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RESEARCH ARTICLE

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How numeric advice precision affects advice taking

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Abstract

Advice is a powerful means to improve peoples' judgments and decisions. Because advice quality is rarely apparent, decision-makers must infer it from the characteristics of the advisor or the advice itself. Here, we focus on a largely neglected advice characteristic that should signal quality: advice precision. In a preregistered, high-powered study (N = 195), we tested the effects of advice precision on advice taking. Drawing from past research and theorizing on anchor precision, we derived and tested two competing hypotheses for the relation of advice precision and advice taking—one predicting a monotone increase in advice taking when advice precision increases and the other predicting a backfiring effect of overly precise advice resulting in an inverted U-shape. Our results support the notion of a monotone, albeit not a strong monotone, relationship. Higher perceived advice quality correlated with individuals' advice taking. Consistent with the idea that advice precision serves as a cue for advice quality, the effect of advice precision on advice taking was statistically mediated by perceived advice quality. Although the mediation analysis does not allow for causal interpretation because we did not manipulate the mediating variable, it shows that the effect of advice precision on advice taking is not merely a demand effect. Implications of our findings for theory and practice are discussed.

KEYWORDS
advice taking, judgment and decision making, numerical precision, social influence

1 | INTRODUCTION

Advice is an effective means to improve the quality of peoples' judgment and decision making (Yaniv, 2004). How much decision-makers benefit from advice depends crucially on the quality of the advice. Because advice quality is rarely apparent, decision-makers must infer it when deciding whether (and how much) to heed the advice. Previous research has shown that decision-makers are sensitive to a wide range of advisor characteristics indicating advice quality—advisors' expertise (Harvey & Fischer, 1997), past performance (Yaniv & Kleinberger, 2000), or confidence (Van Swol, 2009). When information about the advisor is unavailable, decision-makers can use characteristics of the advice itself as an indicator of advice quality. One central and evident characteristic that should signal advice quality is precision. The more knowledgeable an advisor is, the more precise their recommendation should be (see Welsh, Navarro, & Begg, 2011). For example, an expert nutritionist may make more specific dietary recommendations than a novice, and more experienced financial advisors might make more precise recommendations about how much money to save each month for retirement than their less experienced colleagues. In line with the bulk of previous research on advice taking, we will concern ourselves with advice in the context of numerical estimates. Here, we can define advice precision as the extent to which a person rounds their estimate by using trailing zeros versus nonzero, "precise" digits (Janiszewski & Uy, 2008). For example, when consulting financial advisors about the prospects of a stock investment, one advisor may predict an increase of 10.00%, whereas another predicts an increase of 9.87%. The question then is how advice precision impacts decision-makers' evaluation of advice quality and, ultimately, their decision (and proclivity) to heed the advice. Interestingly, our understanding of how and why advice precision affects the extent of peoples' advice taking remains limited.

The, thus far, sole published study on advice precision shows that decision-makers perceive advisors who make precise estimates ("Mississippi is 3,992 miles long" vs. "Mississippi is 4,000 miles long") as more confident and report a preference for precise advisors (Jerez-Fernandez, Angulo, & Oppenheimer, 2014). Building on this study, we address two important questions. First, we investigate whether the effect of advice precision extends from self-reported advisor preferences to the actual amount of advice taking. This is crucial because the way decision-makers actually use advice can differ substantially from their a priori stated intentions (Fiedler, Hütter, Schott, & Kutzner, 2019; Schultze, Mojzisch, & Schulz-Hardt, 2017).

