Cognitive Ability Does Not and Cannot Correlate with Susceptibility to Anchoring Effects

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This is a preprint. Changes since the version 3 include a meta-analysis of 15 effects across our study and four other studies, an overview of all reported reliabilities of anchoring tasks in which we also included a study that was published this year, and a few minor corrections.

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Abstract

On a scale from 1 to 10, would you consider anchoring effects to be more or less important than a rating of 9? The seminal studies on anchoring effects are still surrounded by many mysteries regarding the question of how these effects come about. One of the mysteries is the influence of a person's cognitive ability. Some studies found that cognitive abilities moderated participants' susceptibility to anchoring, but others did not. We aimed to resolve these inconsistencies by making a distinction between informative and uninformative (e.g., random) anchors. In a preregistered online experiment, we tested the hypothesis that anchoring effects are weaker for people with higher cognitive abilities but that this relation only occurs if the anchors are perceived as random, and it vanishes if they are presented as informative. We found no support for the hypothesis. Results from a meta-analysis of 15 effects across our study and four other studies revealed no overall effect of cognitive ability on the susceptibility to anchoring, $g = 0.003$, 95% CI $[-0.031, 0.037]$, $N_{\text{total}} = 1165$. Moreover, we observed that 10 items across three typical anchoring tasks had only very low internal consistency ($\alpha = .11$). Our analysis of additional published and unpublished data confirmed that a person's susceptibility to anchoring cannot be measured reliably. This explains why previous results on possible moderators of anchoring (e.g., studies on the Big Five or on fluid intelligence) have been highly inconsistent. We suggest that research on anchoring moderators needs to take a step back and develop a reliable measure for the susceptibility to anchoring.

Keywords: anchoring effect, cognitive ability, random anchors, reliability, meta-analysis

Cognitive Ability Does Not and Cannot Correlate with Susceptibility to Anchoring Effects

If people consider an evidently arbitrary numeric value before completing an estimation task, their estimate will usually be closer to that numeric value than when such a prime is not given. The phenomenon has been termed the *anchoring effect*.

Anchoring effects are well-known in decision making research. However, they are still not well understood, and numerous competing theories (e.g., Frederick & Mochon, 2012; Mussweiler & Strack, 1999) are aimed at explaining the phenomenon. The theories are nonexclusive, and none are sufficient for explaining the wide range of anchoring phenomena that have been observed over the last four decades. Moreover, replication issues have recently arisen for key findings from the Selective Accessibility Model (Harris et al., 2019) and the Numeric Priming account of anchoring (Klein et al., 2018).

Some studies have investigated the question of whether and how anchoring effects are associated with personality traits. There were some initial findings that showed moderating effects of conscientiousness, agreeableness, extraversion (e.g., Eroglu & Croxton, 2010), and openness to experience (e.g., McElroy & Dowd, 2007), but these could not be replicated (Furnham et al., 2012; for an overview, see also Cheek & Norem, 2019). Various studies have investigated cognitive ability as a possible moderator of susceptibility to anchoring. One study found that cognitive ability did indeed moderate susceptibility to anchoring (Bergman et al., 2010), whereas others found no evidence of such a moderating effect (Furnham et al., 2012; Stanovich & West, 2008; Teovanović, 2019). Briefly, findings on various moderators of anchoring effects have been inconsistent and contradictory, but the reason for these inconsistencies is still unknown.

Specifically for cognitive abilities, their effect on anchoring is inconclusive. Thus, we aimed to resolve the inconsistencies by embedding existing findings into Stanovich and West's (2008) framework, which conceptualizes individual differences in the operation of heuristics and thinking biases.

When does Cognitive Ability Influence Anchoring Effects?

