Unhealthy and Unaware?

Misjudging Social Comparative Standing for Health-Relevant Behavior

Published at Journal of Experimental Social Psychology

<https://doi.org/10.1016/j.jesp.2019.103873>

Jane E. Miller ^a, Paul D. Windschitl ^b, Teresa A. Treat ^c, Aaron M. Scherer ^d

^a Corresponding author

Department of Psychological and Brain Sciences
University of Iowa
Iowa City, IA, 52242
jane-miller@uiowa.edu

^b Department of Psychological and Brain Sciences

University of Iowa
Iowa City, IA, 52242
paul-windschitl@uiowa.edu

^c Department of Psychological and Brain Sciences

University of Iowa
Iowa City, IA, 52242
teresa-treat@uiowa.edu

^d Department of General Internal Medicine

University of Iowa Hospitals and Clinics
Iowa City, IA, 52242
aaron-scherer@uiowa.edu

This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

Abstract

People sometimes modify their behavior based on whether they believe they do more or less of that behavior than others. But are people's perceptions of their social-comparative status for behaviors generally accurate? The current research assessed accuracy and bias in perceived social-comparative status for a number of health-related behaviors. In two studies, participants estimated their social-comparative percentile regarding behavior frequency for 20 behaviors—pre-classified according to a 2 (healthy or unhealthy) x 2 (generally common or uncommon) design. Participants also reported their absolute frequency of engagement for the behaviors, and these reports were used to approximate people's actual percentiles. Subjective percentile estimates were overly favorable for both healthy and unhealthy behaviors and were biased by a behavior's general commonness/rarity. People who were least healthy regarding a behavior tended to be the most miscalibrated in their percentile estimation for that behavior. There was also support for a noise-plus-bias model of people's percentile estimates.

Keywords: social comparison, health behavior, self-assessment, comparative bias, judgment accuracy

Unhealthy and Unaware?

Misjudging Social Comparative Standing for Health-Relevant Behavior

Compared with other people, how healthy are your behaviors? Do you engage more or less often than other people in health-related behaviors such as eating vegetables, distracted driving, adding salt to food, and hand washing? In the studies reported here, we investigated accuracy and bias in these self-assessments. Research on social comparisons suggests that people are not merely concerned with how their characteristics, abilities, and outcomes measure against objective standards; they also care about their social comparative standing (Buunk, Gibbons, & Visser, 2002; Festinger, 1954; Klein, 1997). Furthermore, social comparisons often have a critical influence on people's emotional responses and motivations for behavior change (Buunk et al., 2002; Gerber, Wheeler, & Suls, 2018; Klein, 1997). Consequently, it is important to know whether people's perceptions of social-comparative status for health behaviors tend to be accurate or are vulnerable to systematic biases and error.

Types of biases in people's social-comparative assessments have been studied extensively, with research focused on assessments of abilities, traits, and chances of experiencing future events (Alicke & Govorun, 2005; Chambers & Windschitl, 2004; Kruger, 1999; Kruger & Dunning, 1999; Shepperd, Carroll, Grace, & Terry, 2002). For example, in better-than-average effects detected for skill perception, people tend to overestimate their skill relative to peers or to others in the sample (Alicke & Govorun, 2005; Klein & Buckingham, 2002; Kruger, 1999). This

paper examines whether four biases from this literature—one of which was analogous to the better-than-average effect—apply systematically to people’s perceptions of their social comparative standing for health behaviors. Tests of two of these biases address the important issues of *for whom* and *when* these errors are most prominent. Are unhealthy people particularly bad at judging their social comparative standing? That is, is there an effect akin to an unskilled-and-unaware effect (i.e., Kruger & Dunning, 1999) that has been detected in the ability-judgment literature? Or, are observed patterns of who shows the most bias (and when) not analogous to the unskilled-and-unaware effect—perhaps instead dependent on moderators relevant to a different explanation called the noise-plus bias model (described below; Burson, Larrick, & Klayman, 2006; Krueger & Mueller, 2002).

To preview our empirical approach, we asked people to estimate their percentile (i.e., social-comparative standing) for a variety of health behaviors (e.g., how often do you wash your hands compared to other people?). They also reported absolute behavior frequency (e.g., how many times a week do you wash your hands?). We used the absolute estimates to approximate their actual percentile standing and then examined accuracy and bias in their subjective percentile estimates. For reasons that will be clear shortly, we queried people about behaviors in four categories: 1) common and healthy, 2) common and unhealthy, 3) uncommon and healthy, and 4) uncommon and unhealthy.

Possible Biases

Previous research on self-assessments provides guidance for what systematic biases to look for. Below we provide background for each of four biases and explain how they apply in the current studies.

Favorable overplacement effect. The better-than-average effect is a common

comparative bias in which too many people from a sample believe they are above average in a variety of abilities and traits (Alicke, 1985; Alicke & Govorun, 2005; Chambers & Windschitl, 2004). The term *overplacement* has also been applied to this type of effect (Larrick, Burson, & Soll, 2007; Moore & Healy, 2008). In a related effect, called unrealistic comparative optimism, people judge themselves as less (more) likely than others to experience negative (positive) life outcomes (Shepperd, Klein, Waters, & Weinstein, 2013; Weinstein, 1980). Both better-than-average and unrealistic comparative optimism effects have been traditionally considered motivated biases—people can be motivated to self-enhance, protect their egos, make themselves feel better, and avoid anxiety or other distress (Alicke, 1985; Brown, 2012; Klein & Kunda, 1993; but see also Chambers & Windschitl, 2003).

In the present research, we tested for an effect akin to better-than-average and comparative optimism effects, which we'll refer to as a favorable overplacement effect. Namely, participants might report their percentile as below 50 for unhealthy behaviors but significantly higher, and greater than 50, for healthy behaviors. Our dependent variable (beliefs about relative health behavior frequency) is different from those in most previous studies that have focused on beliefs about abilities, traits, vulnerability, or life outcomes. With that said, there are some prior studies on specific eating behaviors that produce evidence of favorable overplacement in health-related behaviors (e.g., Klein & Kunda, 1993; Miles & Scaife, 2003; Scherer et al., 2016; see also Dunning, Heath, & Suls, 2004). Our studies broaden the examination of favorable overplacement across a variety of unhealthy and healthy behaviors in different domains.

Rarity-commonness effect. Favorable overplacements are not detected across all task abilities and future outcomes. In the case of task abilities, general task difficulty moderates the strength and even direction of comparative bias (Kruger, 1999; Larrick et al., 2007; Moore &

Small, 2007; Windschitl, Kruger, & Simms, 2003). When people are queried about skills on tasks that are generally easy for people in the sample, above-average effects are typically observed, but for skills that are generally hard, below-average effects appear (Kruger, 1999). In the case of people's estimates about their comparative chances of future outcomes, the rarity/commonness of an outcome plays a similar moderating role. People typically give social-comparative likelihood judgments that are low for outcomes that are generally rare, but high for outcomes that are generally common (Chambers, Windschitl, & Suls, 2003; Kruger & Burrus, 2004). The hard-easy and rarity-commonness effects appear primarily attributable to versions of egocentrism, in which people base their comparative judgment mainly on their own general ability or perceived likelihood, neglecting the importance of the referent (Chambers & Windschitl, 2004; Chambers et al., 2003; Dunning & Hayes, 1996; Klar, Medding, & Sarel, 1996; Kruger, 1999; Price, Pentecost, & Voth, 2002; Weinstein & Lachendro, 1982). For the present studies, we expected a rarity-commonness effect on participants' social-comparative percentile estimates for how often they did behaviors. They would give percentile estimates that are too high for behaviors that are generally common within the sample (e.g., washing hands), and estimates that are too low for behaviors that are generally uncommon within the sample (e.g., going to the doctor).

Unhealthy-and-unaware effect. We have discussed the previous effects as if they are sample-wide effects. However, previous research also provides potential insight about which subsets of the sample might exhibit these effects the most. In research on how people estimate their social-comparative standing on abilities, Kruger and Dunning (1999, 2002) documented *unskilled and unaware effects*, where participants in the lowest objective quartile for performance on a given skill were more inaccurate in their subjective percentile estimates than

were participants in other quartiles (see also Ehrlinger, Johnson, Banner, Dunning, & Kruger, 2008). In fact, participants in the lowest quartile tended to believe they were above average (above the 50th percentile). Participants in the highest quartile slightly underestimated their percentile placements, but not to the same degree that the lowest quartile overestimated. Kruger and Dunning offered a metacognitive explanation for the excessive miscalibration of the *unskilled*. Namely, those who are unskilled on a particular task also tend to lack the ability to recognize their poor performance on that task (Kruger & Dunning, 1999, 2002; for follow-up support of this idea, see also Dunning, Johnson, Ehrlinger, & Kruger, 2003; Schlösser, Dunning, Johnson, & Kruger, 2013; but see also Krueger & Mueller, 2002).

The present paper is the first that we know of to address whether an effect similar to the unskilled and unaware effect exists for how people judge their social comparative standing on health behaviors. We tested for what we call an *unhealthy-and-unaware effect*. Because we have both healthy and unhealthy behaviors in our design, an unhealthy-and-unaware effect would manifest in a mirrored fashion for the two valences. For a given healthy behavior, miscalibration of subjective percentiles would be highest among people in the lowest quartile (i.e. those who do that healthy behavior the least). For a given unhealthy behavior, miscalibration would be greatest in the highest quartile (i.e. those who do that unhealthy behavior the most).

If observed, this effect could be attributed to motivated and nonmotivated causes. A motivated explanation could posit that, when people are relatively unhealthy on a given behavior, they are reluctant to believe that their social comparative standing is especially unfavorable, whereas people who are healthy on that behavior would be open to holding a more accurate conclusion (Klein & Kunda, 1993; Miles & Scaife, 2003). A nonmotivated explanation could suggest that people who are unhealthy on a given behavior also tend to be poor in their

awareness of the norms for that behavior or how often the average person engages in that behavior, whereas people who are healthy on a behavior are more aware of norms and the tendencies of others. This latter explanation bears some resemblance to the traditional metacognitive explanation offered for the unskilled and unaware effect in skill domains (Kruger & Dunning, 1999, 2002).