Second, we examine the functional relation of advice precision and advice taking. To derive predictions about this relation, we drew on existing literature and theorized on the effects of numerical precision in anchoring and negotiations. Similar to what we propose here in the context of advice taking, numerical precision has been proposed as a cue for expertise and competence in anchoring and negotiation research. Consistent with conversational logic (i.e., *conversational implicatures*; Grice, 1975), higher precision of first offers signals greater familiarity with the object of a negotiation or with an estimation task in a classical anchoring paradigm. Accordingly, people are anchored more potently by precise than by round anchor values—$4.85 versus $5.00 for cheese (Janiszewski & Uy, 2008) or $29.75 versus $30 for a DVD drive (Zhang & Schwarz, 2013). In negotiations, people consider precise first offers to be more plausible (Loschelder, Stuppi, & Trötschel, 2014), and they attribute greater expertise and knowledge to first-movers making more precise offers (Loschelder, Friese, Schaerer, & Galinsky, 2016). Interestingly though, prior research on anchoring and negotiation and the conversational maxims by Paul Grice (1975) allow us to derive two competing hypotheses about how advice precision affects advice taking. On the one hand, increasingly precise advice could lead to a monotone increase in perceived advisor confidence and, ultimately, advice taking (Loschelder, Stuppi, & Trötschel, 2014; Mason, Lee, Wiley, & Ames, 2013; Zhang & Schwarz, 2013).

Hypothesis 1a Advice taking increases with advice precision (monotone increase).

Note that Hypothesis 1a predicts a monotone but not necessarily a strong monotone relationship. That is, Hypothesis 1a predicts for any level of advice precision that increasing advice precision leads to greater or at least equal levels of advice taking. In contrast, Hypothesis 1a would be falsified if we observe for at least one level of advice precision that increasing advice precision *reduces* the extent of advice taking.

We consider one such case to be a plausible alternative to Hypothesis 1a. Specifically, the relation of advice precision and advice taking could resemble an inverted U-shaped effect (see Grant & Schwartz, 2011). Although increasing precision could elevate perceptions of advice quality and advice taking up to a certain point, it might backfire once precision exceeds a plausible threshold (e.g., "Mississippi is 5,841.73 kilometers long"). This backfiring hypothesis builds on the conversational maxims of quantity and quality (Grice, 1975; see Zhang & Schwarz, 2013). People expect opposing communicators to truthfully provide as much valid information as is needed—but neither less information nor more (see Wänke, 2007 for a review). Specifically, "the maxim of quantity represents an ideal point at which everything that is said is informative and everything that is informative is said" (Wänke, 2007, p. 225). On the basis of these maxims, an anchor recipient may assume that each digit of a moderately precise advice ("Mississippi is 5,650km long") is necessary to adequately express an advisor's true knowledge. However, an overly precise estimate ("Mississippi is 5,641.73km long") could well violate the conversational maxims, as it provides *too much* information, and in this way undermine the credibility that advisees ascribe to the overly precise advisor (see Loschelder, Friese, Schaerer, & Galinsky, 2016 for a similar result in a negotiation setting). In all, high precision may signal more advice quality than rounded estimates; yet, if advice becomes implausibly precise, decision-makers might lose confidence in the expertise of their advisor and, thus, use the advice less.

Hypothesis 1b The relation of advice precision and advice taking follows an inverse U-shape. High precision leads to greater advice taking than moderate precision, but extreme precision backfires, leading to lower advice taking compared with high precision.[1]

Previous research has shown that precise advisors are perceived as more confident but did not explicitly measure decision-makers' beliefs about the expertise of the advisors or in the quality of the provided advice (Jerez-Fernandez, Angulo, & Oppenheimer, 2014). However, previous studies on advice taking established that decision-makers use advisor confidence as a cue for advice quality (Van Swol, 2009; Yaniv & Kleinberger, 2000). Thus, we would expect numerical advice precision to affect advice taking because it influences decision-makers' perceptions of advice quality. If so, we should find that perceived advice quality mediates the effect of advice precision on advice taking.[2] Accordingly, we further postulated perceived advice quality as an underlying mediator.

Hypothesis 2 Participants' perception of advice quality mediates the effects postulated in H1a or H1b.

[1]The wording of Hypothesis 1b deviates slightly from the preregistration to reflect a relabeling of the three levels of advice precision in our study as suggested by an anonymous reviewer. Instead of low, moderate, and excessively high precision (the original wording), we now refer to moderate, high, and extreme precision.

[2]Note that evidence of regression-based mediation does not constitute positive evidence of causality in our study because we did not experimentally manipulate advice quality (i.e., *measurement-of-mediation*; Spencer, Zanna, & Fong, 2005; see Baron & Kenny, 1986). We address this issue in the discussion.

