The Optimist Within? Selective Sampling and Self-Deception

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## Abstract

The nature and existence of self-deception is controversial. On a classic conception, self-deceived individuals carry two conflicting representations of reality. Proponents of an alternative, deflationary account dispute this, arguing that putative cases of self-deception simply reflect distorted information processing. To investigate these alternatives, we adapted a paradigm from the “crowd-within” literature. Participants provided two different estimates for each of a series of incentivized questions. Half of the questions were neutral in content, while half referred to undesirable future events. Whereas the first and second estimates for neutral questions did not differ systematically, second estimates for undesirable questions were more optimistic than first estimates. This result suggests that participants were sampling selectively from an internal probability distribution when providing estimates for undesirable events, implying they had access to a less rosy representation of their future prospects than their individual estimates conveyed. In short, self-deception is real.

***Keywords:*** Crowd within; optimism bias; self-deception; unrealistic optimism; wisdom of the crowd***.***

Self-deception, the motivated acquisition and retention of a belief in the face of countervailing evidence (Deweese-Boyd, 2012), is a long-debated phenomenon. A classic conception ("real" self-deception; Mijovic-Prelec & Prelec, 2010) construes self-deception as analogous to interpersonal deception: some part of the self actively misleads another part (Gur & Sackeim, 1979; Trivers, 2000). The implication is that self-deceived individuals carry two conflicting representations of reality. Proponents of an alternative, “deflationary” account dispute this, arguing that the interpersonal analogy is misguided and that putative cases of “self-deception” simply reflect distortions in the processing of relevant information (Mele, 1997).

To illustrate, consider the “optimism bias,” the alleged tendency of healthy individuals to underestimate their likelihood of future misfortune (Sharot, 2011; Weinstein, 1980, 1989; cf. Shah, Harris, Bird, Catmur & Hahn, in press). On the classic conception of self-deception, a heavy smoker who believes her future health prospects are good may also harbour a more accurate – and less rosy – belief about this. In contrast, proponents of the deflationary view might argue that whereas this person may be processing evidence about the health implications of smoking in a biased fashion (Sharot, 2011), there is no need to suppose that she carries two conflicting representations of reality.

A potential means of teasing these alternatives apart involves adapting a paradigm from the “crowd-within” literature. Participants in prominent optimism bias experiments estimate their likelihood of undesirable future outcomes (e.g., having a limb amputated) and receive directional feedback pertaining to each of these estimates (i.e., feedback suggesting each estimate was too high or too low) before supplying second estimates (Sharot, 2011; Sharot, Korn, & Dolan, 2011). In contrast, participants in crowd-within experiments supply second estimates for *neutral* questions (e.g., “What percentage of the world’s airports are in the United States of America?”), *without* intervening directional feedback.

The crowd-within effect refers to the fact that the average of the two estimates in these studies has a smaller error than the errors of the individual estimates on average (Herzog & Hertwig, 2009, 2014a; Vul & Pashler, 2008). This effect partly arises through bracketing of the true value, in which one estimate underestimates and the other overestimates the true value (Herzog & Hertwig, 2009). As Larrick and Soll (2006) point out, when there is at least one instance of bracketing, the error of the average estimate is less than the average estimate error. An example can help clarify this. Assume a person is asked to provide two guesses for an item with a true value of 70. If the person guesses 60 and 66 (i.e., does *not* bracket the true value), the error of the average estimate (63) is 7 and the average estimate error (i.e., the average of 10 and 4) is also 7. However, if the person guesses 60 and 84 (i.e., *does* bracket the true value), the error of the average estimate (72) is 2, while the average estimate error (the average of 10 and 14) is 12.

To investigate potential self-deceptive optimism, we adapted this crowd-within paradigm in the present study by having participants supply repeated estimates, without intervening directional feedback, for neutral *and* undesirable questions. We incorporated financial incentives for accuracy.

The different estimates provided in crowd-within studies are thought to be sampled randomly from an internal distribution of potential estimates (Vul & Pashler, 2008). Accordingly, one possibility is that when asked to supply multiple estimates of their probability of experiencing undesirable outcomes, people sample randomly from an internal probability distribution. If so, the second estimate is just as likely to be more optimistic than the first as it is to be less optimistic than the first, irrespective of the underlying distribution's shape (indeed, this is the basis for the distribution-free Wilcoxon signed-rank test; Howell, 2010).

