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Knowing Where They Stand: The Role of Inferred Distributions of Others in Misestimates of Relative Standing

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People often estimate how they compare to other consumers when they make purchase decisions. Unfortunately, they tend to err in this task, and this can lead to negative consequences in their choices. Previous literature has largely argued that these errors in estimates of relative standing are due to underweighting or ignoring the reference group. Using a novel measure of people's perception of the reference group, we show that consumers do attend to that information but err in their estimates of relative standing because they tend to overestimate the dispersion of others' performances and attributes. Three studies support this argument and provide insights that enable marketers to alter consumers' relative assessment process in formerly discounted ways. We demonstrate straightforward tools that can change consumers' impressions of others and thus change relative assessments and purchase decisions.

Consumers often choose between options that vary on dimensions such as size or complexity. For example, when purchasing a camera, a consumer may choose from alternatives that range from very basic to very sophisticated. One way consumers make these choices is by considering where they are relative to others on a relevant ability, preference, or characteristic. Then they select the product whose position relative to other products matches their position relative to that of other consumers (Burson 2007; Prelec, Wernerfelt, and Zettelmeyer 1997; Simonson 1993; Wernerfelt 1995). For instance, a consumer who estimates that his photography skills place him in the middle of the distribution of consumers will select a camera in the midrange of options.

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Unfortunately, although beliefs about relative standing are used as inputs to choice, people often misestimate how they compare to others. For example, prior research has shown that the majority of people estimate that they are above average at driving, using a computer mouse, telling jokes, investing, and securing jobs (Kruger 1999; Kruger and Burrus 2004; Svenson 1981). Conversely, in other domains people tend to underestimate their relative standing. For example, the majority of people believe that they are below average at riding unicycles, receiving traffic tickets, programming computers, gaining weight, and playing chess (Kruger 1999; Kruger and Burrus 2004). These examples show that people's relative assessments are biased by difficulty. As a result, miscalibrated consumers may not select appropriate products (Burson 2007; Prelec et al. 1997), leaving them disappointed and dissatisfied with their purchases (e.g., episode DESQ-105 of *Sweat Equity*, DIY Network, February 23, 2009). Indeed, one-quarter of products are returned not because of a product defect but simply because consumers have purchased products that they do not understand or cannot properly operate (Stanley 1997). Though much research has attempted to explain errorful relative estimates, there is disagreement about their source (e.g., Moore 2007).

The present research uncovers one source of the systematic biases in consumers' estimates of relative standing. A large body of prior research has concluded that errors in estimates of relative standing result from a failure to adequately rely on information about others (Chambers and

Windschitl 2004; Klar and Giladi 1999; Klar, Medding, and Sarel 1996; Kruger 1999; Kruger and Burrus 2004; Kruger et al. 2008; Windschitl, Kruger, and Simms 2003; Windschitl et al. 2008). As such, estimates of relative standing would not be affected by changes in estimates of how others are distributed. However, we propose an alternative explanation for biased estimates of relative standing: people do use information about others as inputs into estimates of their relative standing, but guided by extreme examples, they tend to imagine distributions that are overly dispersed. We show that people make overly dispersed estimates of others and then use them as inputs into estimates of their own relative standing. We further show that by providing extreme examples of others, people's estimates of relative standing can be influenced, leading to changes in product preference. More important, we show how this explanation of overdispersion can account for commonly observed above- and below-average effects previously attributed to insufficient consideration of others. In the process, we make a methodological contribution, using a more complete and sensitive measure of participants' perception of reference groups than has been previously employed, allowing us to both better understand inferences about reference groups and demonstrate the degree to which they influence estimates of relative standing.

We present three studies that support our overdispersion hypothesis. In study 1, we replicate a well-documented finding that consumers inflate estimates of their own relative standing on an easy task and deflate their estimates of relative standing on a difficult task (e.g., Kruger 1999). We then show that participants overestimate the dispersion of their comparison group in both easy and difficult tasks and that this misconstrual of others can explain their biased estimates of relative standing. Finally, we demonstrate that these biased relative estimates go on to influence consumption decisions. Studies 2 and 3 further explore this insight. In study 2 we show that, even when the task is fixed such that individuals' absolute performance is held constant, influencing estimates of how others are distributed can affect relative standing estimates. Finally, in study 3, we show that even estimated relative standing for personal characteristics, such as one's height, can be influenced by perceptions of how others are distributed, and this is enough to influence product preference.