2 | METHOD

2.1 | Transparency statement

This study was preregistered at the Open Science Framework prior to data collection. The preregistration is available at https://osf.io/jgmd6/ (Schultze & Loschelder, 2020). We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures collected in this study.

2.2 | Participants and design

We computed the required sample size based on the results of a pilot study (N = 85). The pilot study tested for effects of advice precision on (1) advice taking and (2) ratings of advice quality, manipulating three levels of advice precision within participants. Data revealed (1) a moderately sized effect of advice precision on advice taking (f = .22, p = .017; correlation of measurements: r = .70) and (2) a small and statistically insignificant effect of advice precision on perceived advice quality (f = .10, correlation of measurements: r = .40).

The present study realized a one-factorial within-subjects design with advice precision (moderate vs. high vs. extreme) as the independent variable (see Footnote 1). We aimed for a power of $1 - \beta = .80$ for the weaker of the two effects obtained in the pilot study (i.e., the effect of precision on ratings of advice quality) with the usual type-I error level of α = .05. Hence, assuming an effect size of f = .10 and a correlation of measurements of r = .40, the power analysis suggested a required sample size of 195 participants. For the moderate f = .22 effect on advice taking obtained in the pilot study, this sample size

TABLE 1 Target stimuli, true values, and advice as a function of precision used in the study

		Advice by level of precision		
Name of the river	True value (in km)	Moderate	High	Extreme
Rio Japura	2,816	3,000.00	3,037.00	3,024.23
Lena	4,294	3,500.00	3,532.00	3,473.21
Vistula	1,047	1,800.00	1,781.00	1,777.54
Po	652	400.00	366.00	414.95
Ems	371	500.00	462.00	491.73
Rhone	812	1,100.00	1,139.00	1,085.55
Rio Jurua	3,283	2,500.00	2,522.00	2,548.48
Sambesi	2,574	2,500.00	2,465.00	2,453.62
Rio Sao Francisco	3,199	3,000.00	2,949.00	3,049.53
Wiljui	2,650	3,000.00	3,021.00	2,972.05
Murrumbidgee	1,579	1,200.00	1,147.00	1,174.23
Brisbane River	309	500.00	482.00	523.21
Rhein	1,238	1,200.00	1,241.00	1,177.54
Niger	4,184	3,000.00	3,046.00	3,034.95
Indus	3,180	2,500.00	2,512.00	2,531.73
Tigris	1,900	2,000.00	1,989.00	2,045.55
Volga	3,530	3,500.00	3,472.00	3,538.48
Meuse	874	900.00	945.00	913.62
Tejo	1,007	900.00	939.00	859.53
Ottawa River	1,271	300.00	311.00	322.05
Rio Purus	3,210	2,500.00	2,537.00	2,554.23
Arkansas River	2,333	2,800.00	2,832.00	2,823.21
Ural	2,428	4,000.00	3,961.00	3,967.54
Vltava	430	3,000.00	3,026.00	3,004.95
Colorado River	2,330	2,500.00	2,472.00	2,461.73
Irtysh	4,248	3,200.00	3,159.00	3,245.55
Canadian River	1,458	600.00	612.00	558.48
Snake River	1,674	2,500.00	2,455.00	2,463.62
Adige	415	800.00	829.00	819.53
Rio Grande	3,034	3,000.00	2,971.00	2,962.05

Note: True values were derived from German Wikipedia entries for the respective rivers.

ensures a power close to 1 ($1 - \beta > 99.9\%$). As stated in the preregistration, we collected data until we had 195 valid data sets (i.e., participants completed the study and indicated after the study that we could trust their data). Two participants indicated that we should not trust their data and were replaced prior to data analysis to ensure the final sample size of 195. Participants were undergraduate and graduate students with a mean age of 23.03 years (*SD* = 4.05 years); 122 (63%) reported their gender as female, and the remaining 73 (37%) identified as male. All participants provided informed consent about participation.