In line with the “real” self-deception approach, however, a second possibility imputes more intentionality to the optimist, who samples *selectively* from the optimistic end of an internal distribution.[[1]](#footnote-1) In this case, the two estimates might vary systematically. On the one hand, participants might sample less selectively second time around, providing a less optimistic estimate and perhaps producing an *enhanced* crowd-within effect through reduction in systematicerror. On the other hand, they might sample even *more* optimistically second time around, perhaps as a kind of defensive maneuver (e.g., Harris & Napper, 2005; Weinstein, 1980). Gal and Rucker (2010) found that individuals induced to experience doubt about their beliefs became stronger advocates of those beliefs than did individuals induced to feel confident of their beliefs, especially when the beliefs were viewed as particularly important. In their experiments, confidence in beliefs was not shaken by presenting evidence that contradicted those beliefs, but via more subtle means (e.g., asking participants to write about their beliefs using their non-dominant hand). We used dialectical instructions in the present study. This entailed telling participants to assume their first estimate was incorrect and asking them to think about reasons why they could have been wrong. This prompt for an alternate estimate to the one already provided might shake confidence in the initial estimate, leading to attempts to bolster one’s position by selecting even more optimistic estimates.

Our primary aim was thus to investigate whether participants provide second estimates for undesirable questions that are more optimistic than their first estimates for such questions, instead of less optimistic or equivalent, and thus to seek evidence for optimistic “self-deception”. In addition, we hypothesized that we would replicate the crowd-within effect for neutral questions and potentially extend this to estimates of undesirable future outcomes.

## Methods

### Participants

Participants were 104 students from Royal Holloway, University of London (RHUL; 41 male, 63 female; mean (SD) age = 20.38 (1.90) years). As several of our research questions were novel, no established effect sizes were available for power analyses. Instead, following an equivalent laboratory study of the crowd-within effect (Herzog & Hertwig, 2009; n=101) we decided to test at least 100 participants. We collected data in five group sessions, each of which 25 participants could sign up to. This way, we were likely to collect at least n=100, even if several registered participants failed to attend each session. Participants received a show-up fee of £3 and a decision-based bonus of between £0 and £2 (mean (SD) = £1.83 (£0.38)). The Psychology Department Ethics Committee of RHUL approved this study.

### Materials

The study included two question types: neutral (eight questions, e.g., “What percentage of the world’s roads are in India?”) and undesirable (eight questions, e.g., “What is the chance that you will die before 90?”). The full list of 16 questions is reported in Table S1 in the Supplemental Online Material. The neutral questions were the eight used in the original crowd-within study (Vul & Pashler, 2008). The undesirable questions were selected from eighty items used by Sharot et al. (2011; see Supplemental Online Material for information about how we selected these items). The required responses to all questions were percentages.

The “true” answers to the undesirable questions were population base rates (derived and calculated from PubMed and the Office for National Statistics; Sharot et al., 2011; mean (SD) = 35.88 (20.55), range = 11-68) and were not significantly different from the answers to the neutral questions (32.50 (23.46), range = 6-72), *t*(14)=.306, *p*=.764, *d*=.164, 95%-CIDIFFERENCE
[-27.021, 20.271], *p*BIC(H0|D)=.791, *p*BIC(H1|D)=.209), so this could not explain systematically different estimates being provided for these two question types.

We incentivized accurate responding for both question types, paying participants in proportion to how close their estimates were to the true values (see the Supplemental Online Material for details). As participants might have had individuating information regarding their own personal vulnerability to certain future misfortunes (e.g., family history of an illness), in addition to providing their *own* estimates we also asked them to estimate the average answers given by other participants in the session to each of the questions. These averages, which we computed in session, enabled us to pay participants based on their accuracy in estimating objectively correct responses for both neutral *and* undesirable questions (we do not report analyses of these other-estimates, as they were included for logistical reasons and do not bear on our research questions). Although we did not deceive our participants, we did not make it explicit that payment was not based on their *own* estimates for undesirable questions – as far as participants were concerned, they were paid based on their accuracy in estimating both question types (which was true), but did not know which particular questions payment was based upon.

### Procedure

The experiment was conducted over five sessions, using z-Tree software (Fischbacher, 2007) in the EconLab at RHUL. Participants provided estimates to the 16 questions in each of four rounds: in round one their own estimates (e.g., “What percentage of the world’s roads are in India?”); in round two their estimates of the average answers given by other participants in the session (e.g., “What is the average estimate for the following question: What percentage of the world’s roads are in India?”); in round three alternative, second own estimates for the exact same questions as in round one; and in round four alternative, second estimates of the others’ average estimates for the exact same questions as in round two. The order in which questions were presented within each round was randomized for each participant; this order remained the same across the four rounds.