THEORETICAL DEVELOPMENT

Research has shown that people regularly make comparative judgments of their own abilities, attitudes, behaviors, and characteristics (Festinger 1954; Kruglanski and Maysel 1990). These judgments influence their decision making across a wide range of tasks including product search (Moorman et al. 2004), product choice (Burson 2007; Prelec et al. 1997), and intentions to seek medical testing and treatment (Menon, Block, and Ramanathan 2002).

Prelec et al. (1997) first showed that consumers draw inferences about the appropriateness of a product for themselves from a product's relative position in a choice set or

array. When consumers are presented with multiple products that vary on some important attribute, such as size or complexity, they select a product that matches their own relative standing in the population. For example, in one study, participants chose a rain poncho from a set of three that varied in lengths ranging from 32 to 36, 34 to 38, 36 to 40, or 38 to 42 inches. Regardless of the actual lengths of the ponchos, 70% of choices could be explained by participants selecting the poncho that matched their relative height. The authors used this evidence to explain the compromise effect—the tendency for people to choose the middle product in an array—because many consumers believe that they are average and thus choose middle products. Subsequent research has shown that a consumer's beliefs about her skill relative to that of others can be shifted, affecting product choice. For example, Moorman et al. (2004) showed that participants who were led to believe that their own nutrition knowledge was relatively poor shopped in a manner consistent with a consumer who did not care about nutrition, unlike those who thought that they were more knowledgeable about nutrition than others. Similarly, Burson (2007) manipulated individuals' beliefs about their own golfing skills by giving them either a difficult or easy putting task. Compared to participants given the difficult putting task, those in the easy condition estimated that they were better at golf relative to others as well. As a result, those in the easy condition went on to choose equipment designed for more skilled players, and those in the difficult condition chose equipment designed for less skilled players.

Errorful Estimated Relative Standing

Despite using their estimates of relative standing as an input in product choice, people are quite prone to err in these estimates. As noted above, prior work has found that people report being above average in numerous domains where success seems easy or common but below average in domains that are perceived as difficult or rare (e.g., Chambers and Windschitl 2004; Kruger and Burrus 2004). The combination of above-average estimates on easy tasks and below-average estimates on difficult tasks is called the "difficulty effect" (Burson, Larrick, and Klayman 2006). Of course, such biases in estimation of relative standing could lead consumers to choose products ill suited to their needs.

Researchers have suggested a number of motivational and cognitive explanations for the difficulty effect and other systematic errors in judgments of relative standing (see Chambers and Windschitl [2004] for a review), but the primary explanation has been that they fail to consider, or underweigh, others (Klar and Giladi 1999; Kruger 1999; Kruger and Burrus 2004; Kruger et al. 2008; Windschitl et al. 2003). That is, people consider their own absolute performance on a task but either ignore or insufficiently weigh the performances of others (which would also tend to be high on easy tasks and low on difficult tasks). As a consequence of this "asymmetric weighting" of relevant information (rather than a symmetric, equal judgmental weight on self and on the distribution of others), a person who

objectively performs well on an easy task fails to adequately consider that others probably also performed well. So, high absolute performance is translated into (too) high relative performance. Conversely, a person who objectively performs poorly on a hard task fails to adjust for the fact that others also performed poorly and so estimates (too) low relative standing.

Although this account is consistent with other work on egocentric biases (Ross and Sicoly 1979), some researchers have suggested that individuals may not totally ignore the distribution of others (e.g., Burson and Klayman 2010; Moore and Small 2007). If this is the case, then an alternative explanation for observed errors in estimates of relative standing is that consumers do consider others but that estimates of how others are distributed are systematically flawed and malleable. We argue that consumers are largely unaware of the true distribution of others, and so their estimates of others are subject to contextual cues. The most salient cues are those of distributional extremes, so consumers tend to imagine those extremes as more populated than they actually are. As a result, consumers' estimates of how others are distributed are overly dispersed across the range of possible abilities, preferences, or characteristics. Consumers rely on these flawed, overly dispersed, distribution estimates in making estimates of relative standing.

If consumers tend to make overly dispersed estimates of how others are distributed and then incorporate these distributions (or even an approximation of them) into their estimates of relative standing, it would provide a distinct (though not necessarily mutually exclusive) explanation for why people overestimate their relative abilities on easy tasks and why they tend to underestimate them on difficult tasks. Consider that for an easy task such as driving a car, a consumer who has received only a few traffic violations in his driving career may think that he performs well in an objective sense. If he does not recognize that other drivers also tend to perform well and he misinfers that people are fairly diverse in their driving abilities, he will perceive that many others have worse ability than he does when he compares himself to this false distribution. This impression will cause him to overestimate his ability relative to others.