Noise-plus-bias model. There have been several responses to the unskilled-and-unaware effect (e.g., Ackerman, Beier, & Bowen, 2002; Krajc & Ortmann, 2008; Krueger & Mueller, 2002; see also Ehrlinger et al., 2008; Schlösser et al., 2013). One response that is relevant to the current studies is called the *noise-plus-bias model* (Burson, Larrick, & Klayman, 2006). Our studies are the first to test the noise-plus-bias model in the context of how people judge their social comparative standing on health behaviors. In the original formulation, the “noise” part of the model’s name refers to the notion that people’s percentile estimates are imperfect and noisy (Burson et al., 2006). The “bias” part of the model’s name refers to insight from previous research showing hard-easy effects—namely, that estimates of social comparative standing tend to be systematically low for hard tasks and high for easy tasks (Kruger, 1999). The model posits that, regardless of skill level, noise makes percentile estimates regressive towards a mean level, but this also combines with the impact of the bias. In the case of a task that is generally easy, the net result is substantial overestimation of percentiles from unskilled participants (the regression and bias effects work in the same direction), but better-calibrated estimates from skilled participants (the regression and bias effects offset). This matches the unskilled-and-unaware pattern. Critically, in the case of a task that is generally difficult, the net result is a different pattern in which the highly skilled, not the unskilled, would be most miscalibrated because they substantially underestimate their percentile.

For our studies, we predicted a pattern of results analogous to those anticipated by the noise-plus-bias model. We expected that when people estimate their percentile for how often they engage in a given behavior compared to other people in the sample, their responses are somewhat noisy and biased. The noise yields regression effects that “push” people who do the behavior most infrequently (people in the low percentiles) towards overestimating their percentile, and “push” people who do the behavior most frequently (people in the high percentiles) towards underestimating their percentile. Simultaneously, an egocentric bias is at work, which systematically depresses percentile estimates for rare behaviors but elevates them for common ones. With common behaviors, the net effect of the noise and egocentric bias would be that people in the lowest quartile for a given behavior are most miscalibrated. They would substantially overestimate their percentile, whereas people in the top quartile would be better calibrated. All else equal, the opposite would be true for uncommon behaviors, with people in the lower quartile most calibrated and people in the top quartile substantially underestimating.

Summary and Preview

We have described four patterns of bias that might be relevant to how people judge their social comparative standing: 1) favorable overplacement, 2) rarity-commonness effect, 3) unhealthy and unaware, and 4) noise plus bias. These patterns are not mutually exclusive, so it is logically possible for all four patterns to be observed in the same study. Below, we present two studies addressing these possible biases in perceived social comparative standing on health behaviors. The two studies are similar, and both were preregistered. For Study 1, we preregistered specific directional predictions about patterns 2 and 4. Namely, we expected that people’s percentile estimates about their social-comparative standing would be significantly higher for behaviors that are generally common vs. rare. We also expected that this bias, along

with the regressive effects of noise, would do a good job accounting for who is most biased in their percentile estimates (i.e., we'd observe the pattern predicted from the noise-plus-bias model). For Study 2, which is a replication attempt of Study 1, we preregistered directional predictions for all four patterns. That is, we predicted that they would all be observed within the same sample.

To our knowledge, neither the unskilled and unaware effect nor the noise-plus-bias model have been tested in the context of social comparative beliefs about health behaviors. As addressed more in the General Discussion, understanding these biases are important because they reveal who is most biased and under what conditions. This could allow for better targeting in interventions aimed at altering social comparative beliefs, which can ultimately influence health behavior (Lockwood & Pinkus, 2008).

Study 1

Method

Pilot testing to establish the behavior set. We used pilot testing with undergraduate participants ($N=89$) to establish the set of 20 behaviors that would be used for the main study (see online Supplementary Materials for a full description of the process). Based on pilot ratings of commonness and healthiness for 54 behaviors, we selected 5 behaviors fitting each of these categories: 1) common and healthy, 2) common and unhealthy, 3) uncommon and healthy, and 4) uncommon and unhealthy. See Table 1 for the 20 behaviors.

Design, participants, and statistical power. The design was a 2 (valence: healthy/unhealthy) $\times$ 2 (commonness: common/uncommon) within-subject factorial, with 5 behaviors in each of the 4 cells. Our sample-size goal of 100 was determined *a priori* and preregistered on the Open Science Framework (osf.io/9jqbz). We collected data from all the

sessions we posted in anticipation of reaching our sample-size goal, which ultimately yielded a sample size of 112 University of Iowa undergraduates (74 women, 37 men, 1 unreported). The full sample was collected prior to running any analyses, and no participants were excluded from the data analysis.

Our sample-size goal was based on providing at least 80% power for detecting small-medium effects in within-subject comparisons. However, some of our key tests involve comparisons between participants in the top and bottom quartiles, and our initial power estimates did not treat stimuli as a random factor. Based on Westfall, Kenny, & Judd (2014) and using their power calculator (jakewestfall.org/power/), we conducted a sensitivity analysis for detecting quartile differences for focused comparisons involving relevant subsets of our participants (61 of 112) and stimuli (10 of 20 stimuli). For these comparisons, we had 80% power to detect effects with a Cohen's $d = .73$. This is reasonable considering that the relevant effects sizes for 1st-vs-4th quartile comparisons (where expected) in Burson, Larrick, & Klayman (2006) ranged between $d = 0.82$ and $d = 2.48$.

Procedure and measures. Participants were tested at individual terminals. After the consent process and general instructions about percentiles, participants completed a set of percentile items—one for each of the 20 behaviors—in random order. The items focused on social comparative standing of behavior frequency, and asked participants to place themselves relative to 99 other people who took the study. For example:

Assume there are 99 other people in the study and we ordered them according to how often they eat breakfast in the morning. Slide the scale to indicate where you place among 99 other people in the study. I eat breakfast more than ___ of the other people in the study.

The wording for these percentile items was always the same except for the specified behavior.

The participant used a slider scale with a numerical readout to respond between 0 and 99. A reminder of what responses like 99, 50, and 0 would mean always appeared in small font above an item (e.g., “0 would mean you do this behavior the least of anyone else in the study.”). A row of 99 person icons always appeared below the slider scale, as a reminder of the social comparative nature of the scale.¹

Next, participants responded to 20 nonsocial frequency or proportion questions—henceforth called absolute questions.² For some behaviors, the absolute question asked for the frequency the participant engaged in the behavior within a particular time-period. For example, for *washing hands*, participants estimated how many times they performed that behavior in a typical day. For *seeking advice from a doctor*, they estimated how many times that occurred in a typical year. Each question was answered on a dropdown menu with a range of responses. For other behaviors, the absolute question requested a nonsocial proportion. An example is: *What percentage of time do you cover your mouth when you cough or sneeze?* (answered from 0 to 100 in a text-entry box).

Finally, before demographic questions and a debriefing, participants gave 20 personal-frequency ratings (1=*I don't do this*; 6=*I do this very frequently*) and 20 commonness-among-students ratings (1=*very uncommon*; 5=*very common*) in two counterbalanced blocks. No other measures or manipulations were included in the study.

Table 1. *Study 1 Frequency, Commonness, and Percentile Means per Behavior*

Behavior	Mean Frequency Rating	Mean Commonness Rating	Mean Subjective Percentile
Healthy/Common			
Washing hands	3.78 (1.31)	3.25 (0.96)	65.79 (21.82)
Using turn signal when you should	4.31 (1.15)	3.56 (1.15)	82.50 (17.26)
Talking to friends and family	3.43 (1.31)	3.76 (1.08)	56.12 (28.39)
Walking > 5 minutes	4.44 (0.85)	4.42 (0.73)	71.30 (21.03)
Hanging out with friends	3.80 (1.24)	4.60 (0.73)	60.16 (23.86)
Healthy/Uncommon			
Climbing 2+ flights of stairs	3.54 (1.37)	3.27 (1.19)	60.74 (25.80)
Eating breakfast	2.61 (1.69)	2.57 (1.17)	38.99 (31.92)
Eating fish	1.92 (1.42)	2.07 (0.87)	35.06 (28.06)
Eating vitamin-rich food	3.07 (1.07)	2.66 (0.96)	50.01 (22.48)
Going to the doctor	1.93 (1.19)	2.45 (0.93)	37.24 (23.95)
Unhealthy/Common			
Drinking caffeinated beverages	2.36 (1.72)	4.07 (1.43)	40.37 (29.45)
Rubbing eyes (without washing hands)	3.17 (1.28)	3.86 (1.07)	58.52 (25.18)
Listening to loud music through headphones	3.10 (1.70)	4.36 (1.00)	50.96 (28.37)
Engaging in too much screen time	4.33 (0.94)	4.69 (0.59)	66.19 (21.10)
Eating fast food	2.22 (1.29)	3.79 (1.29)	35.83 (24.08)
Unhealthy/Uncommon			
Adding salt to food	1.87 (1.45)	3.18 (1.06)	33.64 (27.65)
Changing lanes without looking	1.41 (1.10)	3.12 (1.25)	30.34 (23.42)
Drive to a place within walking distance	2.24 (1.56)	3.15 (1.13)	37.93 (25.63)
Getting too much sun exposure	2.49 (1.52)	3.59 (1.32)	43.61 (27.43)
Text/Call while driving	1.86 (1.31)	3.70 (1.21)	35.83 (24.10)

Note: N = 112. Parentheticals reflect standard deviations. For frequency ratings, participants rated the frequency they engaged in each behavior from 1 (never) - 6 (very frequently). For commonness, they rated how common each behavior was among college students, from 1 (very uncommon) - 5 (very common). Percentile estimates were from 0-99.

Results

Preliminary issues. Data from the two final blocks of questions confirmed that the set of behaviors we classified as common were rated as more frequent and common than those we classified as uncommon ($ps < .001$). See online Supplementary Materials for additional analyses of these data.

The main analyses in this Results section are patterned on those from previous papers on the unskilled and unaware effect, particularly Burson et al. (2006; see also Kruger & Dunning, 1999, 2002). They rely on standard ANOVA and regression approaches. However, a mixed-effects modeling approach to the key analyses support essentially identical conclusions and can be found in the Supplementary Materials.

Main Analyses

Overall effects on percentile estimates. For each of the 20 behaviors, the percentile estimates should, from an objective perspective, center around the 50% percentile for the full sample. More precisely, they should average to 49.5%, given the 0-99 range of percentiles. In actuality, the means of percentile estimates ranged from 30.3 to 82.5 across behaviors. Our initial questions of interest were how participants' percentile estimates were impacted by the healthiness and commonness of the behaviors. We created composites (averages) across the 5 behaviors within each of the four categories of behaviors. These four composites were submitted to a 2 (Healthiness) x 2 (Commonness) repeated measures ANOVA. Figure 1 depicts the pattern of results.

Consistent with favorable overplacement, a main effect of healthiness revealed that people's percentile estimates were higher for healthy behaviors ($M = 55.79$, $SD = 11.61$) than for unhealthy behaviors ($M = 43.64$, $SD = 11.12$), $F(1, 111) = 66.68$, $p < .001$, $\eta_p^2 = .375$, 95% CI of

the difference [9.07, 15.22]. In fact, the estimates for healthy behaviors were significantly above the 50th percentile, $t(111) = 5.28$, $p < .001$, $d = 0.49$, 95%CI [3.61, 7.96], whereas estimates for unhealthy behaviors were significantly below the 50th percentile, $t(111) = -6.06$, $p < .001$, $d = -0.57$, 95%CI [-8.44, -4.28].