2.3 | Procedure

We programmed the experiment using the software ALFRED (Treffenstaedt & Wiemann, 2018). Upon arrival at the laboratory, experimenters, who were blind to the experimental condition, welcomed participants and individually seated them in front of a networked computer. After providing informed consent, participants received all standardized instructions on their respective computer screens. Participants learned that their task would be to estimate the length of 30 rivers in kilometers as accurately as possible (Table 1 lists the stimuli). To avoid statistical outliers due to absurd estimates, we informed participants that the length of these rivers varied between a few hundred and several thousand kilometers and that, as a frame of reference, the Amazon River is the longest river on earth with a length close to 7,000 km.

Participants next learned that each trial consisted of two phases. In the first phase, they were to make an initial estimate of the target river's length. In Phase 2, participants would be shown their own initial estimate along with the estimate of an advisor (the advice). Participants then first rated the quality of this advice on a 7-point Likert scale (1 = *not at all competent*; 7 = *very competent*) before making their final estimate. Participants could enter values between 0 and 7,000 km with up to 2 decimals for their estimates. In addition to a participation fee of €5 (or course credit), participants learned that they could earn a bonus of up to €3 based on the accuracy of their final estimates. They received €0.10 for each final river length estimate that did not deviate by more than 10% from the true value. The final part of the instructions informed participants that there were three different advisors, (randomly) labeled Persons A, B, and C, who would each provide advice on 10 of the 30 trials.

2.4 | Experimental manipulation

We manipulated advice precision so that each advisor provided advice of a certain numerical precision on all 10 trials (moderate vs. high vs. extreme precision). Advice of moderate precision was rounded to 100 km and consisted of the actual estimates made by a pretest participant. In this pretest, participants worked on the same tasks and with the same incentives as participants in our present study. We chose the pretest participant whose accuracy marked the average of the pretest sample as the advisor for our study. To create advice of high precision, we modified the moderately precise advice (rounded to 100 km) by first randomly adding or subtracting 10, 20, 30, or 40 km and then adding or subtracting another 1, 2, 3, or 4 km. To create advice of extreme precision, we modified the highly precise advice by randomly adding or subtracting another 0.1, 0.2, 0.3, or 0.4 km as well as 0.01, 0.02, 0.03, or 0.04 km. Using this procedure, we created one set of advice for each level of precision (see Table 1), that is, advice for a given river was identical to all participants if the respective trial fell into the same precision category.

Note that the majority of participants decided to round their estimates to hundred kilometers in this judgment task.[3] Thus, for most participants, highly precise advice, specified to single kilometers, is already more precise than their own initial estimates by two orders of magnitude (i.e., two additional precise, nonzero digits). However, although the level of precision for highly precise advice was still realistic (it matched the level of precision of the official measurements and geography classes in German secondary schools where students learn about river lengths), we designed the extremely precise advice to be unusually precise (i.e., more precise than the official measurements by a factor of 100 and more precise than the initial estimates of the typical participant by a factor of 1,000) to allow for a fair test of Hypothesis 1b.

We displayed all advice as well as participants' own estimates with two decimals to avoid confounding advice precision and the graphical presentation of the advice (i.e., numbers of [decimal] digits; see Wong & Kwong, 2000). Our manipulation allowed us to create advice that varied in precision but not in accuracy, meaning that we can attribute all differences in advice taking to the precision of the advice (not its distance from true values). This is important because decision-makers can detect differences in advice quality even in the absence of performance feedback or information about advisor expertise (Yaniv & Kleinberger, 2000). The order of the rivers was identical for all participants. We randomized which advisor provided the advice on a given trial and which advisor—A, B, or C—was associated with each level of advice precision.

2.5 | Final questionnaire

Upon completing the last trial, participants were asked to fill in a final questionnaire, reporting their age, gender (male, female, or diverse), and whether we could trust their data. In addition, participants indicated which advisor (A, B, or C) they would prefer to receive advice from in the future. We sought to use this variable for exploratory analyses (see Jerez-Fernandez, Angulo, & Oppenheimer, 2014). Unfortunately, we recorded which advisor (A, B, or C) participants preferred, but the computer program did not contain a variable indicating which advisor had been randomly assigned to which level of precision for a specific participant, rendering this variable useless. The final

[3]For example, in this study, participants' preadvice estimates were rounded to 100 km in 62% of the cases, to 10 km in 17% cases, and to single kilometers in 18% of the cases. In the remaining 3% of the cases, the initial estimate was precise to at least one decimal.

questionnaire also contained an open-response field for participants to state their beliefs about the aim of our study. Finally, participants learned the size of their monetary bonus, were paid, thanked, and debriefed.