Errorful Estimated Distributions

Evidence of errors in inferences about the distributions of others is plentiful. A number of studies have shown that although individuals may be sensitive to the true distribution of others, they tend to overestimate the degree of variability or dispersion (Judd, Ryan, and Park 1991; Nisbett et al. 1983; Nisbett and Kunda 1985; Park and Hastie 1987).

One likely explanation for this overdispersion in estimates is that people are more likely to remember extreme performances than middling ones (Nisbett and Kunda 1985). This availability effect may be exacerbated by the fact that people assume that there is a reason for the range and the increments of any response scale (Grice 1975; Schwarz 1999). That is, people assume that categories of classification (such as novice, intermediate, advanced, and expert or small, medium,

and large) serve a purpose in separating people into groups, and these groups are diagnostic of differences between people. After all, if every respondent fell into just one category and no one fell into any of the others, the classification scheme would not be useful. However, the assumption is that the categories are meaningful: the endpoints of the set are likely to include people in the extremes of the distribution, and points in the middle are likely to include those who are more typical. Therefore, the distribution of consumers is assumed to span the scale.

We believe that prior work on estimates of relative standing has not found support for use of information about others in part because the measures that have been used to capture perceptions of others do not allow for this level of detail. Typically, these studies have explored how estimates of relative standing are predicted by individuals' beliefs about their estimated absolute ability (or actual absolute ability) and by individuals' estimates of some singular measure of central tendency such as the group mean, the median person, an average person, or a randomly selected person from the reference group (Chambers and Windschitl 2004; Moore 2007). A more complete measure of the judgmental weight of others would be one that took into account individuals' estimates of the entire distribution. We do not intend to claim that consumers spontaneously calculate the variance, skewness, and kurtosis of their reference group when they make estimates of their relative standing. However, we hypothesize (and our data reveal) that consumers have at least an approximate distribution of others in mind. Indeed, a representation of the distribution, such as the approximate range, may in fact be easier to bring to mind than making a calculation of its mean (Nisbett and Kunda 1985). Further, even if a consumer were to accurately estimate and utilize a single representation of others, such as the mode, it would not necessarily be sufficient information to ensure that he accurately estimated his relative standing because there are many ways in which he could estimate how others are dispersed. If he estimates that the distribution is fairly skewed, with many unskilled others and very few experts, his own poor ability could still put him in the middle relative to others. However, if he estimates that there is a fairly wide dispersion across the range of possible skills, then he would also estimate that there are many more who are more skilled than he is. Of course single measures of central tendency used by other researchers reflect some aspects of the distribution of others. For example, Moore and Small (2007) show that absolute estimates of another participant's performance are regressive: where own estimates are high, those for another person are lower, and where own estimates are low, those for another person are higher. This finding suggests that perhaps the estimated distribution of other performances is not moving enough in response to difficulty, maybe because it is too dispersed. We develop this insight, arguing that it is the perceived dispersion on either side of the judge that is most important in making a relative estimate; therefore, it is critical that this dispersion be measured.

In sum, we suggest that biases in estimates of relative

standing may occur not because people fail to consider information about others but because they make flawed estimates of these distributions and use them to form estimates of their relative standing. If people do indeed incorporate estimates of the distributions of others into their estimates of relative standing, then downstream behavior such as product choice will be influenced by manipulations to consumers' distribution estimates and antecedents of distribution estimates can be manipulated to influence estimates of relative standing. Building on Nisbett and Kunda's (1985) observation, we manipulate availability of individuals at extremes of the distribution and examine the effects on consumers' estimates of their relative standing as a result of influencing their distributional assumptions. Prior work has shown how ease of generating extreme examples influences estimates of how others are distributed (Gershoff, Mukherjee, and Mukhopadhyay 2008; Rothbart et al. 1978). Thus, if we make examples of others who fall at one end of a distribution relatively available, then the perceived distribution will be shifted toward that end. In turn, estimates of relative standing will be affected by this change in the perceived distribution.

In the studies that follow, we first explore whether overdispersion of others across the range of possible abilities and characteristics influences bias in relative estimates. To this end, we employ a novel measure of participants' distributional assumptions. Then, we simultaneously illustrate that extreme examples dictate the shape of the estimated distribution and provide a remedy for the bias by manipulating the availability of particular individuals in the distribution.