Figure 1. Participants' subjective percentile estimates for the four categories of behaviors from Study 1. Error bars denote standard errors.

The predicted main effect of rarity/commonness was also significant, $F(1, 111) = 223.51$, $p < .001$, $\eta_p^2 = .668$, 95%CI of the difference [15.88, 20.84]. Percentile estimates were higher for behaviors in the common set ($M = 58.76$, $SD = 9.71$) than for behaviors in the uncommon set ($M = 40.39$, $SD = 10.89$). In fact, the means for the two sets were significantly above and below the 50th percentile, respectively, $t(111) = 9.55$, $p < .001$, $d = 0.90$, 95%CI [6.94, 10.57] and $t(111) = -9.34$, $p < .001$, $d = -0.88$, 95%CI [-11.65, -7.57].

Finally, the Healthiness x Commonness/Rarity interaction was also significant, $F(1, 111) = 16.18$, $p < .001$, $\eta_p^2 = .127$. This unanticipated effect was smaller in magnitude relative to the main effects. Both simple effects of healthiness were significant ($ps < .001$), but the interaction

reveals that the effect was moderately stronger among the common behaviors than the uncommon ones (see pattern in Figure 1).

Testing the unhealthy-and-unaware effect and the noise-plus-bias model. The analyses in the previous section revealed overall bias (i.e., sample-wide) and how the direction and magnitude of the bias were influenced by a behavior's healthiness and commonness. However, the unhealthy-and-unaware hypothesis and the noise-plus-bias model make more specific predictions about who will be most biased and when. Whereas studies involving perception of skills have used test performance as a determinant of a person's actual percentile, we used participants' absolute frequency reports (e.g., How many times did you do X) to estimate their actual percentile and determine the quartile to which they belonged.³ As mentioned earlier, the analytic approach here is similar to Burson et al. (2006; see also Kruger & Dunning, 1999, 2002), but mixed-effects modeling described in the Supplemental Materials support the same conclusions.

Figure 2 concerns healthy behaviors only, and it plots subjective percentile estimates as a function of objective quartile. Each data point is a mean across the 10 healthy behaviors. The shallow slope of the line for subjective percentile estimates reflects that people were imperfect in their estimates; the correlations between people's estimated and actual percentiles ranged from .26 to .84 across the 10 behaviors, with a mean correlation of .58. Figure 3 shows results for unhealthy behaviors, for which the estimated-actual correlations ranged from .39 to .81, with a mean correlation of .60.

Because Figure 2 focuses on healthy behaviors, the results for the least healthy people (per a given behavior) are aggregated under Quartile 1, and results for the most healthy people are aggregated under Quartile 4. The reverse is true for Figure 3, because it focuses on unhealthy

behaviors.

Figure 2. Plots of means for percentile estimates regarding healthy behaviors from Study 1. The solid line plots the mean subjective percentile estimates as a function of actual quartile status. The dotted line plots the actual (objective) percentile mean per actual quartile. Error bars denote standard errors.

We used paired t-tests to compare the subjective vs. actual mean percentiles, within quartiles. Because a given person served in different quartile groups across behaviors in an uncontrolled manner, this precludes conducting quartile analyses with participants as the unit of analysis. Therefore, per-behavior means were the unit of analysis. First, we report findings for questions about healthy behaviors (Figure 2). The most unhealthy quartile of participants on a given behavior—those who were in the bottom quartile and whose *actual* percentiles necessarily centered around 12.5—significantly overestimated their percentiles ($M_{estimate} = 37.53$, $SD = 20.12$), $t(9) = 3.943$, $p = .003$, $d_{rm} = 1.79$, 95% CI[10.71, 39.52].⁴ The most healthy quartile of participants—those who were in the top quartile and whose actual percentiles necessarily centered around 87.5—significantly underestimated their percentiles ($M_{estimate} = 74.72$, $SD =$

11.26), $t(9) = -3.113$, $p = .012$, $d_{rm} = -1.63$, 95% CI [-19.86, -3.15].

Next we report findings regarding unhealthy behaviors (Figure 3). The most unhealthy participants (i.e., the top quartile) significantly underestimated their percentiles ($M_{estimate} = 63.89$, $SD = 11.46$), $t(9) = -6.34$, $p < .001$, $d_{rm} = -2.59$, 95% CI [-30.99, -14.68]. The most healthy participants (i.e., the bottom quartile) significantly overestimated their percentiles ($M_{estimate} = 23.95$, $SD = 13.12$), $t(9) = 2.841$, $p = .019$, $d_{rm} = 1.25$, 95% CI [2.40, 21.16].

Figure 3. Plots of means for percentile estimates regarding unhealthy behaviors from Study 1. The solid line plots the mean subjective percentile estimates as a function of actual quartile status. The dotted line plots the actual percentile mean per actual quartile. Error bars denote standard errors.

These results are generally consistent with the unhealthy-and-unaware hypothesis, but the more critical prediction for that hypothesis is that unhealthy people will be more miscalibrated in their percentile estimates than will healthy people. To prepare for this analysis, we computed a subjective-vs.-actual percentile deviation score for each behavior per participant. Similar to Burson et al. (2006), we coded the signs of the deviation scores in a matter that kept the signs in a consistent direction for comparisons among quartiles. Namely, if the participant's quartile

group generally underestimated their actual percentiles, the deviation score for that participant was computed as actual minus subjective. But if the participant's quartile group generally overestimated their actual percentiles, the deviation score for that participant was computed as subjective minus actual. The deviation scores were then aggregated into 80 averages—one per quartile for each of the 20 behaviors. See Table 2 for descriptive statistics for each category of behavior.

Table 2. Study 1 Mean Deviation Scores Per Behavior Category and Actual Quartile Standing

Behavior Category	Mean Percentile Deviation Scores			
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Healthy/Common	38.36 (16.53)	28.86 (10.58)	10.53 (10.50)	8.59 (2.47)
Healthy/Uncommon	12.19 (13.39)	9.23 (8.12)	14.87 (10.33)	17.75 (12.01)
Unhealthy/Common	16.68 (17.28)	15.02 (9.03)	11.02 (10.78)	15.73 (8.45)
Unhealthy/Uncommon	5.89 (5.27)	10.94 (7.34)	21.85 (7.78)	29.95 (9.72)

Note: Parenthetical numbers reflect standard deviations. Mean deviation scores reflect the difference scores between subjective and objective percentiles (directionally signed per quartile as described in the text).

The average deviation from the first and fourth quartiles were submitted to a 2 (Healthiness) x 2 (Commonness) x 2 (1st or 4th Quartile) repeated-measures ANOVA.⁵ The only significant effects from this analysis were two of the two-way interactions (all other $ps > .10$). The first of these, a significant Healthiness x Quartile interaction, directly supports the unhealthy-and-unaware hypothesis, $F(1,16) = 8.32$, $p = .011$, $\eta_p^2 = .342$. For healthy behaviors, the deviation scores were directionally larger in the 1st quartile ($M = 25.28$, $SD = 19.78$) than in the 4th ($M = 13.17$, $SD = 9.50$, $p = .053$, 95% CI [-0.19, 24.41]), but for unhealthy behaviors the deviation scores were directionally larger in the 4th quartile ($M = 22.84$, $SD = 11.40$) than in the 1st ($M = 11.28$, $SD = 13.32$, $p = .064$, 95% CI [-0.74, 23.85]). In other words, the significant interaction shows that people who are most unhealthy regarding a particular behavior tend to be

most miscalibrated in their estimation of their percentile.

The second significant interaction from the ANOVA directly tests the noise-plus-bias hypothesis. See Figure 4 for a depiction of these results.

Figure 4. Plots of means for percentile estimates for common and uncommon behaviors from Study 1. The solid lines plot the mean subjective percentile estimates as a function of actual quartile status. The dotted line plots the actual percentile mean per actual quartile standing (averaged across common and uncommon behaviors). Error bars denote standard errors.

The Commonness x Quartile interaction was significant and revealed the expected pattern $F(1, 16) = 13.53, p = .002, \eta_p^2 = .458$. For common behaviors, the deviation scores were larger in the 1st quartile ($M = 27.52, SD = 19.61$) than in the 4th ($M = 12.16, SD = 6.97, p = .018, 95\%CI [3.07, 27.66]$), but for uncommon behaviors the deviation scores were larger in the 4th quartile ($M = 23.85, SD = 12.15$) than in the 1st ($M = 9.04, SD = 10.16, p = .021, 95\%CI [2.51, 27.10]$). This pattern reveals that, separate from an unhealthy-and-unaware pattern, the determination of who is most miscalibrated (1st or 4th quartile participants) significantly depends on the commonness of the behavior in question.

Study 1 Discussion

Study 1 revealed evidence of four patterns: 1) favorable overplacement—participants estimated their percentile standing to be higher than warranted for healthy behaviors and lower than warranted for unhealthy behaviors, 2) rarity-commonness effect—participants estimated their percentile standing to be higher for common behaviors than uncommon behaviors, 3) unhealthy and unaware—participants who were the most unhealthy on a given behavior were the most miscalibrated on their percentile estimates, and 4) noise plus bias—who was most miscalibrated depended on the commonness or rarity of a given behavior.

As discussed earlier, the first two effects—favorable overplacement and the rarity-commonness effect—have been primarily associated with motivated and nonmotivated mechanisms, respectively. Favorable overplacement is often attributed to motivated concerns such as avoiding anxiety or maintaining self-esteem and feelings of security (e.g. Brown, 2012; Klein & Kunda, 1993; Kunda, 1990; Weinstein, 1980; but see also Chambers & Windschitl, 2004). The rarity-commonness effect has been linked to the cognitive bias of egocentrism, whereby people judge their comparative standing by mostly by considering how much they do a behavior without fully weighting similar thoughts about others (Chambers et al., 2003; Kruger & Burrus, 2004; see also Dunning & Hayes, 1996; Weinstein & Lachendro, 1982). For a common behavior, for example, this meant people's social-comparative beliefs were primarily influenced by thoughts about how they did the behavior a lot—failing to adequately adjust for the fact that most other participants also did the behavior a lot.

Neither the unhealthy-and-unaware effect nor the noise-plus-bias model have previously been examined in the context of health behavior in published research, so inferring the underlying causal mechanisms is somewhat speculative. In the noise-plus-bias effects described

by Burson et al. (2006), a key component was the bias in how people responded about easy vs. hard tasks (Kruger, 1999). Analogous to this, in our demonstration of the noise-plus-bias effect, a key component was bias in how people responded about rare vs. common behaviors. Again, people were egocentrically biased—causing overestimates of percentile standing for common behaviors and underestimates for rare behaviors. Although the assertion of egocentrism playing a role in the bias part of the noise-plus-bias model is generally well founded given various pieces of prior research (e.g., Chambers et al., 2003; Kruger & Burrus, 2004), our first study did not contain measures that could yield a direct test of egocentrism. In our second study, we added a measure of people's beliefs about absolute frequencies of the average person's respondents behavior. The addition of this measure allowed us to conduct regression analyses that have been commonly used to detect egocentric weighting in comparative evaluations (e.g., Kruger, 1999).