Anchoring effects refer to the observation that numerical estimates are assimilated toward a previously presented value. We use the term in its plural form because of the large variety of paradigms that are used. Anchoring effects have been shown to be robust and to involve multiple nonexclusive mechanisms, such as insufficient adjustment and priming (e.g., Simmons et al., 2010; Chaxel, 2014) and scale distortion (Frederick & Mochon, 2012). Yet, individual differences in the susceptibility to anchoring effects are not clear, and with respect to the role of cognitive abilities, the findings are rather heterogeneous. Basing our work on Stanovich and West's (2008) framework, we show that additional factors need to be distinguished such as whether anchors are perceived as plausible estimates with respect to the issue at hand. In this framework, there are several conditions that must be met for cognitive ability to moderate whether there will be a heuristic response (i.e., strong anchoring effect) or a deliberate response (i.e., no anchoring effect). We argue that the crucial parameter is whether participants perceive that there is a need to override their initial heuristic response. We hypothesize that when people are confronted with anchors that are obviously useless with respect to the estimate, they know they need to ignore that initial anchor to provide an accurate estimate. In this case, cognitive ability may help them free their reasoning from the anchor. However, when people interpret the anchor as a useful hint, there is no need to override the initial response. In this case, cognitive ability should not make a difference. Past findings differ with respect to whether an effect of

cognitive ability on the susceptibility to anchoring effects was found (Bergman et al., 2010) or not (Stanovich & West, 2008). They also differ with respect to whether the anchor could be interpreted as informative (e.g., it was provided by the experimenter) or not (e.g., it was presented as a randomly generated number). In our study, we manipulated whether anchors were informative or random and expected cognitive ability to moderate the susceptibility to anchoring effects only when anchors were informative.

Method

Our hypothesis, method, and plan for data analysis were rigorously preregistered (<https://osf.io/n8d6v/>). We report how we determined our sample size, all data exclusions, all manipulations, and all measures (Simmons et al., 2012). We deviated from our preregistration in that we recruited more participants due to an initial lack of participants. This deviation did not affect the results. We manipulated whether the anchor that participants received was presented as a random number or as an informative suggestion. Our manipulation check consisted of two items that asked whether the number that was presented (i.e., the anchor) was determined randomly and whether participants tried not to let the number influence their estimate. The question about whether participants let the number influence their estimate was used for exploratory purposes to investigate whether participants readjusted their values consciously after they were presented with the anchor.

Participants

Data were collected online via social networks; participants were rewarded with feedback on their intelligence test. Besides the several types of media we planned to use (e-mail: via a student newsletter, university mailing lists, and social networks), we used additional sources to

achieve the sample size we aimed to reach. We posted our study on the web page of an online psychology journal and contacted trainee nurses via e-mail.

We had planned to recruit 174 participants up to January 31, 2018. Due to an initially low response rate, we extended the recruitment period and oversampled. The sample consisted of 225 complete cases (69.8% women) with a mean age of 26.54 years ($SD = 8.62$). This raised the power from .80 to .89. The results of the hypothesis test were the same no matter whether we used the full sample of 225 or only the first 174 participants. Participants were excluded from the analysis if at least one of the following preregistered criteria was met: (a) missing values, (b) overly consistent responses, (c) estimated celebrity ages in the age estimate task below 0 or above 100 on the age estimation task, (d) price estimates below 0€ or 0 cents and above 100€ or 1000 cents on the willingness to pay task, and (e) age above 100 years or below 18 years. The dropout rate was 724/949 clicks (76% drop out) of which most were excluded because they quit the study early.

Manipulations

Between-subject manipulations. The relevance of the anchor was determined by random computerized assignment to one of two groups: In the random anchor group, participants clicked on a box to generate a random anchor before each trial, whereas in the informative anchor group, participants were immediately presented with the anchor, which was described as a suggestion.

Within-subject manipulations. The anchor tasks consisted of three blocks of within-subject anchoring tasks with the following blocks and manipulations.

Quantities. The estimates of quantities featured six matrices with black or white boxes. They were created by a 2 (high vs. low anchor) $\times$ 3 (true value is 40, 50, or 60) complete within-

subject design. The assignment of a high or low anchor to the stimulus was completely randomized. High and low anchors were the true value plus and minus 30 (e.g., for the two matrices with 40/100 black squares, the anchors were 10 and 70). As this task involved perception rather than thinking, the influence of cognitive ability on susceptibility to anchoring could generally be lower than in other tasks as no actual reasoning was involved in the process of disregarding the anchor.

Prices. The estimates of prices (i.e., participants' *willingness to pay*; Bergman et al., 2010, p. 66) featured six products that were also created by a 2 (high vs. low anchor) $\times$ 3 (product: keyboard, apple, pen) design. The two keyboards, apples, and pens were chosen to visually resemble each other. Using a pilot test, we had made sure that they were perceived as equally valuable. The high and low anchors were (in the same order as the products) 20€ versus 10€, 80ct versus 20ct, and 90ct versus 50ct, respectively.