STUDY 1: ESTIMATES OF OTHERS IN DIFFICULTY EFFECTS

Study 1 has four purposes. The first is to replicate "difficulty effects"—the finding that estimated relative standing is higher on easy than on difficult tasks. The second is to show that people estimate overly dispersed distributions of others' abilities when they make these estimates. The third is to illustrate that estimates of how others are distributed do indeed play a role in forming estimates of relative standing. This will offer support for our novel explanation for why people show difficulty effects. Finally, study 1 aims to replicate consumer product matching—the finding that people tend to choose a product whose relative position in an array is similar to their own position relative to that of other consumers.

To elicit participants' relative self-assessments, we used quizzes within the popular consumer domain of do-it-yourself (DIY) projects. Consistent with prior research, we expected that when asked easy DIY questions, participants would provide higher estimates of their relative standing than when asked difficult questions (e.g., Burson 2007; Burson et al. 2006; Kruger 1999). We also expected that participants' estimated distributions of others would be error-prone. Specifically, their estimates would be more dispersed

than the actual distribution. In contrast to prior research, we expected that participants' (flawed) estimates of how others are distributed would predict their estimates of their own relative standing. We also included measures of product choices related to DIY abilities to confirm the role of relative estimates in purchase decisions shown by Burson (2007) and Prelec et al. (1997).

Method

This experiment was a between-subjects design manipulating the difficulty of a DIY quiz (easy quiz vs. difficult quiz). One hundred and fifty-eight people from a nationwide online service participated for a prorated equivalent of \$6.00 per hour. Participants were 53% female. Ages ranged from 18 to 63 with an average age of 35.47. On the first page of the survey, participants answered eight multiple choice questions about hardware and tools. Participants were randomly assigned to answer questions that were either easy or difficult. For example, in the easy condition participants were asked "What is the most common tool used for tightening and loosening bolts?" and "What are the prongs or claw on the end of a hammer primarily used for?" In the difficult condition questions included "What type of hammer is most appropriate for driving chisels or tapping wood joints together?" and "What is a saw cut along the grain of the wood commonly called?"

After finishing the quiz, all participants estimated the number of questions they answered correctly (zero to eight). The relative standing measure asked participants to estimate, out of 100 other participants who would also take the quiz, the number of participants who would perform worse than they had. (Participants in all studies also estimated the number of people who did exactly the same as they did and the number of people who did better than they did. The results do not change depending on measure type. Therefore, we use the measure that most closely matches that used in previous work.) Estimates of the distribution of other participants' performances were collected by having participants allocate 100 points to "bins" to indicate the number of others who would achieve each possible score on the quiz, from zero to eight (e.g., Nisbett and Kunda 1985). Finally, participants reported their preference for the type of tool set, DIY project, and home repair course they might purchase by using a 7-point scale for each, with anchors labeled "very basic" to "very advanced."

Prior research in this area has not found any effects from the order of eliciting estimates of own performance, another's performance, and one's own relative standing (Klar and Giladi 1999; Klar et al. 1996; Kruger and Burrus 2004; Moore and Small 2007). Likewise, Nisbett and Kunda (1985) found no effect on the estimates of distributions collected from participants who either did or did not first report their own attitudes. Even so, to rule out the possibility that estimates of relative standing in this study were affected by our request that participants also provide us with estimates of how others are distributed, the order of eliciting these measures was counterbalanced. Half of the participants

made their estimates of relative standing before they were asked to estimate how others' scores were distributed. Consistent with the prior work, the order in this study did not affect relative estimates in either condition (t 's < .12, p 's > .1) and is not included in our subsequent analyses.

Results

Effect of Task Difficulty. As expected, participants in the difficult condition answered fewer questions correctly ($M = 2.80$) than those in the easy condition ($M = 6.48$; $t(156) = 13.73$, $p < .001$). Participants' perceptions of the quizzes were consistent with this: the easy quiz led to higher score estimates ($M = 5.78$) than the difficult quiz ($M = 3.25$; $t(156) = 9.69$, $p < .001$).

Participants' estimates of relative standing between the two conditions replicated the difficulty effect. In the easy condition, participants estimated that more people would perform worse than they had ($M = 33.42\%$) than in the difficult condition ($M = 22.58\%$; $t(156) = 3.32$, $p = .001$).

Effect of Task Difficulty on Estimated Distributions. Both estimated and actual distributions were influenced by the quiz difficulty (see fig. 1). Using Nisbett and Kunda's (1985) method for classifying distributions, we observe that for the easy condition, the actual distribution of scores was J Right (JR), and for the difficult condition the actual distribution was Unimodal Symmetric (US).