Regarding the unhealthy-and-unaware effect, both motivated and nonmotivated accounts are plausible, and they are not mutually exclusive. A motivated explanation suggests that when people are relatively unhealthy on a given behavior, they are reluctant to believe that their social comparative standing is especially unfavorable (Klein & Kunda, 1993; Miles & Scaife, 2003; Renner, Schütz, & Sniehotta, 2008). A nonmotivated explanation bears some resemblance of the traditional metacognitive explanation offered for the unskilled and unaware effect in skill domains (that unskilled individuals lack the metacognitive skills needed to recognize when their performance is poor; Kruger & Dunning, 1999, 2002; Schlösser et al., 2013). It would suggest that people who are unhealthy on a given behavior also tend to be poor in their awareness of the norms for that behavior or how often the average person engages in that behavior, making them poor judges of their comparative standing. In Study 1, because we did not ask participants to give absolute judgments of other people's behavior frequencies, we are not able to assess whether

healthy and unhealthy participants differ in accuracy in such estimates. This will be addressed in Study 2.

However, one relevant additional analysis that we can do on Study 1 data is to examine the correlational accuracy or sensitivity of people's percentile estimates, which is different from the analyses already reported on calibration (see Burson et al., 2006). Within each quartile group for a given behavior, we computed the Fisher-transformed correlation between participant's subjective and actual percentiles (the grand M for raw correlations was $r = .26$). In a 2x2 ANOVA on the correlations in the first and fourth quartiles as a function of behavior healthiness, the interaction was not significant, $F(1, 17) < 1.00$. That is, the relative ordering of subjective percentiles was no less accurate for the unhealthy quartile than for the healthy quartile. In short, although the primary analyses showed that percentile estimates for unhealthy people were uniquely miscalibrated—shifted in a favorable direction—analyses of correlational accuracy show that the relative order of their estimates within their quartile were not especially jumbled.

A different type of nonmotivated explanation for the observed unhealthy-and-unaware effect posits that there may be cognitive-ability differences between people who tend to be unhealthy (and classified in unhealthy quartiles) across the 20 behaviors we tested, versus those who tend to be healthy. Perhaps generally unhealthy people have poorer cognitive capacity for judging percentiles. However, we started this project with the assumption that a person's levels of relative healthiness would vary and be largely independent across the 20 behaviors. If so, it is unlikely that any cognitive capability would correlate highly with average percentile standing for healthiness. To examine this issue, we coded all objective percentiles in the same direction for healthiness (i.e., a high percentile meant they were healthy on that behavior) and computed inter-item correlations (i.e., inter-behavior correlations). The average inter-item correlation was .05,

which suggests a great deal of independence on people's healthiness standings across specific behaviors. We also calculated each participant's average objective percentile for the 10 unhealthy behaviors and for the 10 healthy behaviors. The correlation between those averages was only $r = .082$, ($p = .391$, $n = 110$), revealing no significant relationship.⁶ Given these results, it seems unlikely that cognitive-ability differences between people who tend to be unhealthy versus healthy would explain the unhealthy-and-unaware effect we observed. Study 2 also addresses this issue.

Study 2

Study 1 is the first that we know of to document the unhealthy-and-unaware effect and the noise-plus-bias model regarding health behaviors. Given the novelty and potential significance of these findings, it is important to test whether they replicate. Study 2 is a direct replication attempt of Study 1, with one key change and a number of extensions. The one key change is in how the reference group was specified to participants. In Study 1, participants were not given explicit information about who was in the study; the percentile items asked participants to place themselves relative to "other people in the study." We assumed that participants would rightly believe that the study was full of other students from their university, but we can't be assured of that assumption. Therefore, in Study 2, participants were explicitly informed that the other students were University of Iowa students, and the percentile items asked participants to place themselves relative to "other University of Iowa students in the study." This is an important change because the interpretation of biases like the favorable overplacement effect get murky if participants misunderstand who is in the relevant comparison group.

The extensions added to Study 2 allow us to conduct more analyses relevant to possible explanations for the unhealthy-and-unaware effect and the noise-plus-bias model. We added a

measure of people's beliefs about the absolute frequency with which the average respondent does each behavior (i.e., absolute-other questions). This allows for: 1) a more focused analysis of whether participants who were healthy vs. unhealthy on a given behavior differed in how accurate they were about other people's behavior and 2) regression analyses to detect egocentric weighting in people's percentile estimates (e.g., Kruger, 1999).

We also added the 7-item expanded version of the Cognitive Reflection Test (CRT; Toplak, West, & Stanovich, 2014) in Study 2 to examine if individual differences in cognitive capability could be related to percentile miscalibration. We chose the CRT because it has shown to be significantly associated with numeracy (Bialek & Pennycook, 2018; Liberali, Reyna, Furlan, Stein, & Pardo, 2012) and metacognitive monitoring (Mata, Ferreira, & Sherman, 2013; Pennycook, Ross, Koehler, & Fugelsang, 2017). Performance on the CRT can also be used as a metric of general mental ability (Blacksmith, Yang, Behrend, & Ruark, 2019), as well as a marker of people's willingness to think analytically and use their cognitive resources (Frederick, 2005; Šrol, 2018). By using this measure and also self-reports of ACT or SAT scores, we can examine if individual differences in cognitive ability are related to participants' healthiness and/or quartile standing. Although we expected that individual differences in cognitive capability would not be related to participants' objective healthiness, it is important to examine that as a possible alternative explanation.

Finally, we added two sets of measures to conduct a manipulation check for our healthiness categorizations of the 20 behaviors.

Method

The design, materials, and procedures of Study 2 were identical to those of Study 1, with the following exceptions. First, the wording of the percentile questions included explicit

reference to University of Iowa students: “Assume there are 99 other University of Iowa students in the study...Slide the scale to indicate where you place among 99 other University of Iowa students in the study...”.

A second change was that after participants completed the 20 self-absolute questions, they completed the newly added *absolute-other* questions. For each of the 20 behaviors, they gave an absolute estimate about behavior of the “typical University of Iowa student.” The soliciting questions and response options were otherwise identical to those used for the absolute-self questions. See the Online Supplementary Materials for a full list of the absolute-other questions.

A third change was that two sets of healthiness items were answered after the other manipulation check measures. These first of these healthiness measures, which were the same as those used in pilot testing for Study 1, asked: “Would you consider these behaviors to be positive health behaviors or negative health behaviors?”—with response options of positive, negative, or neither/neutral. The second asked: “Is it better to do these behaviors more or less often than the average person does?”—with response options of more than, less than, or about the same as the average person. Participants gave separate responses for each of the 20 behaviors listed below a given question prompt.

A fourth change was the addition of the 7-item expanded Cognitive Reflection Test (CRT) administered right before the demographic questions (Bialek & Pennycook, 2018; Frederick, 2005; Srol et al., 2018). Among the demographic questions, we also asked participants to report their ACT scores or SAT scores.

Design, participants, and statistical power. As in Study 1, the design was a 2 (valence: healthy/unhealthy) x 2 (commonness: common/uncommon) within-subject factorial. When pre-

registering our sample size on the Open Science Framework (osf.io/ec6tp), we indicated we would collect data from a minimum of 80 participants, but up to as many we could test until the end of the then-current semester, or a maximum of 150. This ultimately yielded a sample size of 114 University of Iowa undergraduates (58 women, 55 men, 1 unreported, $M_{age} = 19.19$, $SD = 1.00$), which provided essentially the same statistical power as we had in Study 1, where $N = 112$.

Results and Discussion

Preliminary issues. Analyses of data from the four blocks of manipulation check questions confirmed that our a priori classifications established from pilot testing and used in Study 1 remained valid when used in Study 2. The set of behaviors we classified as common were rated as more frequent and common than those we classified as uncommon ($ps < .001$). The set of behaviors we classified as healthy were rated as more positive and better to do more of than those we classified as unhealthy ($ps < .001$). See Online Supplementary Materials for additional analyses regarding the manipulation checks.

Main analyses. As was done for Study 1, the main analyses using standard ANOVAs were patterned on those from previous papers on the unskilled and unaware effect (e.g., Burson et al., 2006). A mixed-effects modeling approach to the key analyses support essentially identical conclusions and can be found in the Online Supplementary Materials. To overview our findings, each of the four key patterns found in Study 1 was successfully replicated in Study 2.

Overall effects on percentile estimates. To test for both favorable overplacement and the rarity-commonness effect, we created composites across the five behaviors within each of the four categories of behaviors and submitted the composites to a 2 (Healthiness) x 2 (Commonness) repeated measures ANOVA.

Consistent with Study 1, evidence of both favorable overplacement and the rarity-commonness effect was found (see Figure 5). A main effect of healthiness showed that people's percentile estimates were higher for healthy behaviors ($M = 54.01$, $SD = 11.66$) than for unhealthy behaviors ($M = 41.55$, $SD = 11.02$), $F(1, 113) = 78.55$, $p < .001$, $\eta_p^2 = .410$, 95% CI of the difference [9.59, 15.32]. The former estimates were significantly above the 50th percentile, $t(113) = 3.671$, $p < .001$, $d = 0.34$, 95% CI [1.85, 6.17], whereas the latter estimates were below the 50th percentile, $t(113) = -8.19$, $p < .001$, $d = -0.77$, 95% CI [-10.49, -6.40]. A main effect of rarity/commonness showed that percentile estimates were higher for common behaviors ($M = 56.95$, $SD = 10.73$) than for uncommon behaviors ($M = 38.54$, $SD = 10.17$), $F(1, 113) = 240.24$, $p < .001$, $\eta_p^2 = .680$, 95% CI of the difference [16.06, 20.77]. The former estimates were significantly above the 50th percentile, $t(113) = 6.92$, $p < .001$, $d = 0.65$, 95% CI [4.96, 8.94], whereas the latter estimates were below, $t(113) = -12.04$, $p < .001$, $d = -1.13$, 95% CI [-13.35, -9.57].

The Healthiness x Commonness/Rarity interaction was not significant, $F(1,113) = 1.65$, $p = .201$. Although there was a significant interaction in the first study, that effect was unanticipated and therefore we have no speculations on why the interaction pattern did not replicate.

Figure 5. Participants' subjective percentile estimates for the four categories of behaviors from Study 2. Error bars denote standard errors.