Age. The age estimation task was chosen as a general knowledge test and featured eight people who were also chosen according to a 2 (high vs. low) $\times$ 4 (age of the person) design. The respective people were pairs of celebrities who were actually the same age. To determine the anchors, their age was rounded to the nearest 10, and then 10 years were added (high anchor) or subtracted (low anchor). The people we chose were: Joschka Fischer (German politician), Al Gore (U.S. politician), Sandra Bullock (actress), Russel Crowe (actor), Jessica Alba (actress), Fernando Alonso (Formula 1 driver), Lionel Messi (soccer player), and Sebastian Vettel (Formula 1 driver). Using a pilot test, we had made sure that the age estimates for the matched celebrities did not differ and that people were usually familiar with them but did not know their actual age. On the list, two consecutive people had the same age at the time of the experiment. They were presented with their name and profession.

Measures and Materials

Cognitive ability was measured with the Hagen Matrices Test (HMT; Jäger & Heydasch, 2016). Self-control was measured with a German version of the Self-Control Scale (Sproesser et al., 2011; Tangney et al., 2004). Self-esteem was measured with the German version of Rosenberg's Self-Esteem Scale (Collani & Herzberg, 2003). Susceptibility to anchoring effects was measured with participants' reactions to the tasks described above. We chose to include a new task (the quantity estimates task) because it has several advantages over commonly used anchoring tasks: (a) the existence of maximum, minimum, and true values (which are unknown to participants); (b) the anchors can be used symmetrically on two consecutive stimuli with the same true value (e.g., for a true value of 50, the anchors are 50 ± 30); (c) The repeated-measures manipulation helps to test for the reliability of susceptibility scores and provides high power; (d) the random and difficult-to-remember stimuli allow the tasks to be repeated. For example, if we asked about the boiling point on Mt. Everest (e.g., Epley & Gilovich, 2001), people who did not know the answer might look it up after the experiment and know that it is 71°C (160 °F). This approach addresses a big problem of tasks such as the cognitive reflection task (Frederick, 2005), which is that the measure's construct validity deteriorates when participants know the answer or look it up (Toplak et al., 2013). The repeated-measures design allowed us to examine whether susceptibility to anchoring effects is robust across multiple anchoring tasks, and we could thus estimate its reliability.

Procedure

First, participants answered demographic questions (i.e., sex, age) and provided a personal code for potential subsequent experiments (i.e., to assess the retest reliability of the susceptibility-to-anchors score). Then they completed a brief version of the Self-Control Scale

(German translation) and the Rosenberg Self-Esteem Scale. Next, participants were randomly assigned to one of two experimental conditions. In each condition, the participants performed three anchoring tasks: (a) a quantity estimation task, (b) an age estimation task, and (c) a willingness to pay task. Half of the participants were instructed that they would be presented with a random number. The numbers 10, 20, ..., 90 were presented for 50 ms in a random order. Participants pressed a “generate random number” button and were then given the anchor. The other half of the group was given a number that the experimenter proposed to them.

For the quantity estimation task, the participants were instructed that they would see six 10x10 matrices with black and white squares for 1 s each. They were asked to estimate the number of black squares after each presentation. The task was followed by the age estimation task and the purchase decision.

Then, participants were asked whether they thought that they were influenced by the number they had been given and whether they thought the number was determined randomly (manipulation check). For exploratory purposes, we asked whether they had tried to suppress thoughts of the number, whether they had their estimate in their minds before they were presented with the number, and whether they had heard about the anchoring effect.

Then, participants conducted the HMT (test of fluid intelligence). The test was presented at the end due to technical restrictions. We believe that the measurement time should not be relevant as intelligence is considered a trait, and performance should be independent of the tasks conducted beforehand.