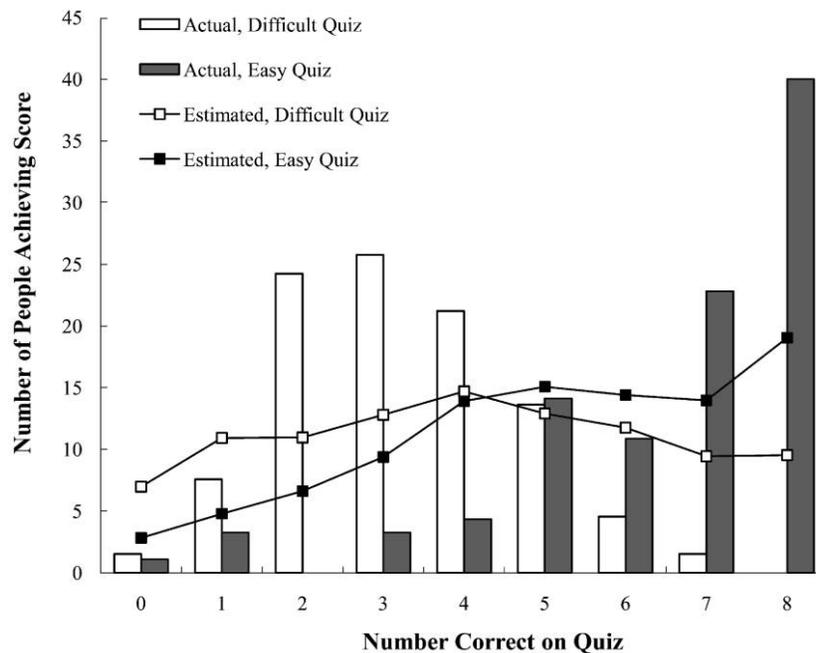
Next we tested our hypothesis that estimated distributions

were overly dispersed. For each participant, we calculated his or her miscalibration at the low (lowest two bins), high (highest two bins), and middle (middle bins) range of the distribution (using the cutoffs applied by Nisbett and Kunda [1985]). This was done by taking the difference between their estimated and the actual count of others who fell into each of the bins. If our overdispersion prediction is correct, participants taking the easy quiz, which has a JR distribution, should overestimate the number of people who scored the lowest and in the middle and underestimate the number of people who scored the highest on the quiz. People taking the difficult quiz, which has a US distribution, will overestimate the number of people in the lowest and highest bins and underestimate those in the middle bin.

A repeated-measures ANOVA was run on the miscalibration scores with quiz difficulty as a between-subjects factor and the three score groups (low, middle, high) as a within-subjects factor (see Steenkamp et al. [2001] for a discussion of this method for analyzing constant sum scores). A quiz difficulty condition by score group interaction confirms a difference in the miscalibration pattern by quiz condition ($F(2, 312) = 170.12$, $p < .001$). Figure 1 reveals that the pattern of miscalibration across the score groups differed by quiz difficulty. On the easy quiz, participants overestimated the number of others who scored in the low and middle groups and underestimated those falling in the high score group (all bin-set contrast p 's < .001). On the difficult quiz, they overestimated the number of others

FIGURE 1

STUDY 1: ESTIMATED RELATIVE TO ACTUAL DISTRIBUTIONS FOR EASY AND DIFFICULT QUIZZES



scoring in the lowest and highest groups but underestimated the number who scored in the middle (all bin-set contrast p 's < .019). In this and all our experiments, the main effects of condition and bin-set on miscalibration were also significant. However, as the main effect of condition by itself is uninformative and the main effect of bin-set is better understood by looking within condition, we do not discuss these effects.

Estimates of Others in Estimates of Relative Standing.

To explore whether participants use these overly dispersed estimated distributions in forming their estimates of relative standing, we examine three measures: (1) the participant's estimate of relative standing, (2) the participant's estimate of own score on the quiz, and (3) the estimate of relative standing the participant would be expected to provide if she indeed considered both her own score and the scores of others when assessing her relative standing. To create this last measure, we looked at the participants' estimate of their own score on each quiz compared to their estimated distribution of others. This provided the number of people who would have done worse than each participant on the quiz based on their own estimates of others' performances. We call this measure "derived relative standing." The derived relative standing measure allows us to look at the role of distributional assumptions in relative estimates without reducing this information to a mean, median, or random other person.