2.6 | Dependent variable—Advice taking

Our measure of advice taking was the AT score (see Harvey & Fischer, 1997), which is defined as follows: AT = (final estimate − initial estimate)/(advice − initial estimate). Errors when entering an estimate (e.g., entering 200 km instead of the intended 2,000 km) can produce extreme (and erroneous) AT scores. Such errors have the potential to distort the mean AT scores drastically. Following our preregistered data cleaning protocol, we winsorized AT scores at 0 and 1 as is common in research on advice taking (e.g., Minson & Mueller, 2012; Soll & Larrick, 2009; see also Bonaccio & Dalal, 2006). Winsorizing means that values smaller than 0 were set to 0 (1.5% of all trials), whereas values exceeding 1 were set to 1 (3.4% of all trials). For each participant, we then computed their mean AT score for each level of advice precision. In 0.9% of all trials (53 out of 5,850), AT scores were not defined because participants' initial estimates were identical to the advice. We omitted these trials—all of which occurred in the low precision condition—when computing the mean AT scores.

3 | RESULTS

3.1 | Advice taking

We first analyzed participants' mean AT scores in a repeated-measures analysis of variance (ANOVA). The ANOVA showed a significant effect of advice precision on advice taking, $F(2, 388) = 27.26$, $p < .001$, $\eta^2 = .03$ (Figure 1). To differentiate between Hypotheses 1a and 1b, we followed the ANOVA with a polynomial contrast analysis. To this end, we predicted advice taking from two orthogonal contrasts, one linear (weights: −1, 0, 1) and the other quadratic (weights: −1, 2, −1). The linear contrast was statistically significant, $F(1, 388) = 46.12$, $p < .001$. The same was true for the quadratic contrast, $F(1, 388) = 7.62$, $p = .009$. The pattern of the means (Figure 1) was consistent with a monotone—albeit not a strong monotone—increase in advice taking as a function of increasing precision. The mean AT score was $M = 0.34$ ($SD = 0.19$) for advice of moderate precision. Advice taking was more pronounced for advice of high precision, $M = 0.41$ ($SD = 0.23$), and for advice of extreme precision, $M = 0.42$ ($SD = 0.23$), respectively (Figure 1, left panel). Such a nonlinear relation of advice precision and advice can be approximated reasonably well by combining a linear and a quadratic term. The results underline Simonsohn's (2018) recent critique that quadratic contrasts can be significant *without* any evidence for a backfiring, inverted-U-shape effect.

In sum, the result pattern leads us to accept Hypothesis 1a (monotone, but not a strong monotone, increase); we also reject Hypothesis 1b (inverse U-shape) as there was no empirical evidence for a backfiring effect of extreme precision (Figure 1). Because we found evidence for both a linear and a quadratic trend in our data (i.e., a monotone “plateau” effect), we decided to deviate from our preregistration for all further analyses. Although we stated a priori that we would use either the linear contrast *or* the quadratic contrast as a predictor—depending on which of the two contrasts was significant in the analysis of the AT scores—we instead decided to use them both as predictors.

3.2 | Perceived advice quality

To test Hypothesis 2, we first ran a multilevel regression on participants' mean ratings of advice quality with both the linear and the quadratic contrast of advice precision as fixed effects and random intercepts for participants (testing for mediation required this regression format; a repeated-measures ANOVA similar to the one we report for AT scores yielded similar results). The analysis revealed a significant effect of the linear contrast of advice precision on ratings of advice quality, $B = 0.15$, $SE = 0.03$, $t(388) = 4.55$, $p < .001$. The quadratic contrast was also a statistically significant predictor, $B = 0.05$, $SE = 0.02$, $t(388) = 2.36$, $p = .019$ (see Figure 1, right panel).