Results

Reliability of Measures

The internal consistency of the measure of cognitive ability (HMT) was $\alpha = .75$ (20 items, $N = 225$). Internal consistency was excellent for the Self-Control Scale ($\alpha = .84$, 13 items) and the Rosenberg Self-Esteem Scale ($\alpha = .91$, 10 items). The general tendency to be susceptible to anchoring was measured on the basis of the correlations between the three tasks. The results showed very low correlations between the three task scores ($\alpha = .13$, $\alpha = .11$ for all 10 items; see **Fehler! Verweisquelle konnte nicht gefunden werden.**). The probability that 3 of 45 correlations would be significant given H_0 is $p = .492$.

Table 1

Correlations between Susceptibility to Anchoring Scores

Score	1	2	3	4	5	6	7	8	9	10
1 Quantity 50	1	.185**	.052	-.088	.054	.053	.136*	-.044	-.101	.061
2 Quantity 40	.185**	1	.087	.058	.091	.051	-.038	.029	.022	.024
3 Quantity 60	.052	.087	1	.111	.076	-.028	.028	-.048	-.026	.025
4 Age 69	-.088	.058	.111	1	-.078	.008	.093	-.002	-.007	.010
5 Age 53	.054	.091	.076	-.078	1	.054	-.114	-.110	.049	-.001
6 Age 36	.053	.051	-.028	.008	.054	1	.059	.020	.125	-.042
7 Age 30	.136*	-.038	.028	.093	-.114	.059	1	.057	.158*	-.002
8 WTP pen	-.044	.029	-.048	-.002	-.110	.020	.057	1	.101	-.007
9 WTP apple	-.101	.022	-.026	-.007	.049	.125	.158*	.101	1	-.003
10 WTP keyboard	.061	.024	.025	.010	-.001	-.042	-.002	-.007	-.003	1

Note. $N = 225$.

* $p < .05$ (two-tailed). ** $p < .01$ (two-tailed).

Manipulation Checks and Data Quality Assessment

Randomness. Participants who had “generated” their random anchor by clicking a “stop” button next to a fluctuating number (random anchor condition) perceived the anchor as more random ($M = 2.93$, $SD = 1.22$, $N = 104$) than participants who were presented with the anchor by

the experimenter (informative anchor condition; $M = 2.40$, $SD = 1.16$, $N = 121$). The manipulation check was significant with a medium-sized effect, $t(223) = 3.38$, $p < .001$, $d = 0.45$.

Exploratory items (not preregistered). Participants in the random anchor condition felt that the anchor exerted a smaller influence on them than participants in the informative anchor condition ($M_{random\ anchor} = 2.88$, $SD = 1.07$, $N = 104$; $M_{informative\ anchor} = 3.02$, $SD = 1.08$, $N = 121$). However, this difference was not statistically significant, $t(223) = -0.98$, $p = .330$, $d = 0.13$. There was no statistically significant difference between the two experimental conditions concerning the extent to which participants tried not to let themselves be influenced by the anchor ($p = .522$). Moreover, styles of estimating differed between groups. Participants in the informative anchor condition more often reported that they had thought of a number before they saw the anchor ($M_{random\ anchor} = 2.24$, $SD = 1.22$, $N = 104$; $M_{informative\ anchor} = 2.88$, $SD = 1.17$, $N = 121$). The effect was medium in size, $t(223) = -3.99$, $p < .001$, $d = 0.54$.

Strength of anchoring effects in general (exploratory preliminary analyses). All but two out of 10 items showed significant anchoring effects, namely, the price estimations for the pen ($p = .243$; one-sided test) and the keyboard ($p = .090$; one-sided test). The mean effect size for the anchoring effects was $dz = .26$ ($dz = .31$ when the nonsignificant items were excluded). For an overview of the anchoring effects, see **Fehler! Verweisquelle konnte nicht gefunden**

werden..

Figure 1. Estimates per task and anchor reveal anchoring effects for all but two items. Error bars indicate 95% confidence intervals.

Calculation of Scores

Cognitive ability was measured as the number of tasks solved on the HMT (i.e., 0 to 20). Susceptibility to anchoring effects was computed differently for the three tasks. In the quantity estimation tasks, we computed the z-value for the difference between the mean of the high anchor estimates and the mean of the low anchor estimates. In the willingness to pay task (price estimation), we computed the standardized mean values (i.e., z-values) for the difference between the high anchor price estimate and the low anchor price estimates. Note that the computation was different for the products because the ranges of the anchors were not constant (unlike in the other tasks). In the age estimation task, we used the standardized mean values (i.e., z-values) for the difference between the high anchor estimates and the low anchor estimates.