A positive relationship between derived relative standing and estimated relative standing suggests either that one of the two components of derived relative standing (estimated own score or estimated distribution) helps explain percentile estimates or that the combination of those two ingredients helps explain relative standing estimates. There is no direct way to test the unique role of distribution estimates in estimates of relative standing. However, we can easily regress relative estimates onto estimated own score to see if it is well predicted by that measure alone. Then, to see if people consider their own score in the light of what they think about the shape of the distribution, we can include derived relative standing as a predictor to see if accounting for distributional beliefs increases the predictive power of the model. This essentially backs out the role of distributional assumptions: any significant improvement in the fit of a model that included derived relative standing above and beyond estimated own score can be attributed only to consideration of the distribution. Critically, if derived relative standing predicts estimated relative standing in spite of the presence of a unique own score measure and increases the adjusted R -squared of the model through its inclusion, then the unique ingredient—distributional estimates—must be a component of relative standing estimates after all (see Pindyck and Rubinfeld 1998, 132). However, if participants are not relying on their estimates of how others are distributed, we would not expect the derived relative standing to explain any additional variance in participants' estimates of relative standing.

For each quiz we used a two-step regression that allowed

us to test the added benefit of including distributional estimates in the model. First, we regressed participants' estimates of relative standing on their estimates of their own scores. In the next step, we included the derived relative standing in the regressions. Table 1 presents the results for both the easy and difficult conditions. In both conditions, the derived relative standing explained participants' estimated relative standing better than their estimated score alone and reduced the role of their estimated score to insignificance. Note that, while this analysis cannot rule out an asymmetric weighting process in which self and other explain relative estimates but self explains more variance in those estimates than other, it does rule out the absence of weighting of others in estimates of relative standing. The significant change in R -squared is the measure of the significant role of others in those estimates.

Distributions and Accuracy. Overdispersion can also explain the simple miscalibration in each participant's relative estimate as well as the difficulty effect. We calculated each participant's actual relative standing. The sign of miscalibration depends on the actual skill level of the participant and the difficulty of the task, making unsigned measures of miscalibration appropriate (Burson et al. 2006). Regressing the squared difference between estimated and actual number of others performing worse than the participant on the sum of squared errors in distribution estimates explained relative miscalibration ($\beta = .206$, $t(154) = 2.61$, $p = .010$), even controlling for miscalibration in own score and quiz difficulty. This means, again, that estimated distributions significantly contribute to relative estimates and that the more errorful the estimated distribution, the more errorful that relative estimate will be. Furthermore, the role of errors in estimates of score was not significant ($\beta = .081$, $p > .30$), suggesting that it is primarily errors in imagined distributions, not own performance, that led to relative miscalibration.

Relative Standing and Purchase. Finally, as in prior research (e.g., Burson 2007; Prelec et al. 1997), we examine whether estimates of relative standing play a role in the products consumers choose. Participants' product prefer-

TABLE 1
STUDY 1: REGRESSION PARAMETERS PREDICTING PARTICIPANTS' ESTIMATES OF RELATIVE STANDING

	Estimate of own score	Derived relative standing	R^2
Easy quiz:			
Step 1	.348**12
Step 2	.091	.687**	.53
Difficult quiz:			
Step 1	.355**13
Step 2	.106	.506**	.32

NOTE.—Parameters are standardized betas.

**Significance at $p < .01$; R -squared change from step 1 to step 2 is significant at $p < .001$ in all cases.

ences differed by quiz difficulty. Those in the easy condition, where estimates of relative standing were higher, preferred more advanced tool sets ($M_{\text{easy}} = 3.30$, $M_{\text{difficult}} = 2.30$; $t(154) = 3.70$, $p < .001$), more advanced DIY projects ($M_{\text{easy}} = 3.08$, $M_{\text{difficult}} = 2.38$; $t(154) = 2.59$, $p = .011$), and more advanced home repair classes ($M_{\text{easy}} = 2.93$, $M_{\text{difficult}} = 2.21$; $t(153) = 2.73$, $p = .007$) than those in the difficult condition, where estimates of relative standing were lower. These three measures were combined into a single measure ($\alpha = .91$) and regressed on the difficulty condition, estimated quiz score, and estimated relative standing. Critically, estimates of relative standing were directly related to product preferences ($\beta = .246$, $t(154) = 3.14$, $p = .002$), even controlling for quiz difficulty and estimated quiz score, replicating the results of Burson (2007).

