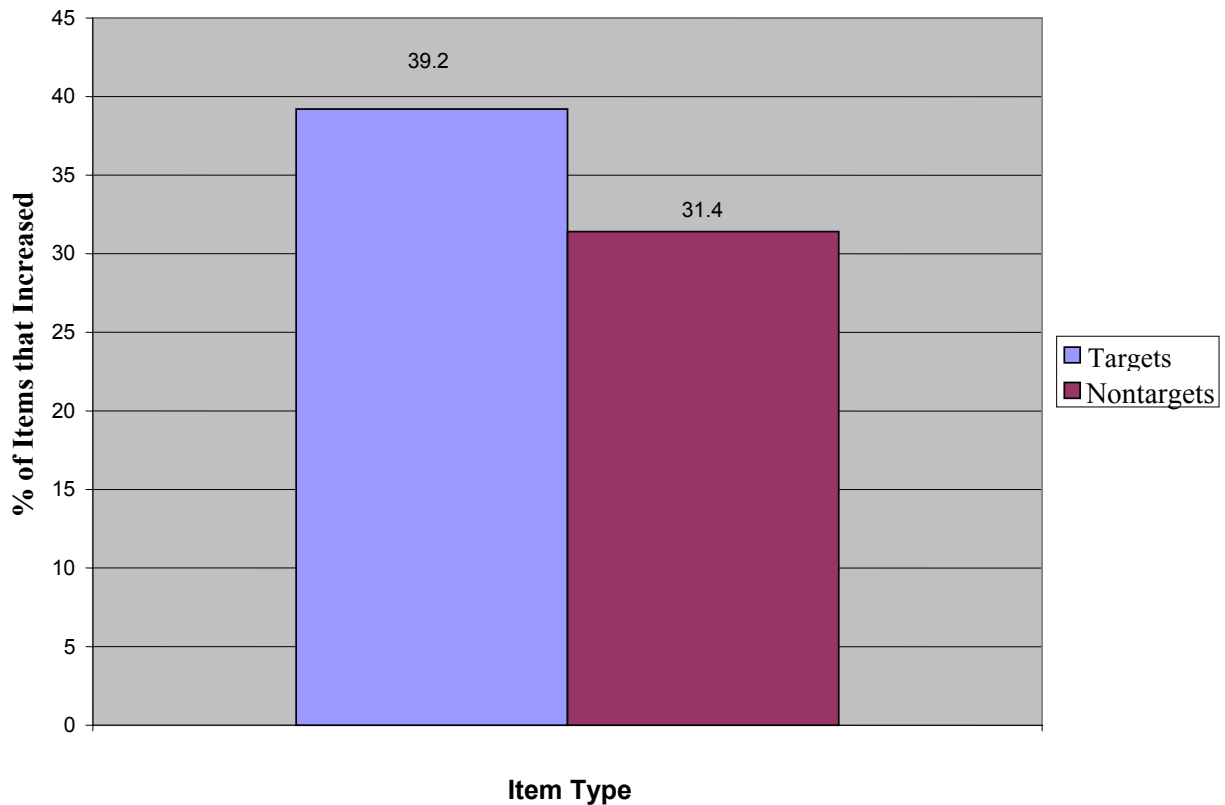
In sum, when considering Low targets and non-targets – the split being based on an item's own Time 1 mean – the expected target/non-target differences occurred despite the experimental condition – imagination or control – in which a participant happened to be placed.

Also of interest was the delay which a participant experienced, and the results indicate that whether the Time 2 questions were asked immediately or after a delay did not affect how the target and non-target difference scores compared [F (1, 164) = .368, p = n.s.]. There also was not a significant difference between the percentage of targets that increased relative to non-targets, as a function of delay [F (1, 164) = .257, p = n.s.].

The results were replicated when based on the item specific Low/High split. Target difference scores did not differ from non-target difference scores as function of whether the Time 2 questions were asked immediately or after a delay [F (1, 158) = .319, p = n.s.], nor did the percentage of targets that increased differ from the percentage of non-targets that increased, as a function of delay condition [F (1, 158 = .243), p = n.s.].

None of the interactions reached significance.