To test our hypothesis, we used a MANOVA with a complete design. The dependent variables were the susceptibility to anchoring effects scores for the three tasks (quantity estimation task, age estimation task, and willingness to pay task). The independent variables were cognitive ability and the relevance of the anchor. We expected an interaction between cognitive ability and the relevance of the anchor with an effect size of $f = 0.214$ or $\eta^2 = .043$. We did not expect any of the other effects in the model to be significant.

Hypothesis Test

Contrary to our preregistered hypothesis, the interaction between cognitive ability and the relevance of the anchor had no effect on susceptibility to anchoring effects, $F(1, 221) = 1.37, p = .243, \eta_p^2 = .006$. Instead, there were interactions between the task and cognitive ability, $F(2, 442) = 4.84, p = .008, \eta_p^2 = .021$, as well as between the task and the relevance of the anchor, $F(2, 442) = 3.33, p = .037, \eta_p^2 = .015$. Furthermore, the main effect of relevance was significant, $F(1,$

221) = 4.66, $p = .032$, $\eta_p^2 = .021$. All other effects were not significant. All analyses reported hereafter were exploratory and were not preregistered.

As recommended by Cheek and Norem (2018), we conducted the same analysis as above using the absolute differences between anchors and estimates as the strength of the anchoring effect. This did not change the results. None of the effects described in the model above were significant except for the anchor item (i.e., absolute differences were larger for some items than for others). The internal consistency of all 20 items (two times the 10 items described above because each was presented with a low and high anchor) was slightly larger but still far from acceptable, $\alpha = .385$. There was no correlation between cognitive ability and the mean of all absolute differences, $r(223) = -.020$, $p = .761$.

Post hoc analyses (i.e., not preregistered analyses) revealed that cognitive ability was more strongly correlated with susceptibility to anchoring effects for the score on the quantity estimation task, $r(223) = -.24$, $p < .001$, than for the scores on the age estimation task, $r(223) = -.05$, $p = .459$, or the willingness to pay task, $r(223) = .06$, $p = .399$. We conducted two-tailed z -tests between the three correlations indicating that only the differences between the quantity task and the other two tasks were significant ($z_{quantity\ vs.\ age} = -2.06$, $p = .039$, $z_{quantity\ vs.\ wtp} = -3.18$, $p = .002$, $z_{age\ vs.\ wtp} = -1.12$, $p = .263$). Note that this analysis was not part of our hypothesis and that when we applied a Bonferroni correction to protect against making a Type 1 error, the first difference became nonsignificant.

As our score for a low susceptibility to anchoring effects was sensitive to accurate estimates of the stimuli, we checked whether cognitive ability was correlated with accuracy to eventually control for it. For the accuracy score, we computed the sum of absolute deviations from the actual value. High cognitive ability was correlated with a low absolute deviation from

the true value (i.e., the true number of black boxes), $r(225) = -.17, p = .009$. As the relationship between a low susceptibility to anchoring effects and cognitive ability could be triggered by the accuracy effect, we computed the partial correlation between cognitive ability and susceptibility to anchoring effects while controlling for accuracy and found that the correlation barely changed, $r(222) = -.22, p < .001$. The main effect of the relevance of the anchor hinted that the source was important after all. Informative anchors are stronger than overall anchors, $t(223) = 2.75, p = .006, d = 0.37$.

Self-control and self-esteem. To explore whether personality traits such as self-control and self-esteem have an impact on people's susceptibility to anchoring effects, we computed correlations for participants' self-control and self-esteem with their susceptibility to anchoring effects. Neither variable was significantly correlated with susceptibility: self-control, $r(223) = -.02, p = .753$; self-esteem, $r(223) = .03, p = .627$.

Knowledge of the anchoring effect. People who were familiar with the anchoring effect were less affected by the anchors ($M = -.22, SD = .61, N = 65$) than people who were not familiar with it ($M = .09, SD = .59, N = 165$), $t(224) = 2.99, p = .003$. Note that the two groups did not differ in their cognitive abilities ($M_{yes} = 8.82, SD = 3.84, N = 65$; $M_{no} = 8.08, SD = 3.41, N = 160$), $t(223) = 1.41, p = .160, d = 0.20$. More specifically, absolute differences between the high anchor and low anchor estimates for each of the pairs of stimuli were all lower for people who knew of the anchoring effect. However, only one of the pairwise comparisons was significant ($p = .049$).