3.3 | Mediation analysis

We next predicted mean AT scores from both contrasts of advice precision (linear and quadratic) and from mean ratings of advice quality in a multilevel model, with the contrasts of advice precision and the quality ratings as fixed effects and random intercepts per participant. The analysis revealed a significant effect for the linear contrast of advice precision, $B = 0.02$, $SE = 0.004$, $t(391.32) = 5.12$, $p < .001$. The quadratic contrast was no longer statistically significant, $B = 0.004$, $SE = 0.003$, $t(388.06) = 1.67$, $p = .096$. Finally, the quality ratings were significantly related to advice taking, $B = 0.11$, $SE = 0.01$, $t(490.71) = 17.09$, $p < .001$. We tested for statistical mediation of the precision effect on advice taking via participants' self-reported advice quality by computing the indirect effect as well as the bias corrected and accelerated (BCa) 95% confidence intervals (CI) for both contrasts using 10,000 bootstrap simulations (Hayes, 2013). To this end, we computed two mediation models, each estimating the indirect effect of one of the two contrasts—linear and quadratic—while statistically controlling for the effect of the other contrast. The estimate of the indirect effect of the linear contrast was $b = 0.017$, BCa $CI_{95\%}$ [+0.009; +0.025], whereas the indirect effect of the quadratic contrast was $b = 0.005$, BCa $CI_{95\%}$ [+0.001; +0.009], with both confidence intervals excluding zero. These indirect effects accounted for 42% (linear contrast) and 53% (quadratic contrasts) of the total effects. Thus, we accept Hypothesis 2—correlation-based mediation analyses suggested that participants' subjective perceptions of advice quality partially mediated the behavioral effect of precision on participants' actual amount of advice taking.

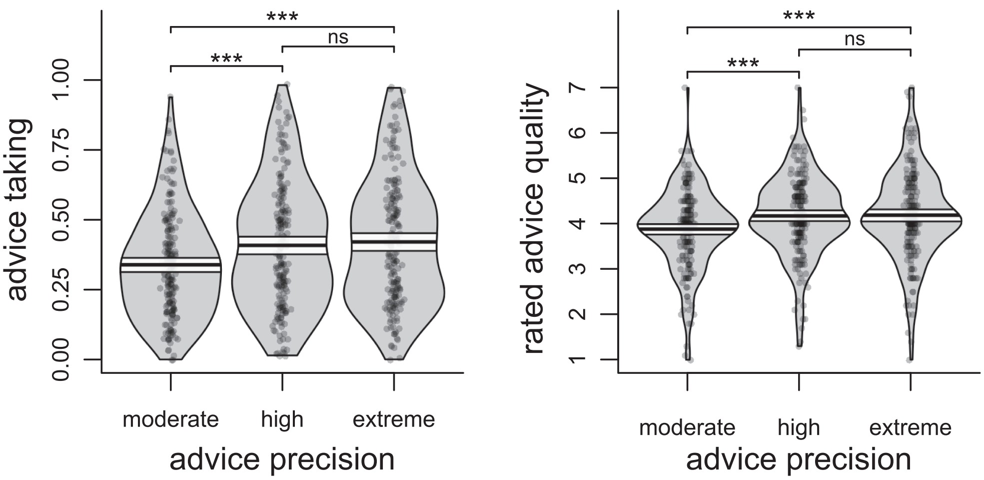

FIGURE 1 Pirate plots of mean AT scores (left panel) and mean ratings of subjective advice quality (right panel) as a function of advice precision (moderate, high, and extreme). The plots show the distribution of the data as well as individual data points. Each point represents a participant's mean AT score across all 10 trials of the respective level of advice precision. The width of the beans corresponds to the estimated density of the respective dependent variable at a given point of the *y* axis. The bold horizontal lines represent the means, whereas the white bands denote 95% confidence intervals around the means. Pairwise comparisons of the means are based on paired *t* tests with 194 degrees of freedom. *** denotes $p < .001$

4 | DISCUSSION

Although previous research showed that decision-makers perceive more precise advisors as more confident and report to prefer them as advisors (Jerez-Fernandez, Angulo, & Oppenheimer, 2014), we knew surprisingly little about (a) precision effects on actual advice taking and (b) whether this relationship is monotone or would backfire for excessive precision. Our preregistered, high-powered study shows that the advice precision effect translates to actual advice taking and is monotone (albeit not strong monotone). Greater advice precision led decision-makers to heed advice more, ceteris paribus. Crucially, even if precision reached a level that one could consider too precise to be true and as breaching conversational norms of quantity and quality (e.g., "The Colorado River is 3,461.73km long"), we did not observe a backfiring effect. Instead, advice taking seemed to reach a plateau once a certain level of precision was reached.