Following Herzog and Hertwig (2009, 2014a, 2014b), we used dialectical instructions to elicit estimates as different from one another as the participants deemed reasonable, rather than inducing them to anchor on the first estimate. First own-estimates were elicited with the prompt: “Please enter what you think the answer is from 0% to 100%.” First estimates from the others’ perspective were elicited with the prompt: “Please enter what you think the average answer from this session is from 0% to 100%.” The second estimates were elicited with the following prompt (mentions of average estimates, in square brackets, were only used when eliciting estimates from others’ perspective): “Now, assume your previous estimate [of the average] is incorrect. Think about a few reasons why that could be. Which assumptions and considerations could have been wrong? What do new considerations imply? Was your first estimate too high or too low? Now, based on this new perspective, please enter a second, alternative estimate of what you think the correct answer [for the average estimate] is from 0% to 100%.” The previous estimate was shown, but it was not possible to enter the same estimate as on the first guess to avoid a confound of zero difference between the estimates (White & Antonakis, 2013).

Participants provided written informed consent at the beginning of the session, then when everyone had read the instructions the task began. After completing the four rounds, participants answered demographic questions (gender, age, nationality), check questions (in which participants indicated whether they had previously experienced or were currently experiencing any of the possible undesirable events), and were paid for their participation.

### Analytic strategy

We investigated the crowd-within effect for own estimates by comparing the absolute error of the first own estimate , the absolute error of the second own estimate , and the absolute error of the two own estimates averaged . Because squared errors penalize large errors more heavily than smaller errors, and therefore favor average errors over either of the errors chosen at random (Soll & Larrick, 2009), we did not follow Vul and Pashler (2008) in measuring accuracy through squared errors. Instead, we calculated errors as per White and Antonakis (2013), with the slight alteration of using the mean rather than the median to align with more recent work (Herzog & Hertwig, 2014b):







Here   is the mean absolute difference between participants’ first (second) responses,  , and the true values, , across the eight questions (*i*). These are compared to , which is the mean absolute difference between participants’ averaged first and second responses, , and the true values. Together, these values constituted the three within-subjects levels of estimate-type. We calculated these values separately for both question types. To investigate the crowd-within effect for neutral and for undesirable questions, we conducted a 3 (estimate-type: first estimate versus second estimate versus the average of the two estimates) × 2 (question-type: neutral versus undesirable) repeated-measures analysis of variance (RM ANOVA) on the means of absolute errors.

We also analyzed *signed* (i.e., non-absolute) errors of own estimates to investigate selective sampling. We investigated whether second estimates became more or less optimistic compared with first estimates by performing a 2 (estimate-type: first estimate versus second estimate) × 2 (question-type: neutral versus undesirable) RM ANOVA on the means of log-transformed (see *data screening* sub-section below) signed errors (i.e., observed bias).

We adopted an alpha level of .05 for these analyses. When the assumption of sphericity was violated, we used Greenhouse-Geisser corrections or multivariate tests (if ε<.7 in Mauchly’s test). We report 95% confidence intervals (95%-CIs) on the differences between untransformed means (Cumming, 2014). We also report probabilities for the null and alternative hypotheses in each case, based on Bayes factors, as classic null-hypothesis significance testing (NHST) does not provide a quantification of the extent to which the data supports each hypothesis (Masson, 2011).

## Results

### Data screening

Kolmogorov-Smirnov tests were used to check for normality, but as these tests tend to detect even trivial deviations in larger samples (Field, 2013), an α-level of .01 was adopted. The tests indicated that signed errors for undesirable items were not normally distributed for the first estimate (*p*=.004) and for the second estimate (*p*=.005). To correct for positive skew, all signed errors were transformed by first adding a constant to make all values positive and then taking the logarithm (i.e., log(*x*+31), where *x* was the original score) as advised by Field (2013).After this transformation, all variables were normally distributed. Analyses included all participants (n=104). However, seventeen participants endorsed at least one check question. Therefore, we also conducted analyses with a subsample of n=87, consisting only of participants who had not experienced (or were not currently experiencing) any of the undesirable events. These analyses did not lead to different results than those obtained with n=104, and thus only the latter are reported. Reported means and standard errors (and our graphical display) are based on non-transformed data to facilitate interpretation.