Discussion

In this research, we applied Stanovich and West's (2008) framework, which conceptualizes individual differences in the operation of heuristics and thinking biases, to

people's susceptibility to anchoring effects with the aim of explaining why different studies have found different results concerning the relation between cognitive ability and susceptibility to anchoring effects. We aimed to test the relevance of anchors (presenting them as either random or informative) as a crucial variable that might determine whether cognitive ability does or does not weaken the influence of anchors.

One of our primary goals was to explain why Bergman et al. (2010) found a moderating effect of cognitive ability in their experiment, whereas in the experiments conducted by Stanovich and West (2008) and Furnham et al. (2012), no moderating effects were found. However, we are still not able to provide an explanation on the basis of our results. Instead, our results add to the contradictions and inconsistencies: We did not find a moderating effect on the general knowledge (age estimation) task, thus contradicting Stanovich and West (2008) and Furnham et al.'s (2012) results. We also did not find an effect on the willingness to pay task, thus contradicting Bergman et al.'s (2010) results. The most likely explanation for our findings is that susceptibility to anchoring effects cannot be measured reliably. Although the phenomenon of anchoring is said to be robust—and sometimes even termed *reliable*—there is apparently yet no way to measure susceptibility to anchoring *reliably* on a participant level.

Meta-Analysis of the Effect of Cognitive Ability on Susceptibility to Anchoring

Fehler! Verweisquelle konnte nicht gefunden werden. provides an overview of all experiments involving cognitive ability and anchoring effects. Apart from the presentation of the stimulus, there is currently no single factor that can explain the presence or absence of a correlation between cognitive ability and the susceptibility to anchoring effects. Given the low reliability, the two positive effects are puzzling.

Note that Bergman et al. (2010) reported a p-value of $p = .043$ (p. 67) and Teovanović (2019) who only found an effect after splitting the sample into high and low scores based on presumptions about subjects' reflectivity, their score in the cognitive reflection task ($p = .003$, p. 16). To determine whether the overall effect is homogeneous and significantly larger than zero, we conducted a meta-analysis. Due to insufficient reports, we had to assume $F = 1$ for cases in which $F < 1$ was reported (Furnham et al., 2012). A multilevel random effects meta-analysis revealed no significant effect of all 15 effects across the 5 studies, $t(14) = 0.21$, $p = .833$, $g = 0.003$, 95% CI $[-0.031, 0.037]$, $N_{\text{total}} = [1165, 1178]$. Effects were homogeneous, $Q(14) = 3.33$, $p = .998$ (data and code are available at <https://osf.io/qdy3p/>). See *Figure 2* for a forest plot of the results.

Table 2

Designs and Results for all Studies Involving the Susceptibility to Anchoring Effects and Cognitive Ability

Variable	Bergman et al., 2010	Stanovich & West, 2008	Furnham, Boo, & McClelland, 2012	Teovanovic, 2019	Quantity Estimation	Age estimation	Willingness to Pay Estimation
Task	Willingness to pay with "real decisions"	General knowledge	General knowledge	General knowledge	Quantity estimation	General knowledge	Willingness to pay with hypothetical decisions
Source of the anchor	Social Security Number	Determined by experimenter	Determined by experimenter	Determined by experimenter	Random or determined by experimenter	Random or determined by experimenter	Random or determined by experimenter
Cognitive ability measure	Predicting Job Performance [mainly fluid intelligence]	SAT score [crystallized intelligence]	Baddeley Reasoning Test (1968) [fluid intelligence]	Raven's matrices (1979) [fluid intelligence]	HMT [fluid intelligence]	HMT [fluid intelligence]	HMT [fluid intelligence]
Calculation of susceptibility to anchoring scores	None	None	Logarithmic transformation	Difference between no-anchor estimate and anchor estimate	Standardized by item and task	Standardized by item and task	Standardized by item and task
Method	Correlation	Interaction in a linear model	Interaction in a linear model	Correlation	Correlation	Correlation	Correlation
Participants' country of origin	Sweden	USA	USA	Serbia	Germany	Germany	Germany
High anchors	[00, 99]	65, 1000	68, 77, 113, 187	Computed individually	80, 70, 90	80, 60, 50, 40	90, 80, 20
Low anchors	[00, 99]	12, 85	22, 17, 49, 24		20, 10, 30	60, 40, 30, 20	50, 20, 10
Mean anchoring effect size [standardized r]	.369	.581, .721	.457	[.206, .242]	.207	.289	.083
Kind of stimulus	Presented and disguised OR visible during estimation	Remembered from LTM	Remembered from LTM	Remembered from LTM	Presented, then disguised	Remembered from LTM	Visible during estimation
Sample size	116	434	172	236	225	225	225
Design of factor "anchor"	Between subjects	Between subjects	Between subjects	Within subjects	Within subjects	Within subjects	Within subjects