Testing the unhealthy-and-unaware effect and the noise-plus-bias model. In the same way as Study 1, analyses regarding the unhealthy-and-unaware effect and the noise-plus-bias model used participants' absolute frequency reports to estimate their objective percentile and assign them to their respective quartile. This allows us to both compare subjective and objective percentiles, as well as see which quartile was the most miscalibrated in their estimates.

For healthy behaviors (Figure 6), the most unhealthy of participants, those who were in the first quartile, significantly overestimated their percentiles ($M_{estimate} = 32.96$, $SD = 19.68$), $t(9) = 3.05$, $p = .014$, $d_{rm} = 1.70$, 95%CI of the difference [5.18, 35.09]. The most healthy of participants, those in the fourth quartile, significantly underestimated their percentiles, ($M_{estimate} = 73.97$, $SD = 11.40$), $t(9) = -3.50$, $p = .007$, $d_{rm} = -1.51$, 95% CI of the difference [-20.72, -4.44]. For unhealthy behaviors (Figure 7), the most unhealthy of participants significantly underestimated their percentiles ($M_{estimate} = 63.71$, $SD = 12.14$), $t(9) = -5.99$, $p < .001$, $d_{rm} = -2.58$, 95%CI of the difference [-31.66, -14.32], while the most healthy participants directionally, but

not significantly, overestimated their percentile ($M_{estimate} = 20.11$, $SD = 13.14$), $t(9) = 1.81$, $p = .103$, $d_{rm} = 0.86$, 95% CI of the difference $[-1.88, 17.10]$.

Figure 6. Plots of means for percentile estimates regarding healthy behaviors from Study 2. The solid line plots the mean subjective percentile estimates as a function of actual quartile status. The dotted line plots the actual percentile mean per actual quartile. Error bars denote standard errors.

Figure 7. Plots of means for percentile estimates regarding unhealthy behaviors from Study 2. The solid

line plots the mean subjective percentile estimates as a function of actual quartile status. The dotted line plots the actual percentile mean per actual quartile. Error bars denote standard errors.

These results are generally consistent with the unhealthy-and-unaware hypothesis, but the more critical analysis for that hypothesis is whether unhealthy people are more miscalibrated in their percentile estimates than healthy people. To prepare for this analysis, we computed subjective-vs.-actual percentile deviation scores as we did in Study 1. These scores were then aggregated into 80 averages—one per quartile per behavior (see Table 3).

Table 3. Study2 Mean Deviation Scores Per Behavior Category and Actual Quartile Standing

Behavior Category	Mean Percentile Deviation Scores			
	1st Quartile	2nd Quartile	3rd Quartile	4th Quartile
Healthy/Common	36.12 (10.01)	24.06 (10.14)	9.82 (9.24)	10.13 (8.88)
Healthy/Uncommon	11.17 (10.44)	16.22 (9.72)	14.25 (8.99)	15.03 (14.06)
Unhealthy/Common	14.94 (14.57)	16.30 (5.66)	7.71 (7.71)	16.06 (7.25)
Unhealthy/Uncommon	4.50 (4.95)	13.97 (6.63)	23.49 (6.66)	29.92 (12.57)

Note: Parenthetical numbers reflect standard deviations. Mean deviation scores reflect the difference scores between subjective and objective percentiles (directionally signed per quartile as described in the text).

The average deviations from the first and fourth quartiles were submitted to a 2 (Healthiness) x 2 (Commonness) x 2 (Quartile) repeated-measures ANOVA. Similar to Study 1, there were two significant effects of interests from this analysis—two of the two-way interactions. First, a Healthiness x Quartile interaction replicates the unhealthy-and-unaware effect, $F(1, 16) = 9.29, p = .008, \eta_p^2 = .367$. For healthy behaviors, the deviation scores were directionally larger in the first quartile ($M = 23.65, SD = 16.31$) than in the fourth quartile ($M = 12.58, SD = 11.38, p = .068, 95\% CI [-0.91, 23.03]$), and for unhealthy behaviors the deviation scores were significantly larger in the fourth quartile ($M = 22.99, SD = 12.12$) than in the first quartile ($M = 9.72, SD = 11.64, p = .032, 95\% CI [-25.24, -1.30]$). In other words, people who

were most unhealthy were also the most miscalibrated.

The second significant interaction of interest, the Commonness x Quartile interaction, directly supports the noise-plus-bias model, $F(1, 16) = 11.50$, $p = .004$, $\eta_p^2 = .418$. Figure 8 shows the relevant pattern.

Figure 8. Plots of means for percentile estimates for common and uncommon behaviors from Study 2. The solid lines plot the mean subjective percentile estimates as a function of actual quartile status. The dotted line plots the actual percentile mean per actual quartile standing (averaged across common and uncommon behaviors). Error bars denote standard errors.

For common behaviors, the deviation scores were larger in the first quartile ($M = 25.53$, $SD = 16.23$) than the fourth ($M = 13.10$, $SD = 8.26$, $p = .043$, 95% CI [0.47, 24.41]), but for uncommon behaviors, the deviation scores were larger in the fourth quartile ($M = 22.48$, $SD = 14.82$) than the first ($M = 7.83$, $SD = 8.47$, $p = .020$, 95% CI [-26.61, -2.67]). This noise-plus-bias pattern reveals that, separate from the unhealthy-and-unaware pattern, the determination of who is most miscalibrated (1st or 4th quartile) depends on the commonness or rarity of the given behavior.

Lastly, there was an unanticipated, small but significant, Healthiness x Commonness interaction, $F(1,16) = 4.66$, $p = .046$, $\eta_p^2 = .226$. The deviations differed more between common

and uncommon healthy behaviors than between common and uncommon unhealthy behaviors. As this was an unexpected finding, we have no speculation on why it occurred in this study but not the first study.

Summary of Conclusions from Main Analyses. The above analyses revealed that all four of the key predicted patterns were significant, thereby replicating the findings from Study 1. One notable methodological difference between the studies was that in Study 2, the questions the solicited percentile estimates was explicit in specifying the referent group as other University of Iowa students. Therefore, the replicating results seen in Study 2 rule out the concern that the results in Study 1 were somehow tied to a tendency for participants to assume an inappropriate reference group when making percentile estimates.

Additional analyses relevant to underlying mechanisms. As mentioned previously, the added measures in Study 2 allow for additional analyses of potential underlying mechanisms. The first analysis is relevant to the commonness/rarity effect and noise-plus-bias effect. Recall that we assumed that people's tendency to egocentrically focus on self-relevant information more than other-relevant information is an underlying cause of these effects. To examine this directly, we had participants make absolute estimates of the typical student's behavior in addition to their own. In 20 separate analyses, one per behavior, we regressed subjective percentiles on self-absolute and absolute-other estimates. This type of regression analysis has been commonly used in other work to assess for differential weighting of self- and other-estimates when forming comparative judgments (e.g., Giladi & Klar, 2002; Klar & Giladi, 1997; Kruger, 1999; Moore & Kim, 2003; Windschitl, Kruger, & Simms, 2003).⁷ Here, we report a summary of these findings, but see Online Supplementary Materials for a table with the results of each of the 20 analyses.

The regression coefficients for the self-estimates were positive and significantly different

from zero in each of the 20 analyses (all $ps < .001$), with standardized betas ranging from .290 to .941 (mean $\beta = .614$). If absolute-other estimates had the same weight in determining subjective percentiles, then the standardized betas for absolute-other estimates should be negative and roughly equivalent in absolute magnitude as those for the absolute-self estimates. However, although the standardized betas for absolute-other estimates were negative in sign for 17 behaviors, they were significant for only 10 of the behaviors, ranging from -.532 to .239 (average $\beta = -.116$). A paired samples t -test comparing the absolute magnitudes of two types of betas across the 20 behaviors revealed that the betas for self-estimates ($M = .614$, $SD = .20$) were stronger than those for other-estimates ($M = .159$, $SD = .11$), $t(19) = 10.12$, $p < .001$, $d_{rm} = 2.77$, 95%CI [0.36, 0.55]. This is consistent with the notion that participants were at least somewhat egocentric when making their percentile estimates, and that this played a role in the commonness/rarity effect and, by extension, the noise-plus-bias effect.

The remaining analyses are more relevant to the unhealthy-and-unaware effect. Recall that a potential nonmotivated explanation suggests that people who are unhealthy on a given behavior might also tend to be poor in their awareness of the norms for that behavior or how often the average person engages in that behavior, making them poor judges of their comparative standing. Meanwhile, people who are healthy on a behavior may be more aware of norms and the tendencies of others, making them better judges of comparative standing. In Study 1, we were able to examine this idea in an indirect way testing the correlational accuracy or sensitivity of people's percentile estimates. The analysis revealed that relative ordering of subjective percentiles was no less accurate for the unhealthy quartiles than for the healthy quartiles. We did this same analysis for Study 2. Within each quartile group for a given behavior, we computed the Fisher-transformed correlations between participants' subjective and actual percentiles (the

grand M for raw correlations was $r = .32$). In a 2x2 ANOVA on the correlations in the first and fourth quartiles as a function of behavior healthiness, the interaction was not significant, $F(1, 16) = 3.55, p = .078, \eta_p^2 = .18$. In line with Study 1, this suggests that the ordering of subjective percentiles was no less accurate for the unhealthy quartile than the healthy quartile.

However, with the addition of the absolute-other estimates in Study 2, we can conduct a more direct test of the idea that people who are unhealthy on a given behavior also tend to be poorer in their awareness of the descriptive norms for that behavior. We first calculated how inaccurate each person's estimate was about the behavior frequency of the typical University of Iowa student. Specifically, we computed an absolute deviation score, per participant and behavior, between their absolute-other estimate and the sample's average of absolute-self estimates for that behavior (this sample average reflects an empirical estimate of the what the typical student actually does). Then we computed, separately for each behavior, the correlation between these absolute deviation scores and the respondents' level of healthiness on that behavior (operationalized as their objective percentile standing where higher percentile always means more healthy). These 20 correlations ranged from $-.42$ to $.23$ with their average ($M_r = -.16$) falling significantly below 0, $t(19) = -3.99, p = .001, d = -0.89, 95\% \text{ CI } [-0.25, -0.08]$. This indicates that the healthier a given person is on a behavior, the less inaccurate (or more accurate) they were at estimating other's behavior frequency.