Discussion

The results provide evidence of the role of estimated distributions of others in estimates of relative standing. The data replicated the difficulty effect found by prior researchers with participants estimating that their relative standing was higher in the easy than in the difficult quiz condition. Unlike prior work, however, study 1 measured participants' estimates of how others are distributed and found that these estimates were overly dispersed. These biased distribution estimates are fully captured by the derived relative standing measure and, consistent with our argument, were vital in forming estimates of relative standing. This means that participants are using (not neglecting) their errorful impressions of the distribution of others to estimate their relative standing. However, it remains that in the easy and the difficult task conditions there are differences in participants' own estimate and actual scores. Thus, as in previous work, both score and distribution shift. Studies 2 and 3 examine whether the role of estimates of distributions of others in estimates of relative standing would remain when we manipulated beliefs about how others are distributed while holding constant participants' estimates of their own abilities.

STUDY 2: ISOLATING THE ROLE OF DISTRIBUTION ESTIMATES IN THE DIFFICULTY EFFECT

Study 2 further explores how consumers use estimated distributions to form their estimates of relative standing. In this study, we influence estimates of the distribution of others without also influencing participants' estimated absolute performance. To do this, we hold the difficulty of the task constant. Instead, building on Nisbett and Kunda's (1985) proposition that extreme cases are most likely to mold an imagined distribution, we manipulate whether participants are exposed to reports of others who found the task to be extremely easy or extremely difficult. By increasing availability of instances of others at the high or low end of the distribution between conditions, we expect to influence participants' inferences about others, but by holding the dif-

ficulty of the task constant between conditions, we expect that estimates of participants' own scores will be unaffected. We expect that despite holding constant estimates of objective ability, influencing estimates of the distribution of others will influence estimates of relative ability. Furthermore, estimated distributions will continue to show overdispersion.

Method

This experiment was a between-subject design manipulating the feedback about the difficulty of a photography quiz for other participants (other-reported easy vs. other-reported difficult). One hundred and sixty-four people from a nationwide online service participated for a prorated equivalent of \$6.00 per hour. Participants were 54% female and between 18 and 80 years old with an average age of 33.43. All participants answered eight easy multiple choice questions about photography. For example, "What is a photograph of a person usually called?" and "What is the circular glass part on the front of a camera called?" Next participants were shown a page that ostensibly provided other individuals' evaluations of the quiz. The text read "Many people have taken this quiz. Here are some comments by others who took the same quiz." Depending on condition, the quotes indicated that these people found the quiz to be either easy (other-reported easy condition) or difficult (other-reported difficult condition). In the other-reported easy condition, five quotes such as "That was the easiest photography quiz. I'm sure I got them all right" and "Easy to get all of them" were shown. In the other-reported difficult condition, quotes included statements such as "That was the hardest photography quiz. I'm sure I got them all wrong" and "Not easy to get any of them."

Next participants estimated their own score (zero to eight questions correct), relative standing ("How many out of 100 people also taking the quiz performed worse than you did?"), and the distribution of scores ("How many of 100 people taking the quiz scored [zero, one, two, three, etc. correct]?"). As in study 1, the order of these last two questions was counterbalanced.

Results

Other-Reported Quiz Difficulty. As expected, participants' actual performance on the quiz did not differ between conditions ($M_{\text{easy}} = 7.01$ vs. $M_{\text{difficult}} = 7.27$; $t(162) = 1.16$, $p = .249$). Likewise, participants' estimates of their own performance did not differ ($M_{\text{easy}} = 6.35$ vs. $M_{\text{difficult}} = 6.59$; $t(162) = .95$, $p = .342$).

Effect of Reported Quiz Difficulty on Estimated Distributions. Actual scores on the quiz and estimated distributions of scores by other-reported condition are presented in figure 2. Consistent with the quiz being easy, most participants scored well, and actual scores are categorized as a JR distribution.

In order to test our hypothesis that estimated distributions were overly dispersed, we again calculated a miscalibration

score as the difference between the estimated and the actual count of individuals who fell into each the lowest two bins, the highest two bins, and the middle bins.

We used a repeated-measures ANOVA on the miscalibration scores with other-reported quiz difficulty as a between-subjects factor and the score groups (low, middle, high) as a within-subjects factor. An other-reported quiz difficulty by score group interaction confirms a difference in the miscalibration pattern by reported difficulty condition ($F(2, 324) = 13.12, p < .001$). As revealed by figure 2, the pattern of miscalibration across the score groups differed by reported quiz difficulty because the estimated (but not actual) performances of others shifted. Compared to when the quiz was reported to be easy, when others said it was difficult, there was greater overestimation of the number of others who fell in low and middle score groups and greater underestimation of those who fell in the highest score group (for each score group, contrasts between reported easy and reported difficult conditions' miscalibration were significant, p 's $< .05$).