Figure 2. Forest plot and prediction interval for all effects of cognitive ability on strength of anchoring effects.

Low Reliability of the Susceptibility to Anchoring Effects in Other Data Sets

To even more thoroughly test our claim about the lack of reliability in the anchoring effects measures as an explanation of no correlations and inconsistent results, we computed the correlations between different anchoring tasks in other data sets. We analyzed some previous data from our own lab that involved anchoring effects in general knowledge questions with a sample of 650 participants. We were able to confirm the low reliability ($\alpha = .05$, 16 items).

We further analyzed the data provided by Cheek and Norem (2019; <https://osf.io/a6ywr/>), which had been used to test for interactions between the Big Five and the susceptibility to anchoring effects. We computed the absolute differences between the anchors, standardized them, excluded outliers as they did (± 5 SD), and again found an unacceptable internal

consistency (six items) of $\alpha = .14$ and a maximum correlation between the two susceptibility to anchoring effects scores of $r = .083$.

Another data set was provided by Bergman et al. (2010), who found an interaction between susceptibility to anchoring effects and cognitive ability. The anchor in this case was the last two digits of participants' social security numbers and was used for six product price estimates. As in the other data sets, we computed the absolute differences between the anchor and the price estimates and found an internal consistency (six items) of $\alpha = .69$. This was considerably higher than in the other data sets. Given that the anchor was the same for all six items, and all six tasks were actual price estimates (nonhypothetical decisions), the shared variance between the items may, however, also reflect a generally higher willingness to pay. It is possible that the measure used by Bergman et al. (2010) is not as a measure of susceptibility to anchoring effects but is at least partly a measure of willingness to pay. This would make their significant correlations between cognitive ability and the product price estimates difficult to interpret.

Similarly to Bergman et al. (2010), Frech et al. (2020) also used price estimates. Additionally, they only used high anchors and measured stepwise adjustment from those anchors, thus confounding susceptibility to anchoring with overall willingness to pay. Internal consistency was $\alpha = .513$ (3 items) and thus larger, too (see <https://osf.io/2fkpm/> for the analysis code and <https://osf.io/dq48j/> for the original dataset).

Finally, we found a publication in which the average of the correlations between the anchoring items was reported. Similar to our approach, Teovanović (2019) used a within-subjects anchor (no anchor vs. anchor) manipulation for the general knowledge questions and found an average correlation of $r = .11$ or $r = .27$ depending on how it was calculated and $\alpha = .71$

(8 items, p. 15). It is unclear why this correlation is much higher than correlations between anchoring items in other studies.

Given the wide range of tasks used in our own study and the occurrence of the problem in other studies, we believe that the unreliable measurement of the susceptibility to anchoring effects poses serious problems for research on anchoring effect moderators. An overview of reported and computed reliabilities is provided in **Fehler! Verweisquelle konnte nicht gefunden werden..** Cronbach's alphas are significantly different between studies, $\chi(5) = 126.17$, $p < .001$.

Table 3

Reliabilities available from anchoring research.