Manipulation Checks

A series of manipulation checks indicated that the only items that participants were cognizant of during the entire experimental procedure were the targets; this was measured by asking participants "During the 2-minute waiting period, how much did you try to think about [target or non-target event]?" (1 = Not at all, 8 = Very much). Participants rated targets significantly higher than non-targets, F (1, 160) = 271.8, p = .000 (mean = 4.00 and 1.58, respectively). Participants reported imagining targets to a greater degree than non-targets.

Analyses also revealed that the degree to which participants were cognizant of any item varied according to imagination manipulation. Participants in the imagine condition rated their

items higher on the manipulation check than control participants, F (1, 160) = 69.86, p = .000 (mean = 3.62 and 1.97, respectively). Overall, participants reported more imagining when they were in the imagine condition.

There was also a significant interaction between target/non-target and imagination condition. Not only did targets and non-targets differ within each imagination condition, but imagined targets were rated higher on the manipulation check than were control targets, F (1, 160) = 113.66, p = .000 (mean = 5.61 and 2.40, respectively) (see Figure 3). Participants reported that they imagined targets to a greater degree if they were placed in the imagine condition.

Figure 3. Mean Score on Manipulation Check, as a Function of Target/Non-Target and Imagination Condition.
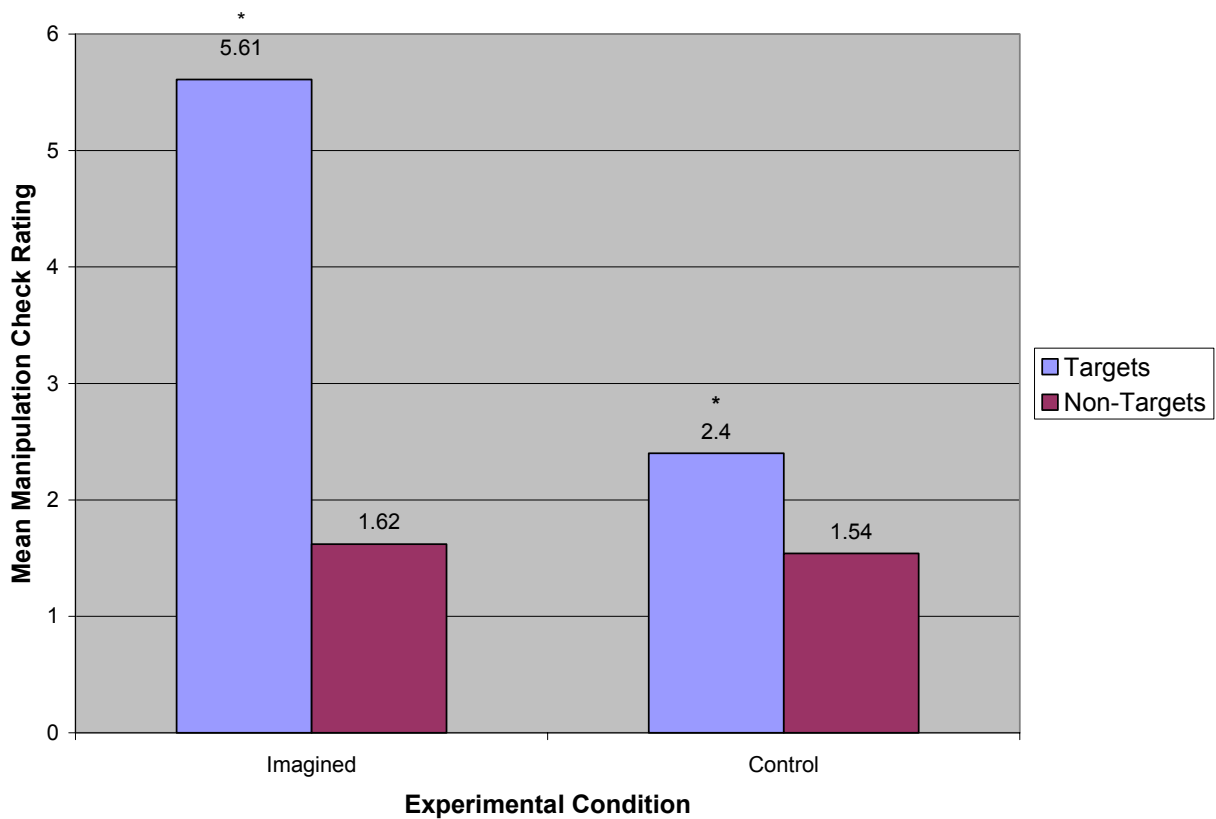
Regression to the Mean

As mentioned, it is also possible to analyze the data comparing the performance of low items to that of high items. This becomes important for determining whether the obtained results reflect a true effect or simply basic statistical movement.