To examine the same issue but narrow it down to just the 1st and 4th quartiles, we first z-scored the absolute deviation scores. Separately for each behavior, we computed the averages of these z-scores for the 1st quartile and for the 4th quartile. We submitted these two averages per behavior to a 2 (1st or 4th Quartile) x 2 (Healthy or Unhealthy) ANOVA. This produced a main effect of quartile, $F(1, 18) = 5.74, p = .028, \eta_p^2 = .242$, but this was qualified by a significant

interaction of interest, $F(1, 18) = 22.14, p < .001, \eta_p^2 = .552$. For healthy behaviors, the fourth quartile (i.e., those who do healthy behaviors the most, $M_z = .003, SD_z = .29$) was directionally more accurate than the first quartile (i.e., those who do healthy behaviors the least, $M_z = .200, SD_z = .25$), $p = .120$. For unhealthy behaviors, the first quartile (i.e., those who do unhealthy behaviors the least, $M_z = -.133, SD_z = .18$) was significantly more accurate than the fourth quartile (i.e., those who do unhealthy behaviors the most, $M_z = .472, SD_z = .20$), $p < .001$. This confirms that there is an accuracy discrepancy between people who are healthy and those who are unhealthy for a given behavior in estimates about others.

Finally, a different type of nonmotivated explanation for the observed unhealthy-and-unaware effect posits that certain people tend to fall into the healthy quartiles and others fall into the unhealthy quartiles, and that participants who tend to fall into the healthy quartiles also tend to be more accurate at giving percentile estimates, due to cognitive abilities or other characteristics. Recall that in Study 1, there was not strong evidence that percentile or quartile membership was substantially intercorrelated across behaviors. For Study 2 we repeated those analyses and extended them with analyses involving the CRT and ACT/SAT scores. First, we again coded all percentiles in the same direction for healthiness and computed inter-item correlations. The average correlation was only .037, again suggesting a great deal of independence in people's healthiness standings across specific behaviors. We also calculated each participant's average objective percentile for the 10 unhealthy behaviors and for the 10 healthy behaviors. The correlation between people's average percentiles for the 10 healthy behaviors and average percentiles for the 10 unhealthy behavior was $r(112) = -.01, p = .894$, revealing no significant relationship.⁸

Despite these results, we checked whether a person's score on the CRT (Toplak et al.,

2014) was correlated with their general level of healthiness (operationalized as the average of their percentile standings, when all standings were coded such that higher is healthier). This correlation was not significant, $r(112) = -.055$, $p = .558$. General healthiness was also not significantly correlated with participant-reported ACT scores, $r(100) = .004$, $p = .971$.⁹ In short, the evidence is not in favor of the idea that a general cognitive ability or characteristic plays a key role in explaining the unhealthy-and-unaware effect.

General Discussion

In two studies, we tested four potential patterns of bias in how people estimate their social comparative standing for health-relevant behavior frequency. In both studies, there was evidence consistent with each of the four patterns. First, there was *favorable overplacement*, which is akin to better-than average effects in skill domains (Alicke & Govorun, 2005) and to comparative-optimism effects (Chambers & Windschitl, 2004; Shepperd et al., 2002). Participants estimated their percentile standing to be higher for healthy behaviors than for unhealthy behaviors. In fact, estimates tended to be too high for the healthy behaviors (averaging significantly above the 50th percentile) and too low for unhealthy ones (see Hoorens & Harris, 1998, for related findings). Second, there was a *rarity-commonness effect*, related to moderator effects found in other comparative-bias studies (e.g., Chambers et al., 2003; Kruger, 1999; Kruger & Savitsky, 2009; Moore & Small, 2007; Windschitl et al., 2003). Estimates of percentile standing tended to be too high for common behaviors and too low for uncommon ones. The third and fourth patterns concern the question of who in the sample was most miscalibrated. Similar to an unskilled-and-unaware pattern (Kruger & Dunning, 1999, 2002), participants who were most unhealthy regarding a particular behavior were the most miscalibrated about their social comparative standing on that behavior. Finally, consistent with the noise-plus-bias model

(Burson et al., 2006; Krueger & Mueller, 2002), the subset of participants who were most miscalibrated also depended on whether a given behavior was generally common or uncommon.

In papers on comparative bias, favorable overplacement and the rarity-commonness effect have been primarily associated with motivated and nonmotivated mechanisms, respectively (for review, see Chambers & Windschitl, 2004). Regarding motivated explanations, believing that one engages in healthy behaviors less than others, or engages in unhealthy behaviors more than others could make one feel uneasy, disappointed in oneself, or anxious about the future. Such motivated concerns are viable candidates for directional inaccuracies in percentile estimates (Brown, 2012; Klein & Kunda, 1993; Kunda, 1990; Scherer et al., 2016; Tyler & Rosier, 2009; Weinstein, 1980). For example, students in our study might have been motivated to selectively imagine the least healthy students from their university in order to justify overly favorable percentiles (Logg, Haran, & Moore, 2018; Perloff & Fetzer, 1986). People might also use a favorable view of themselves or their intentions as a top-down influence on how they estimate relative standing (e.g., “I’m trying to eat right, so I think I’m at least this percentile for eating vitamin-rich food”; see Critcher & Dunning, 2009; Ehrlinger & Dunning, 2003), which may also inform directional inaccuracies in percentile estimates.

Egocentrism is probably the most often-discussed version of a nonmotivated explanation for social-comparative bias, although related constructs such as focalism and self-other knowledge differences can also be relevant (Chambers & Windschitl, 2004; Kruger, 1999; Moore & Small, 2007). An egocentrism account posits that people form their percentile estimate mostly by considering a general impression of how much they do a behavior but do not fully weigh similar thoughts about others (Chambers et al., 2003; Dunning & Hayes, 1996; Kruger & Burrus, 2004; Weinstein & Lachendro, 1982).

In our second study, we directly tested for the presence of egocentric weighting. As expected, the regression analyses showed that participants' percentile estimates were more strongly related to their absolute estimates of their own behavior than their estimates of the typical student's behavior. This supports the notion that egocentrism was a critical underlying bias that yielded the commonness-rarity effect (see related arguments in Chambers et al., 2003; Klar, Medding, & Sarel, 1996; Kruger & Burrus, 2004). It would be difficult to explain the commonness-rarity effect without reference to egocentrism or a closely related construct. In addition, because the commonness-rarity effect is essentially a subcomponent of the noise-plus-bias effect as measured here, it is implicated in that effect as well. Egocentric processing and the commonness-rarity factor work together to determine what is the bias part of the noise-plus-bias model. When a sample of participants are asked to assess their social comparative standing but they actually think egocentrically, the overall sample tends to give estimates that are biased in an upward direction for common behaviors and downward for uncommon ones.

To account for the unhealthy-and-unaware effect, both motivated and non-motivated processes are quite plausible, and they are not mutually exclusive. Motivated concerns could make people who are relatively unhealthy on a given behavior reluctant to believe or acknowledge their true social comparative standing, but these same concerns might not bias people who are healthy for that behavior. In Study 2, we conducted analyses to assess possible nonmotivated mechanisms. On the one hand, we found that people who were unhealthy on a given behavior had more biased, but not more noisy, percentile estimates than people who were healthy on that behavior. On the other hand, we found that people who were unhealthy had more inaccurate frequency estimates for how often other people do behaviors than people who were healthy. This latter finding is consistent with, although not definitive about, the possibility that

differential insight plays a role in the observed effects—somewhat akin to the metacognitive explanation offered for the unskilled and unaware effect (Kruger & Dunning, 1999, 2002; Schlösser et al., 2013).

One type of nonmotivated account that did not fare well in these studies is that the miscalibration pattern that defines the unhealthy-and-unaware effect is due to individual differences between generally healthy people and generally unhealthy people. There was considerable independence in how comparative healthy people were across the 20 behaviors, and specific tests of a cognitive skills account yielded null effects.

Again, the candidate causes of the unhealthy and unaware effect are not necessarily mutually exclusive. In our title, “Unhealthy and Unaware...,” we raised the question of whether people who are most unhealthy regarding a behavior are also especially unaware of their social comparative standing. The present study does indeed suggest that the most unhealthy are also the most miscalibrated. As for whether “unaware” is the best description of this miscalibration, that is a matter that needs to be informed by further evidence. The moniker “unhealthy and defensive” may prove to be just as valid as “unhealthy and unaware” for capturing the major mechanisms underlying the miscalibration (see Ehrlinger et al., 2008 for related discussion).

Limitations

This research is not without limitations. One notable limitation is that we relied on the self-report of absolute behavior frequencies to determine people’s actual percentiles. We note, however, that this reliance allowed us to test far more behaviors than we otherwise could. Imagine attempting to secure physical or observational measurements over an extended time period across our list of behaviors; the difficulties are prohibitive. By testing an array of behaviors in four categories, we have drawn conclusions that would be impossible to arrive at

when using a smaller set of behaviors. Additionally, previous research suggests that people are fairly accurate at estimating their actual behavior frequency, including behaviors such as listening to music, talking to friends and family, getting too much screen time, and drinking alcohol, all of which are included in our study (Del Boca & Darkes, 2003; Vazire & Mehl, 2008).

Implications

These studies are the first that we know of to test for unhealthy-and-unaware effects and noise-plus-bias effects in the context of health-behavior beliefs. The observed patterns help answer questions of who shows the most bias and for what behaviors. The studies also highlight the importance of appreciating the multiple possible dynamics involved in causing or explaining miscalibration patterns—with some dynamics being motivated, others being nonmotivated, and yet another being the noise component that is part of the noise-plus-bias model (Burson et al., 2006). With this appreciation, it is understood that an unhealthy-and-unaware effect can be erased or exaggerated as a function of a behavior's overall commonness or by moderating variables that influence people's egocentric tendencies when making social comparative judgments.

This work has practical implications for the targeting of health interventions and communications. For example, consider the behavior of exercise. Perceived comparative standing has been shown to predict people's beliefs about the importance of increasing exercise frequency (Miller, Windschitl, Treat, & Scherer, 2019) as well as motivate people to complete their exercise goals (Diel & Hofmann, 2019). Our research, however, shows that these comparative estimates can be distorted because of both the healthiness and the commonness of the behavior. Given that exercise can be considered a relatively common, healthy behavior, both

the unhealthy-and-unaware effect and the noise-plus-bias pattern suggest that the people who exercise the least might be quite vulnerable to miscalibration when thinking about how they compare to others. More generally, the unhealthy-and-unaware effect would appear to fuel a pesky problem: the people who most need to change their behavior might be the least likely to recognize the need to change their behavior, which reduces the likelihood of behavior change. The noise-plus-bias model independently suggests that healthy behaviors that are generally common are ones for which there is a high risk of people overestimating their social comparative standing. This is a problem if, despite the commonness of the behavior, health experts agree that most people would benefit from doing more of it.