### Crowd-within effects

We found an overall crowd-within effect, in that the average of the two estimates was more accurate than either individual estimate on average. This was clarified through follow-up analyses of a main effect of estimate type (Wilks’ Lambda=.446, *F*(2,102)=63.395, *p*<.001, ηp²=.554, *p*BIC(H0|D)<.001, *p*BIC(H1|D)>.999). Across questions and participants, the absolute error of the two estimates averaged (mean (SE) = 17.14 (.3)) was lower than absolute errors of first estimates (18.37 (.4); *p*<.001, ηp²=.359, 95%-CI [.833, 1.617], *p*BIC(H0|D)<.001, *p*BIC(H1|D)>.999) and absolute errors of second estimates (17.89 (.4); *p*<.001, ηp²=.133, 95%-CI [.289, 1.203], *p*BIC(H0|D)=.006, *p*BIC(H1|D)=.994). The absolute errors of the first and second estimates did not differ (*p*=.334, ηp²=.024, 95%-CI [-.247, 1.206], *p*BIC(H0|D)=.738, *p*BIC(H1|D)=.262). This crowd-within effect was similar for both neutral and undesirable questions, as there was no interaction with question type (Wilks’ lambda=.991, *F*(2,102)=.464, *p*=.630, ηp²=.009, *p*BIC(H0|D)=.914, *p*BIC(H1|D)=.086). There was no support for a main difference between the question types, with similar absolute errors for undesirable questions (18.37 (.4)) and neutral questions (17.23 (.5); *F*(1,103)=3.636, *p*=.059, ηp²=.034, 95%-CI [-.046, 2.343], *p*BIC(H0|D)=.627, *p*BIC(H1|D)=.373).

Although we found low bracketing rates, bracketing did occur for at least one neutral item for 84.6% of participants and for at least one undesirable item for 76% of participants. This explains the crowd-within effect we found (Larrick & Soll, 2006). Bracketing rates were similar for neutral questions (mean (SD) number of items bracketed = 1.90 (.1)) and undesirable questions (1.63 (.1); *t*(103)=1.611, *p*=.110, d=.317, 95%-CI [-.064,.622], *p*BIC(H0|D)=.737, *p*BIC(H1|D)=.263).

### Selective sampling effects

We found evidence of selective sampling for undesirable questions, but not for neutral questions, as the first and second estimates differed systematically for the former question type but not for the latter (see Fig. 1). First, and most importantly, we found an interaction between estimate-type (first versus second) and question type (*F*(1,103)=8.604, *p*=.004, ηp²=.077, *p*BIC(H0|D)=.135, *p*BIC(H1|D)=.865). For neutral questions, the second estimates (mean (SD) signed error = 2.04 (1.0)) were not different from the first estimates (3.02 (1.0); *p*=.165, ηp²=.019, 95%-CI [-.088, 2.056], *p*BIC(H0|D)=.793, *p*BIC(H1|D)=.207). For undesirable questions, the second estimates (-5.94 (1.1)) were lower than first estimates (-3.60 (1.0); *p*=.001, ηp²=.107, 95%-CI [.972, 3.709], *p*BIC(H0|D)=.028, *p*BIC(H1|D)=.972). Across the two estimates, answers to undesirable questions were lower (-4.77 (1.0)) than answers to neutral questions (2.53 (1.0)), as indicated by a significant main effect of question type (*F*(1,103)=43.993, *p*<.001, ηp²=.299, 95%-CI [4.950, 9.645], *p*BIC(H0|D)<.001, *p*BIC(H1|D)>.999). Across the two question types, second estimates were lower (-1.95 (.8)) than first estimates (-.29 (.8)), as indicated by a main effect of estimate type (*F*(1,103)=11.468, *p*=.001, ηp²=.100, 95%-CI [.654, 2.671], *p*BIC(H0|D)=.041, *p*BIC(H1|D)=.959).



Fig. 1.

The mean biases of the first and second estimates (circles and left vertical axis in each panel) and the differences between these estimates (triangles and right vertical axis in each panel), with 95% confidence intervals, for neutral questions (panel a) and undesirable questions (panel b).