When considering those difference scores based on the midpoint split, as a function both of target/non-target assignment and whether the item was low or high, the results show that low and high items significantly differed; high items decreased slightly more than low items increased, $F(1, 92) = 172.65$, $p = .000$ (mean = -1.07 and .93, respectively). When considering whether an item was low or high, and whether it was a target or non-target, the results mirror the previous ones, in that high targets decreased slightly more than low targets increased, $F(1, 92) = 4.74$, $p = .032$ (see Figure 4). The basic target/non-target differences were also replicated.
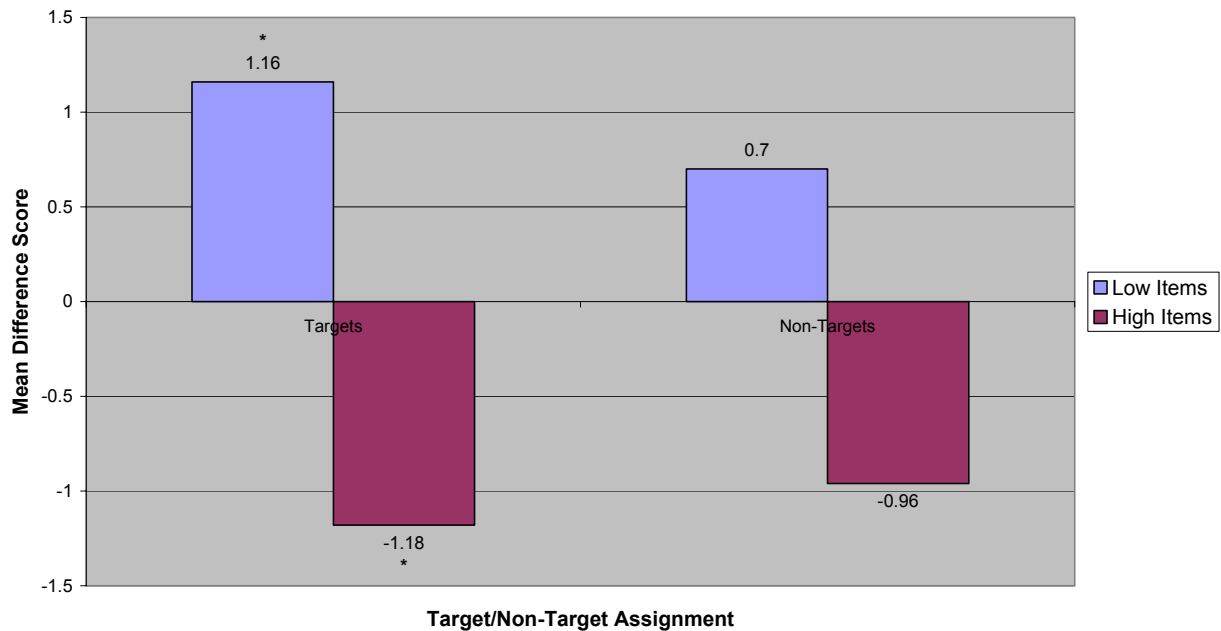
Figure 4. Difference Scores as a Function of Target/Non-Target and Midpoint Split.

When considering those difference scores based on the item specific mean split, as a function both of target/non-target assignment and whether the item was low or high, the results show that overall, not only did low items increase more than high items decreased [$F_{(1, 109)} = 173.22$, $p = .000$, mean = 1.06 and -.77, respectively], they also show that low targets went up more than high targets went down, $F_{(1, 109)} = 7.54$, $p = .007$ (see Figure 5). The basic target/non-target differences were also replicated.