Of course, the present data do not allow us to rule out that such a backfiring effect on advice taking might have occurred had we presented advice of even greater numerical precision. Perhaps participants still considered it plausible that some advisors knew the length of rivers to 10 m, for example, because they believed it to be in line with the capabilities of current technologies such as GPS or satellite measurement, but would have mistrusted advisors when their estimates had been precise to the centimeter or even millimeter. In this regard, it is conceivable that a backfiring effect of overly precise advice might occur only if advisees have the necessary expertise to judge which level of precision is realistically attainable. Investigating this possibility seems a promising avenue for future research. For now, however, our data suggest that increasing the precision of advice increases its influence up to a certain threshold with no indication of further increases or decreases in advice taking thereafter.

The behavioral effect of precision on advice taking was mediated by participants' perceived advice quality. We explicitly refrain from interpreting this mediation causally because we did not manipulate the mediating variable experimentally (see Fiedler, Schott, & Meiser, 2011; Spencer et al., 2005). Because we measured perceived advice quality before judges made their final estimate, in order to account for covariance *and* precedence (Hume, 1748/2003), we can rule out that judges rated the advice more positively because they heeded it more. However, we cannot rule out the alternative explanations that ratings of advice quality and advice taking are relatively independent outcomes of a common cause (a *tertium quid* that accounts for both) or that they are so closely linked that they must be considered operationalizations of the same construct (i.e., the mediator is a confound of the dependent variable). These causality limitations notwithstanding, our results do allow for an important conclusion, however: the parallel findings for perceived advice quality and factual advice taking show that the effect of advice precision cannot be reduced to a demand effect. This result pattern rules out that participants followed precise advice at a shallow level of compliance or conformity. Rather, the effect really coincides with a feeling of enhanced advice quality.

Before we discuss the implications of our results, we need to point to a caveat. The way we presented the advice (rounded to two decimals irrespective of the level of advice precision) avoided confounding advice precision and the graphical presentation of the advice. However, as an anonymous reviewer pointed out correctly, this may have inadvertently created another confound, namely, that advice did not look equally natural in all conditions. Specifically, it may have seemed odd to participants to see two zero decimals when advice was rounded to 100 km or precise to single kilometers. Although this confound should not affect the comparison of moderate and high advice precision (because trailing decimal zeros are equally unnatural in these conditions), it may have affected the comparison of extremely precise advice with the other two conditions. Thus, future studies may want to test whether the effects we report here look any different if decimals are presented only when the level of precision necessitates it (i.e., 3461.73 km) but *not* for moderate (3,500 km) and highly precise advice (3,472 km).

We conclude with two implications of our findings that we consider particularly noteworthy. First, they highlight the importance of studying features of the advice itself in order to understand when, why, and how much decision-makers heed advice. Whereas past research predominantly focused on one specific advice feature, namely, the distance between the advice and the decision-maker's initial opinion (e.g., Ecken & Pibernik, 2015; Schultze, Rakotoarisoa, & Schulz-Hardt, 2015), our study establishes advice precision as another characteristic worthy of investigation. Second, our findings point toward a heuristic that judges may use when deciding whether (and to what extent) to heed advice: advice precision indicates advice quality. This heuristic is likely to be effective in many contexts because we would expect that expert advisors make more precise (Welsh, Navarro, & Begg, 2011) *and* more accurate recommendations than laypeople. However, our findings also suggest that advisors can exploit this heuristic by inflating advisees' perceptions of advice quality. Some evidence already suggests that advisors strategically inflate their confidence to manipulate advisees toward heeding their advice more (Hertz et al., 2017; Van Swol, 2009). Importantly, advisees seem unable to distinguish between advisors with inflated versus genuine confidence. Future research may examine a similar strategic inflation of advice precision to signal greater expertise, as well as potential antidotes for advisees to resist succumbing to (too) precise advice.

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