## Discussion

The wisdom-of-the-crowd effect refers to the fact that a group’s answers to questions involving quantity estimation (e.g., the weight of an Ox; Galton, 1907) or general knowledge (as exploited in the game show 'Who Wants To Be A Millionaire?'; Surowiecki, 2005) are better than the answers of the individuals in the group. Remarkably, this interpersonal phenomenon has also been found to apply *intra*personally: the average of a single person’s repeated estimates of answers to neutral questions such as “What percentage of the world’s roads are in India?” has a lower error than that of the person’s individual estimates on average (e.g., Herzog & Hertwig, 2009, 2014a, 2014b; Vul & Pashler, 2008). Here we replicated this “crowd-within” effect for neutral questions, and extended this effect to repeated estimates of participants’ likelihood of experiencing undesirable outcomes (e.g., dying before they are 90): the error of two estimates averaged was lower, on average, than the individual errors of either estimate. This effect was similar for both neutral and undesirable questions.

A key aim in adapting the “crowd-within” paradigm by including estimates of undesirable future outcomes was to investigate any change across the two estimates provided for such questions. Importantly, we found that whereas the first and second estimates for neutral questions did not differ, the second estimates for undesirable questions were more optimistic than the first estimates for such questions. The significance of this result is that it suggests participants were sampling *selectively* from an internal probability distribution for these questions. Whatever the shape of the underlying distribution, if they had been sampling randomly on both occasions (as per Vul & Pashler, 2008), participants would have been just as likely to provide a more optimistic second estimate as a less optimistic second estimate, and no systematic difference would have emerged across estimates. Our results imply participants carried a less rosy representation of their future prospects than their individual estimates (at least their *second* estimates) conveyed.

Our results need to be replicated to establish the robustness and boundary conditions of the effects we report here. For example, following previous investigations of the optimism bias (Sharot, 2011; Sharot et al., 2011; Weinstein, 1989), our participants estimated their likelihood of experiencing undesirable future outcomes. Future studies could investigate whether second estimates pertaining to *desirable* future outcomes are more optimistic than first estimates, as this would support the notion of self-deceptive selective sampling. In addition, future studies could investigate whether further estimates (third, fourth etc.) would reveal more extreme selective sampling or if the two first estimates set the bounds within which any further estimates are made. This latter option might be more likely given results from crowd-within studies with more than two (modeled) estimates (Rauhut & Lorenz, 2011; Vul & Pashler, 2008).

The crowd-within phenomenon is an intrapersonal analogue of the classic wisdom-of-the-crowd effect. Likewise, proponents of “real” self-deception (e.g., Mijovic-Prelec & Prelec, 2010) construe the process whereby individuals acquire and maintain unwarranted positive beliefs as “an intrapersonal analog of ordinary interpersonal deception” (Mele, 1997, p. 91). Just as one person might seek to mislead another by cherry picking relevant data (e.g., in a job interview, a candidate might showcase select examples of their previous employment performance rather than choosing examples at random), our results indicate that people mislead *themselves* by judiciously selecting estimates of their future prospects. Likewise, just as people who exaggerate their prospects may intensify their claims when challenged, potentially “protesting too much”, our findings suggest the same defensive strategy may operate intrapersonally (McKay, Mijović-Prelec, & Prelec, 2011).

Although our results are consistent with the “real” self-deception account, one might argue that the optimistic estimates our participants provided did not convey their actual beliefs, but were distorted for impression formation purposes. In this case the optimistic estimates would not have been merely *analogous* to interpersonal deception, but provided with the express purpose of deceiving others (e.g., the experimenters). Against this possibility, we note that participants in our study provided their estimates under conditions of strict anonymity (participants sat in individual cubicles and were only identifiable by number).

The opportunity to provide a second estimate for undesirable questions did not attenuate optimism in our study – on the contrary, second estimates for undesirable questions were more optimistic than first estimates. Nevertheless, the fact that the error of both estimates averaged was lower than the error of either individual estimate indicates that individuals could improve the accuracy of their optimistic forecasts by second-guessing themselves and taking the average of the two guesses. Given the potential drawbacks of excessive optimism, which has been implicated in global catastrophes such as world wars, financial crises and environmental disasters (Johnson & Fowler, 2011; Sevincer, Wagner, Kalvelage, & Oettingen, 2014; Sharot, 2011; Ubel, 2009), we may have much to gain as a society from such a practice. We leave it to others to estimate the likelihood that this advice will be taken up and applied.

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1. The “real” self-deception account predicts selective sampling from an internal distribution, but is agnostic as to whether that distribution is itself biased (e.g., an outcome of biased information encoding; Sharot, 2011; Sharot et al., 2011). [↑](#footnote-ref-1)