Figure 5. Difference Scores as a Function of Target/Non-Target and Item Specific Mean Split.
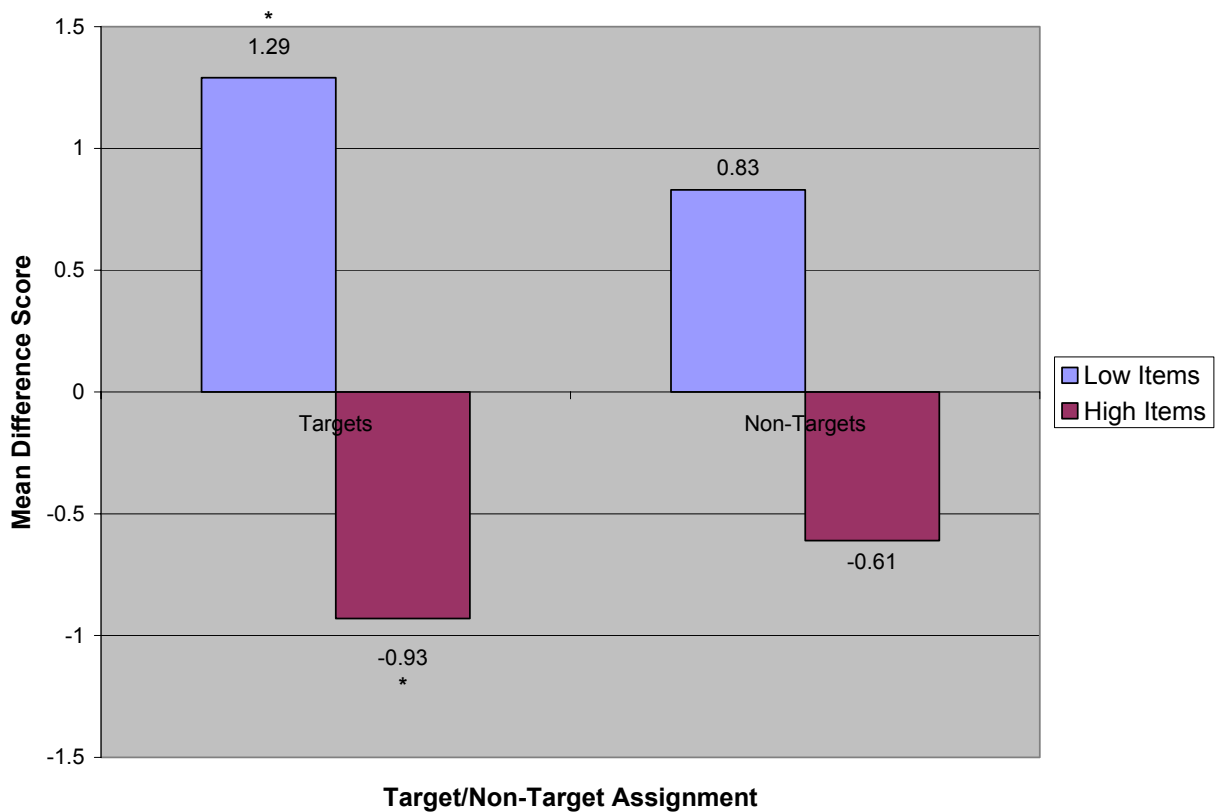
Discussion

In the current study, regardless of whether participants engaged in extensive imagination, targets and non-targets significantly differed with regards both to confidence ratings (targets show greater difference scores from Time 1 to Time 2 than non-targets) and percentage of items that increased (a greater percentage of targets increase than do non-targets). These results confirm the main prediction of this study, that targets and non-targets should significantly differ from one another irrespective of whether participants were assigned to the imagination or control condition. These differences also lend support to the role of the validity effect in autobiographical memory distortion. First of all, the differences between targets and non-targets replicate the main findings by Garry, Manning, Loftus, and Sherman (1996), which show that extensive imagination of "Low" target items leads to 1) greater difference scores between targets and non-targets from Time 1 to Time 2, and 2) a greater percentage of targets that increased from Time 1 to Time 2, relative to non-targets. More importantly, however, when considered in light of the fact that extensive imagination may not in fact be crucial (as evidenced by the lack of interaction between targets/non-targets and imagination/control), the target/non-target differences lend credence to the role of the validity effect in results apparently stemming from "imagination inflation."

Because the design of imagination inflation studies is such that imagination of targets has always been confounded with their constant presentation to participants, researchers cannot undoubtedly claim that imagination alone results in the increase in confidence ratings. The present finding that extensive imagination does not influence the performance of targets relative to non-targets indicates that repetition might be playing an active role, and that the extended imagination instructions present in imagination inflation studies are not necessary. An important

caveat needs to be made, however, regarding the instructions given to the different experimental groups; the control group received ample opportunity to visualize the target items via pre-manipulation instructions. It is unclear at this time how this might have affected the results, if at all, a point to which will be returned later in the discussion.