Potential interventions could help communicate and bring more attention to people's comparative standing, which in turn might trigger more awareness and motivate them to change behavior. Previous research has already shown that making people more aware of the descriptive norms of certain unhealthy behaviors such as alcohol consumption has been successful as an intervention in reducing the frequency of engagement in those behaviors (e.g., Ridout & Campbell, 2014; Rimal & Real, 2005). In addition, descriptive norm interventions have been shown to increase healthy behaviors, such as hand washing (Lapinski, Maloney, Braz, & Shulman, 2013). Other interventions might address possible motivated reasons for bias. If part of the unhealthy-and-unaware effect is due to engagement of defense mechanisms to reduce unhealthy persons' negative feelings about their health, then interventions that reduce defensiveness should be helpful (e.g., Croyle, Sun, & Hart, 1997; Klein & Kunda, 1993; Weinstein & Klein 1995; see also Van 't Riet & Ruiter, 2011). For example, self-affirmation interventions have been found to increase people's acceptance of potentially threatening information, and so they might also be used to make people more willing to accept and confront

their unfavorable standings on various health behaviors (Harris & Epton, 2009; Harris & Napper, 2005; Reed & Aspinwall, 1998).

The present work provides information about who is most inaccurate in their social comparative perceptions, and there are some good candidate solutions for reducing these inaccuracies in a targeted way. We end then with a pair of observations about the finding that people who are most unhealthy regarding a given behavior are also the most inaccurate. A quite positive view is that even modest attempts to target their social comparative beliefs could make a large difference, and thereby trigger behavior change. However, it is also worth noting that because these people are the furthest from being accurate, interventions aimed at the most unhealthy have more ground they must cover to bring people toward a realistic social comparative belief.

Open Practices

Pre-registration. Both studies were pre-registered on the Open Science Framework prior to any data collection, which can be viewed here:

Study 1: osf.io/9jqbz

Study 2: osf.io/ec6tp

Open materials. The materials and measures, including the Qualtrics survey file, are available at the following URLs:

Study 1: osf.io/msphv

Study 2: osf.io/yegrv

Open data. The data files, respective syntax, and variable keys are available on the Open Science Framework at the following URLs:

Study 1: osf.io/msphv

Study 2: osf.io/yegrv

Footnotes

¹ Because we anticipated there would be a few non-drivers in the sample, the four questions about driving behaviors included a checkbox where participants could indicate their non-driver status. As we intended, the 7 participants checking this box did not respond with percentile estimates for driving behaviors.

² Their order was randomized within random block. See online Supplementary Materials for the full list.

³ When ties straddled the cut points for percentiles or quartiles, a random tie break was used.

⁴ Cohen's d calculated per the recommendation of Lakens (2013).

⁵ ANOVA including all four quartiles produced the same pattern of significant findings (see online Supplementary Materials).

⁶ Furthermore, the number of times that a participant fell in the healthiest quartile for the 10 healthy behaviors was not significantly related to the number of times they fell in the healthiest quartile for the 10 unhealthy behaviors ($r = .07, p = .435$). The same was true for tallies of falling in the unhealthiest quartile ($r = -.04, p = .667$).

⁷ Windschitl, Rose, Stalkfleet, and Smith (2008) described a concern with using these regression analyses as a test of egocentrism. As they noted, the concern only applies to cases in which the variability of the absolute-other estimates is smaller than the variability of the absolute estimates about the self. With this mind, we tested whether the variabilities of the two types of estimates

tended to be different within in our study. We computed the standard deviations for self-estimates and for other-estimates for each of the 20 behaviors. We used those 40 standard deviations as data points in paired-samples t-test. The standard deviations of self-estimates were not statistically different from those of other-estimates, $t(19) = 1.49$, $p = .150$, 95% CI [-0.54, 3.27].

⁸ Furthermore, in Study 2, the number of times that a participant fell in the healthiest quartile for the 10 healthy behaviors was not significantly related to the number of times they fell in the healthiest quartile for the 10 unhealthy behaviors ($r = -.02$, $p = .803$). The same was true for tallies of falling the unhealthiest quartile ($r = .16$, $p = .089$).

⁹ We had planned to do similar analyses with participants' SAT scores, but only 24 of the 114 participants reported having taken the SAT and their score. We therefore did not run those analyses and only report the analyses with ACT scores.

References

- Ackerman, P. L., Beier, M. E., & Bowen, K. R. (2002). What we really know about our abilities and our knowledge. *Personality and Individual Differences*, 33(4), 587–605.
[https://doi.org/10.1016/S0191-8869\(01\)00174-X](https://doi.org/10.1016/S0191-8869(01)00174-X)
- Alicke, M. D. (1985). Global self-evaluation as determined by the desirability and controllability of trait adjectives. *Journal of Personality and Social Psychology*, 49(6), 1621–1630.
<https://doi.org/10.1037/0022-3514.49.6.1621>
- Alicke, M. D., & Govorun, O. (2005). The Better-Than-Average Effect. In *The Self in Social Judgment* (pp. 85–106). <https://doi.org/10.1111/j.1559-1816.2011.00752.x>

- Bialek, M., & Pennycook, G. (2018). The Cognitive Reflection Test is robust to multiple exposures. *Behavior Research Methods*, 50, 1953-1959. doi: 10.3758/s13428-017-0963-x
- Blacksmith, N., Yang, Y., Behrend, T. S., & Ruark, G. A. (2019). Assessing the validity of inferences from scores on the cognitive reflection test. *Journal of Behavioral Decision Making*. Retrieved online: <https://doi.org/10.1002/bdm.2133>
- Brown, J. D. (2012). Understanding the better than average effect: Motives (still) matter. *Personality and Social Psychology Bulletin*, 38(2), 209–219. <https://doi.org/10.1177/0146167211432763>
- Burson, K. A., Larrick, R. P., & Klayman, J. (2006). Skilled or unskilled, but still unaware of it: How perceptions of difficulty drive miscalibration in relative comparisons. *Journal of Personality and Social Psychology*, 90(1), 60–77. <https://doi.org/10.1037/0022-3514.90.1.60>
- Buunk, B. P., Gibbons, F. X., & Visser, A. (2002). The relevance of social comparison processes for prevention and health care. *Patient Education and Counseling*, 47(1), 1–3. [https://doi.org/10.1016/S0738-3991\(02\)00054-X](https://doi.org/10.1016/S0738-3991(02)00054-X)
- Chambers, J. R., & Windschitl, P. D. (2004). Biases in social comparative judgments: The role of nonmotivated factors in above-average and comparative-optimism effects. *Psychological Bulletin*, 130(5), 813-838. <https://doi.org/10.1037/0033-2909.130.5.813>
- Chambers, J. R., Windschitl, P. D., & Suls, J. (2003). Egocentrism, event frequency, and comparative optimism: When what happens frequently is “more likely to happen to me.” *Personality and Social Psychology Bulletin*, 29(11), 1343–1356. <https://doi.org/10.1177/0146167203256870>

- Critcher, C. R., & Dunning, D. (2009). How chronic self-views influence (and mislead) self-assessments of task performance: self-views shape bottom-up experiences with the task. *Journal of Personality and Social Psychology*, 97(6), 931–945.
<https://doi.org/10.1037/a0017452>
- Croyle, R. T., Sun, Y. C., & Hart, M. (1997). Processing risk factor information: Defensive biases in health-related judgments and memory. In Petrie, K.J., & Weinman, J. (Eds.), *Perceptions of Health and Illness: Current Research and Applications* (pp. 267-290). Milton, England: Taylor and Francis.
- Del Boca, F. K., & Darkes, J. (2003). The validity of self-reports of alcohol consumption: state of the science and challenges for research. *Addiction*, 98, 1-12.
<https://doi.org/10.1046/j.1359-6357.2003.00586.x>
- Diel, K., & Hofmann, W. (2019). Inspired to perspire: The interplay of social comparison direction and standard extremity in the context of challenging exercising goals. *Social Cognition*, 37(3), 247-265. <https://doi.org/10.1521/soco.2019.37.3.247>
- Dunning, D., & Hayes, A. F. (1996). Evidence for egocentric comparison in social judgment. *Journal of Personality and Social Psychology*, 71(2), 213–229.
<https://doi.org/10.1037/0022-3514.71.2.213>
- Dunning, D., Heath, C., & Suls, J. M. (2004). Flawed self-assessment implications for health, education, and the workplace. *Psychological Science in the Public Interest, Supplement*, 5(3), 69–106. <https://doi.org/10.1111/j.1529-1006.2004.00018.x>
- Dunning, D., Johnson, K., Ehrlinger, J., & Kruger, J. (2003). Why people fail to recognize their own incompetence. *Current Directions in Psychological Science*, 12(3), 83–87.

<https://doi.org/10.1111/1467-8721.01235>

Ehrlinger, J., & Dunning, D. (2003). How chronic self-views influence (and potentially mislead) estimates of performance. *Journal of Personality and Social Psychology*, 84(1), 5–17.

<https://doi.org/10.1037//0022-3514.84.1.5>

Ehrlinger, J., Johnson, K., Banner, M., Dunning, D., & Kruger, J. (2008). Why the unskilled are unaware: Further explorations of (absent) self-insight among the incompetent. *Organizational Behavior and Human Decision Processes*, 105(1), 98–121.

<https://doi.org/10.1016/j.obhdp.2007.05.002>

Festinger, L. (1954). A theory of social comparison processes. *Human Relations*, 7(2), 117–140.

<https://doi.org/10.1177/001872675400700202>

Frederick, S. (2005). Cognitive reflection and decision making. *Journal of Economic Perspectives*, 19, 25–42. DOI: 10.1257/089533005775196732

Gerber, J. P., Wheeler, L., & Suls, J. (2018). A social comparison theory meta-analysis 60+ years on. *Psychological Bulletin*, 144(2), 177–197. <https://doi.org/10.1037/bul0000127>

Giladi, E. E., & Klar, Y. (2002). When standards are wide of the mark: Nonselective superiority and inferiority biases in comparative judgments of objects and concepts. *Journal of Experimental Psychology: General*, 131, 538–551. doi:10.1037/0096-3445.131.4.538

Harris, P.R., & Napper, L. 2005. Self-affirmation and the biased processing of threatening health-risk information. *Personality and Social Psychology Bulletin*, 31, 1250–1263. DOI: 10.1177/0146167205274694

Hoorens, V., & Harris, P. R. (1998). Distortions in reports of health behaviors: the time span effect and illusory superiority. *Psychology & Health*, 13(3), 451–466.

<https://doi.org/10.1080/08870449808407303>

Klar, Y., & Giladi, E. E. (1997). No one in my group can be below the group's average: A robust positivity bias in favor of anonymous peers. *Journal of Personality and Social Psychology*, 73, 885- 901. <http://dx.doi.org/10.1037/0022-3514.73.5.885>

Klar, Y., Medding, A., & Sarel, D. (1996). Nonunique invulnerability: Singular versus distributional probabilities and unrealistic optimism in comparative risk judgments. *Organizational Behavior and Human Decision Processes*, 67(2), 229–245.