Study	Coefficient Alpha	Alpha LCB	Alpha UCB	Average r	Number of Items	N	Note
Teovanovic, 2019	0.75	0.70	0.79	0.11	24	236	
Bergman et al., 2010	0.69	0.59	0.77	0.27	6	116	Confounded with willingness to pay
Frech et al., 2020, Study 2	0.51	0.38	0.62	0.26	3	189	Confounded with willingness to pay
Cheek & Norem, 2019	0.14	0.05	0.22	0.03	6	945	
This study	0.11	-0.07	0.27	0.01	10	225	
Röseler et al., 2020	0.05	-0.06	0.15	0.00	16	650	

Note: LCB = Lower Confidence Bound, UCB = Upper Confidence Bound for $\alpha = .05$.

Use of Difference Scores as a Potential Aspect of the Problem of Low Reliability

Part of the problems encountered with unreliable measurements may be due to the use of difference scores. That is, the reliability of the difference score (e.g., the estimate used for the high anchor minus the estimate used for the low anchor) depends on the reliabilities of the scores from which the difference is computed and the correlation between the scores from which the difference is computed (e.g., Peter et al., 1993). The difference between two unreliable scores is not reliable, and the difference between two strongly correlated scores (e.g., $r = 1$) has no variance and is not reliable either. Parsons et al. (2018) noted that the reporting of reliability in

cognitive bias tasks should be based on a standard to allow for an assessment of how widespread the problem of low reliabilities is. The problem is already known for the dot-probe task, where differences between reaction times are computed to measure attentional biases. Reports of reliability ranged from $r = -.23$ to $r = .70$ (Parsons et al., 2018, p. 15). Due to a lack of reporting standards for tasks such as anchoring, it is unknown how widespread the problem of low reliability is. Anchoring effects are clearly no exception to both the problem of missing reporting standards and the problem of low reliability.

It is possible that there are some items for which intercorrelations may be high for different reasons. For these cases, we advise researchers to critically examine whether the items may measure constructs beyond the susceptibility to anchoring effects. For example, using willingness to pay and high anchors may only yield strong anchoring effects for participants who have a high general willingness to pay. However, this may hold not because anchoring effects are stronger for them but because they have a higher income and give higher “estimates” on average.

Applicability of the Framework for Conceptualizing Individual Differences on Heuristics and Biases Tasks

Because the data were not reliable, we were unable to test the hypothesis we derived from Stanovich and West’s framework. Although this is a serious limitation, we uncovered a very important problem instead. We would even go so far as to say that it is not yet possible to test the hypothesis we derived from the framework. To test the model—and in fact, to test any interaction model on anchoring effects—a reliable assessment of participants’ susceptibility to anchoring effects is necessary.

Future Directions for Personality Trait Research in the Context of Anchoring Effects

To date, findings on moderators of anchoring effects (e.g., correlations between personality or intelligence with people's susceptibility to anchoring effects) are still inconclusive. For example, positive, negative, and null correlations have all been found between each of the Big Five and susceptibility to anchoring effects (Cheek & Norem, 2019). Our results show that anchoring effects cannot be moderated by any trait because the measure of susceptibility is not reliable. Given the unreliable assessment of susceptibility to anchoring effects, our null effects are not surprising and neither are the contradictory findings in the literature. With one exception, past studies did not report the reliability of the anchoring measures, although most designs allowed for a test of reliability, as we demonstrated using the data sets provided by Cheek and Norem (2019) and Bergman et al. (2010). As long as the reliability problem remains, any null findings for anchoring effect moderators do not indicate the absence of an actual effect, and any positive findings require close inspection. Other biases (e.g., the dot-probe task) face similar problems (Parsons et al., 2018), which could partly be resolved through theoretical work (Schäfer et al., 2016). Increasing the reliability of anchoring effects might be possible through the development of more specific theories.

For example, formalization of anchoring effects with the aid of cognitive models such as SARA (Selective Activation, Reconstruction, and Anchoring; Pohl et al., 2003) or TOTE-units (test-operate-test-exit; Epley & Gilovich, 2001) could enhance the reliability of anchoring measures. A descriptive approach was described by Turner and Schley (2016). Given that dynamic models have been used to solve reliability issues for other effects, this research strategy currently seems the most promising in the search for a reliable measure of the susceptibility to anchoring effects. Finally, we have to consider the possibility that observed differences in the

susceptibility to anchoring effects might not be due to actual individual differences but that there might actually be no trait that can be identified as *susceptibility to anchoring*.

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