The prediction regarding the 2nd between-subjects factor of the immediacy of the Time 2 LEI was not supported. Regardless of delay, targets and non-targets showed comparable differences. This result is nevertheless crucial because it demonstrates that the similar observation that was made in the pilot work (Schooler and Halpern, unpublished data, 2000) was not an artifact of that particular experimental design (immediate testing). If people did in fact forget the source of their familiarity, it would be expected that an interaction between targets/non-targets and delay of questioning would occur. It is possible, however, that the delay that participants experienced was not long enough to produce an interaction with the basic target/non-target difference; it lasted on average 10 or 15 minutes. This time might not be enough to allow for the knowledge to face sufficiently. As the study is currently framed, though, it appears to be the case that as with imagination of the item, the differences that exist between the performance of targets and non-targets can be attributed to the inherent differences between targets and non-targets themselves; i.e., to the fact that targets are designed to be repeated and non-targets are not.

A note must be made regarding the potential presence of regression to the mean, a concern which was addressed by splitting the Time 1 LEI's both along the midpoint of the scale and along the mean of each individual item. Recently, Pezdek and Eddy (2001) successfully replicated the basic imagination inflation effect, that is, when considering only low-end items, more imagined items increase than non-imagined items. However, they argued that high-end

items must be considered, as well, and that one cannot fully understand the apparent effects of imagination on confidence ratings without entertaining the possibility of regression to the mean. They demonstrated that both imagined and control high-end items tended to show a decrease from Time 1 to Time 2, and that imagined items and control items did not significantly differ from one another. In fact, Pezdek and Eddy show that in general (i.e., when considering both those items used in the data analyses and those which were part of the LEI's but never used during testing), low-end items tend to move up while high-end items tend to move down. Because this trend occurs for all items, then, regardless of manipulation, Pezdek and Eddy concluded that the driving force of imagination inflation is not the effect of imagination, but rather is an artifact of repeated statistical testing, namely, that ratings over time tend to center around the mean.

This concern is what prompted the decision to analyze the data along both kinds of splits. The results show, however, that low and high items do significantly differ in their performance, despite the parameters by which such assignment occurs. If regression to the mean were present, low and high items should not differ. Since this is not the case when using either split, the concerns raised by Pezdek and Eddy (2001), while valid and thoughtful, are not an issue and have no bearing on the results obtained in the present study.

At first glance, it would appear that the present results provide evidence refuting the claims made by Garry et al. In other words, the results would indicate that it does not matter whether participants imagine target events or not; it appears that simple repetition of an item across the experimental session is enough to boost not only confidence ratings themselves, but also the percentage of items that do increase. The time at which the Time 2 rating is provided also does not appear to be of consequence, since immediate answers and delayed answers are

comparable in terms of magnitude of change and percentage which show an increase. This is especially important, considering that the delay condition represents a replication of the original imagination inflation design. However, these results must be qualified via a limitation in the experimental design.

Although the "imagine" instructions themselves result in a clear division among those who imagine the target items and those who engage in control tasks, the design of the initial target presentation should be clarified. The current materials were adapted from the study conducted by Schooler and Halpern (unpublished data, 2000) in which the target/non-target differences were observed regardless of experimental condition. Although such an adaptation might seem intuitive, it should be considered that the design of that study (and hence, the current one) was such that control participants were in fact primed to engage in some degree of visualization of the target event. Therefore, in the current study, the instructions inform participants that they will be asked to imagine an event, and then prior to those questions designed to function as an initial presentation of the target items, participants are once again asked to imagine the event before answering the questions. This exposure to the word "imagine," as well as subsequent visualization, however brief and while designed to facilitate answers to the questions, created a situation where control participants were not completely manipulation-free. It is very important, however, to note that manipulation checks indicate that control participants provided significantly lower ratings to the question "During the 2-minute waiting period, how much did you try to think about [target event]?". This indicates that the two groups – imagination participants and control participants – were ostensibly operating under different levels of imagination. Although this result is to be expected, it nevertheless is crucial because it serves as a manipulation check and indicates that on the whole, the two groups did differ in the degree to

which they engaged in imagination. However, this allowance for control participants to engage in some imagination might skew the otherwise "clear" results, a possibility which must be entertained.