<https://doi.org/10.1006/obhd.1996.0076>

Klein, W. M. (1997). Objective standards are not enough: affective, self-evaluative, and behavioral responses to social comparison information. *Journal of Personality and Social Psychology*, 72(4), 763–774. <https://doi.org/10.1037/0022-3514.72.4.763>

Klein, W. M., & Kunda, Z. (1993). Maintaining self-serving social comparisons: biased reconstruction of one's past behaviors. *Personality and Social Psychology Bulletin*, 19(6), 732–739. <https://doi.org/10.1177/0146167293196008>

Klein, W. M., & Buckingham, J. T. (2002). Self-other biases in judgments of ambiguous performance and corresponding ability. *Psychologica Belgica*, 42(1–2), 43–64.

Krajc, M., & Ortmann, A. (2008). Are the unskilled really that unaware? An alternative explanation. *Journal of Economic Psychology*, 29(5), 724–738. <https://doi.org/10.1016/j.joep.2007.12.006>

Krueger, J., & Mueller, R. A. (2002). Unskilled, unaware, or both? The better-than-average heuristic and statistical regression predict errors in estimates of own performance. *Journal of Personality and Social Psychology*, 82(2), 180–188. <https://doi.org/10.1037/0022->

3514.82.2.180

Kruger, J. (1999). Lake Wobegon be gone! The “below-average effect” and the egocentric nature of comparative ability judgments. *Journal of Personality and Social Psychology*, 77(2), 221–232. <https://doi.org/10.1037/0022-3514.77.2.221>

Kruger, J., & Burrus, J. (2004). Egocentrism and focalism in unrealistic optimism (and pessimism). *Journal of Experimental Social Psychology*, 40(3), 332–340. <https://doi.org/10.1016/j.jesp.2003.06.002>

Kruger, J., & Dunning, D. (1999). Unskilled and unaware of it: How difficulties in recognizing one’s own incompetence lead to inflated self-assessments. *Journal of Personality and Social Psychology*, 77(6), 1121–1134. <https://doi.org/10.1037/0022-3514.77.6.1121>

Kruger, J., & Dunning, D. (2002). Unskilled and unaware--but why? A reply to Krueger and Mueller (2002). *Journal of Personality & Social Psychology*, 82(2), 189–192. <https://doi.org/10.1037//0022-3514.82.2.189>

Kruger, J., & Savitsky, K. (2009). On the genesis of inflated (and deflated) judgments of responsibility. *Organizational Behavior and Human Decision Processes*, 108(1), 143–152. <https://doi.org/10.1016/j.obhdp.2008.06.002>

Kunda, Z. (1990). The case for motivated reasoning. *Psychological Bulletin*, 108(3), 480–498. <https://doi.org/10.1037/0033-2909.108.3.480>

Lakens, D. (2013). Calculating and reporting effect sizes to facilitate cumulative science: a practical primer for t-tests and ANOVAs. *Frontiers in Psychology*, 4, 1-12. <https://doi.org/10.3389/fpsyg.2013.00863>

Lapinski, M. K., Maloney, E. K., Braz, M., & Shulman, H. C. (2013). Testing the effects of

- social norms and behavioral privacy on hand washing: A field experiment. *Human Communication Research*, 39, 21-46. <https://doi.org/10.1111/j.1468-2958.2012.01441.x>
- Larrick, R. P., Burson, K. A., & Soll, J. B. (2007). Social comparison and confidence: When thinking you're better than average predicts overconfidence (and when it does not). *Organizational Behavior and Human Decision Processes*, 102, 76–94. <https://doi.org/10.1016/j.obhdp.2006.10.002>
- Liberali, J. M., Reyna, V. F., Furlan, S., Stein, L. M., & Pardo, S. T. (2012). Individual differences in numeracy and cognitive reflection, with implications for biases and fallacies in probability judgment. *Journal of Behavioral Decision Making*, 25, 361-381. DOI: 10.1002/bdm.752
- Lockwood, P., & Pinkus, R. T. (2008). The impact of social comparisons on motivation. In J. Y. Shah & W. L. Gardner (Eds.), *Handbook of Motivation Science* (pp. 251-264). New York, NY, US: The Guilford Press.
- Logg, J. M., Haran, U., & Moore, D. A. (2018). Is overconfidence a motivated bias? Experimental evidence. *Journal of Experimental Psychology: General*, 147(10), 1445-1465. <http://dx.doi.org/10.1037/xge0000500>
- Mata, A., Ferreira, M. B., & Sherman, S. J. (2013). The metacognitive advantage of deliberative thinkers: A dual-process perspective on overconfidence. *Journal of Personality and Social Psychology*, 105, 353-373. <http://dx.doi.org/10.1037/a0033640>
- Miles, S., & Scaife, V. (2003). Optimistic bias and food. *Nutrition Research Reviews*, 16, 3–19. <https://doi.org/10.1079/NRR200249>
- Miller, J.E., Windschitl, P.D., Treat, T.A., & Scherer, A.M. (2019). Comparisons as predictors of

people's inclinations to change their health behaviors. *Manuscript under review*.

Moore, D. A., & Healy, P. J. (2008). The trouble with overconfidence. *Psychological Review*, 115(2), 502–517. <https://doi.org/10.1037/0033-295X.115.2.502>

Moore, D. A., & Kim, T. G. (2003). Myopic social prediction and the solo comparison effect. *Journal of Personality and Social Psychology*, 85(6), 1121–1135. <http://dx.doi.org/10.1037/0022-3514.85.6.1121>

Moore, D. A., & Small, D. A. (2007). Error and bias in comparative judgment: on being both better and worse than we think we are. *Journal of Personality and Social Psychology*, 92(6), 972–989. <https://doi.org/10.1037/0022-3514.92.6.972>

Pennycook, G., Ross, R. M., Koehler, D. J., & Fugelsang, J. A. (2017). Dunning–Kruger effects in reasoning: Theoretical implications of the failure to recognize incompetence. *Psychonomic Bulletin & Review*, 24, 1774–1784. doi: 10.3758/s13423-017-1242-7.

Perloff, L. S., & Fetzer, B. K. (1986). Self–other judgments and perceived vulnerability to victimization. *Journal of Personality and Social Psychology*, 50(3), 502–510. <http://dx.doi.org/10.1037/0022-3514.50.3.502>

Price, P. C., Pentecost, H. C., & Voth, R. D. (2002). Perceived event frequency and the optimistic bias: Evidence for a two-process model of personal risk judgments. *Journal of Experimental Social Psychology*, 38(3), 242–252. <https://doi.org/10.1006/jesp.2001.1509>

Renner, B., Schütz, B., & Sniehotta, F. F. (2008). Preventive health behavior and adaptive accuracy of risk perceptions. *Risk Analysis: An International Journal*, 28, 741–748. <https://doi.org/10.1111/j.1539-6924.2008.01047.x>

- Ridout, B., & Campbell, A. (2014). Using Facebook to deliver a social norm intervention to reduce problem drinking at university. *Drug and Alcohol Review*, 33, 667-673.
<https://doi.org/10.1111/dar.12141>
- Rimal, R. N., & Real, K. (2005). How behaviors are influenced by perceived norms: A test of the theory of normative social behavior. *Communication Research*, 32, 389-414.
<https://doi.org/10.1177/0093650205275385>
- Scherer, A. M., Bruchmann, K., Windschitl, P. D., Rose, J. P., Smith, A. R., Koestner, B., ... Suls, J. (2016). Sources of bias in peoples' social-comparative estimates of food consumption. *Journal of Experimental Psychology: Applied*, 22(2), 173-183.
<https://doi.org/10.1037/xap0000081>
- Schlösser, T., Dunning, D., Johnson, K. L., & Kruger, J. (2013). How unaware are the unskilled? Empirical tests of the "signal extraction" counterexplanation for the Dunning-Kruger effect in self-evaluation of performance. *Journal of Economic Psychology*, 39, 85-100.
<https://doi.org/10.1016/j.joep.2013.07.004>
- Shepperd, J. A., Carroll, P., Grace, J., & Terry, M. (2002). Exploring the causes of comparative optimism. *Psychologica Belgica*, 42, 65-98. <https://doi.org/10.1108/09513570610679100>
- Shepperd, J. A., Klein, W. M., Waters, E. A., & Weinstein, N. D. (2013). Taking stock of unrealistic optimism. *Perspectives on Psychological Science*, 8(4), 395-411.
<https://doi.org/10.1177/1745691613485247>
- Šrol, J. (2018). Dissecting the expanded cognitive reflection test: an item response theory analysis. *Journal of Cognitive Psychology*, 30, 643-655.
<https://doi.org/10.1080/20445911.2018.1518327>

- Toplak, M. E., West, R. F., & Stanovich, K. E. (2014). Assessing miserly information processing: An expansion of the Cognitive Reflection Test. *Thinking & Reasoning*, 20, 147-168. <https://doi.org/10.1080/13546783.2013.844729>
- Tyler, J. M., & Rosier, J. G. (2009). Examining self-presentation as a motivational explanation for comparative optimism. *Journal of Personality and Social Psychology*, 97(4), 716–727. <https://doi.org/10.1037/a0016231>
- Van 't Riet, J., & Ruiter, R. A. (2013). Defensive reactions to health-promoting information: An overview and implications for future research. *Health Psychology Review*, 7, S104-S136. <https://doi.org/10.1080/17437199.2011.606782>
- Vazire, S., & Mehl, M. R. (2008). Knowing me, knowing you: the accuracy and unique predictive validity of self-ratings and other-ratings of daily behavior. *Journal of Personality and Social Psychology*, 95, 1202-1216. <http://dx.doi.org/10.1037/a0013314>
- Weinstein, N. D. (1980). Unrealistic optimism about future life events. *Journal of Personality and Social Psychology*, 39(5), 806–820. <https://doi.org/10.1037/0022-3514.39.5.806>
- Weinstein, N. D., & Klein, W. M. (1995). Resistance of personal risk perceptions to debiasing interventions. *Health Psychology*, 14, 132-140. <http://dx.doi.org/10.1037/0278-6133.14.2.132>
- Weinstein, N. D., & Lachendro, E. (1982). Egocentrism as a source of unrealistic optimism. *Personality and Social Psychology Bulletin*, 8(2), 195–200. <https://doi.org/10.1177/0146167282082002>
- Westfall, J., Kenny, D. A., & Judd, C. M. (2014). Statistical power and optimal design in experiments in which samples of participants respond to samples of stimuli. *Journal of*

Experimental Psychology: General, 143(5), 2020-2045.

<http://dx.doi.org/10.1037/xge0000014>

Windschitl, P. D., Kruger, J., & Simms, E. N. (2003). The influence of egocentrism and focalism on people's optimism in competitions: when what affects us equally affects me more.

Journal of Personality and Social Psychology, 85(3), 389–408.

<https://doi.org/10.1037/0022-3514.85.3.389>