Although these instructions created an unfortunate scenario in which control participants were encouraged to engage in brief visualization, the main result – that imagine participants do not differ from control participants with regards to target/non-target differences – is still of great value. At the very least, the fact that these two groups did not differ suggests that the extensive and intense imagination hypothesized to be vital for autobiographical memory distortion may not be as necessary as researchers such as Garry and colleagues might argue. This would seem to quell the argument that strong periods of imagination are required to distort autobiographical memories successfully and to increase confidence ratings. The question of course remains, however, as to whether imagination is necessary at all. In order truly to test the effects of repetition against those stemming from imagination, control participants must not be exposed to any instructions making reference to imagination of the target item.

A study is currently underway which has initiated a change to the present design by instead of asking participants to provide written answers during the presentation of each target item, control participants are asked to make a single slash mark along a line (bookended by "definitely did not happen" on one end to "definitely did happen" on the other), indicating whether the particular target event occurred. This serves the purpose of providing the presentation necessary to test the validity effect without giving participants a baseline rating against which to compare their actual Time 2 rating. Then, in the five minutes normally used to provide the written answers, control participants engage in a number of distracting filler tasks, such as crosswords puzzles and logic puzzles. The two minutes normally reserved for the

crossword puzzle are added on to these five minutes, for a total of seven minutes of a filler task. Redesigning the experiment as such not only attempts to replicate the results of interest from the present study, but it also allows for the achievement of this replication in a much more concise and cleaner fashion.

The follow-up to the present study also makes a few other modifications to the present design. The manipulation checks in the current version indicate that the imagine/control manipulations are valid, so they are not included in the follow-up version. Also, in order to approximate more closely the design used in standard imagination inflation studies, and based on the comparable performance of both delay conditions, only the "delayed" condition is used in the follow-up study, in which participants do not receive their Time 2 questions until after all the targets have been presented. This more concise and "clean" version of the experiment allows for a better determination of the extent to which repetition of a target event supercedes any effect imagination might have.

APPENDIX A

Life Events Inventory (LEI)

Please circle the number that best applies to you and whether this happened to you before the age of 10:

1. Got in trouble for calling 911. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

2. Had to go to the emergency room late at night. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

3. Found a $10 bill in a parking lot. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

4. Met and shook hands with a favorite TV character at a theme resort.

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

5. Won a stuffed animal at a carnival game. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

6. Gave someone a haircut. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

7. Had a lifeguard pull you out of the water. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

8. Got stuck in a tree and had to have someone pull you down. *

      1   2   3   4   5   6   7   8
 Definitely did   **Definitely**
    not happen   **did happen**

9.   Broke a window with your hand. *

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

10. Ate grapes from a grocery store before paying for them.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

11. Got lost in a shopping mall.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

12. Got an autograph of a famous ball player at the park.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

12. Had a pet run away from home.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

14. Got food poisoning from the school cafeteria.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

15. Had a parent treat your cold with a home-remedy of antiseptic mouthwash.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

16. Attended a birthday party at the Y.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

17. Went away for summer camp and got pink eye.

	1	2	3	4	5	6	7	8
	Definitely did							**Definitely**
	not happen							**did happen**

18. Hit sibling in the face.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Definitely did							**Definitely**
not happen							**did happen**

19. Broke your arm on the jungle gym.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Definitely did							**Definitely**
not happen							**did happen**

20. Won an award in school.

1	**2**	**3**	**4**	**5**	**6**	**7**	**8**
Definitely did							**Definitely**
not happen							**did happen**

This is only the first part of this study. The second part involves a sixty minute session, to be scheduled and attended at a later date. If selected, you will receive one (1) hour of course credit. The session will involve answering questions about different scenarios of possible past experiences. If you are interested in possibly participating further in this study, please fill out the contact information at the bottom of this sheet. If you do not continue your participation, your information from today will not be kept. If you do continue you participation, at no time will the information collected in this questionnaire be linked with the information below.

Please note: providing contact information does not necessarily imply that you will be contacted.

PLEASE BE SURE TO WRITE LEGIBLY AND CLEARLY.

Name:_____   **Today's Date:**_____

Email:_____

Thanks for your time and participation!

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