Effect of Other-Reported Quiz Difficulty on Estimates of Relative Standing. As predicted, providing examples of others who believed that they were at the extremes of the distribution affected participants' estimates of relative standing. Specifically, in the other-reported difficult condition, participants estimated that more people would perform worse than they had ($M = 32.2\%$) than in the other-reported easy condition ($M = 44.0\%$; $t(162) = 2.95, p = .004$).

TABLE 2

STUDY 2: REGRESSION PARAMETERS PREDICTING PARTICIPANTS' ESTIMATES OF RELATIVE STANDING

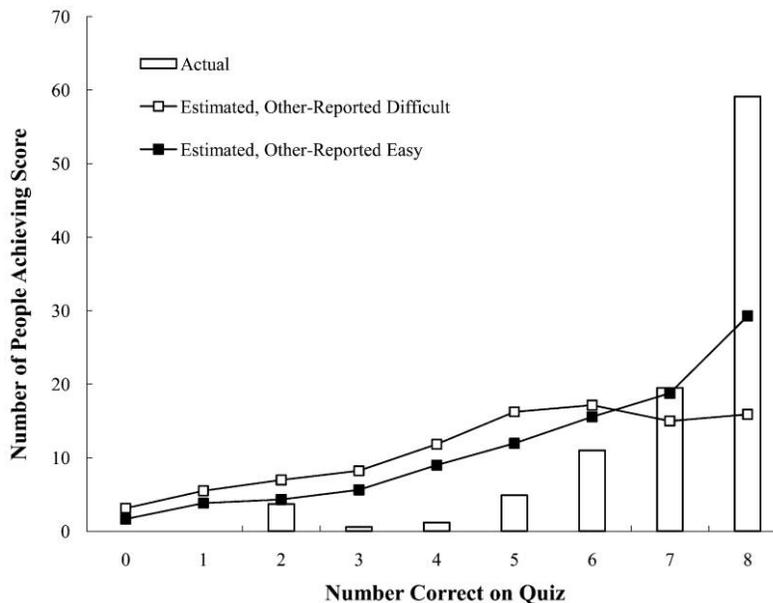
	Estimate of own score	Derived relative standing	R^2
Other-reported easy:			
Step 1	.16001
Step 2	.002	.706***	.50
Other-reported difficult:			
Step 1	.367***14
Step 2	.120	.562***	.38

NOTE.—Parameters are standardized betas.
 ***Significance at $p < .001$; F -squared change from step 1 to step 2 is significant at $p < .001$ in all cases.

Asymmetric Weighting in Estimates of Relative Standing. Though the results so far strongly suggest that participants use impressions of others when they construct relative estimates, we once again sought to show that participants rely on estimates of distributions of others in their estimates of relative standing. Again, we regressed estimated standing on estimates of own score and derived relative standing, calculated by taking each participant's estimate of his or her own score and seeing where it fell in the estimate of how others were distributed. Table 2 shows a pattern identical to that of study 1: participants use their estimates of how others are distributed along with their estimated score to form estimates of relative standing.

FIGURE 2

STUDY 2: ESTIMATES OF OTHERS' QUIZ SCORES BY OTHER-REPORT CONDITION VERSUS ACTUAL DISTRIBUTION



Distributions and Accuracy. Once again, overdispersion also explained miscalibration in relative estimates. Regressing the squared difference between estimated and actual numbers of others performing worse than the participant on the sum of squared errors in distribution estimates explained relative standing miscalibration ($\beta = .201$, $t(160) = 2.55$, $p = .012$), even controlling for miscalibration in own score and the condition. As in study 1, the role of errors in own score was insignificant ($\beta = -.081$, $t(160) = 2.55$, $p > .30$).

Discussion

Study 2 isolates the distributional effects of study 1 by showing that estimates of relative standing can be shifted by influencing only perceptions of the reference group's performance. This finding illustrates the fundamental role of estimates of the distribution of others in difficulty effects: we changed perceived relative standing by moving the estimated distribution while holding the participant's score constant. The next study extends this effect beyond domains of skill.

STUDY 3: BEYOND THE DIFFICULTY EFFECT: THE INFLUENCE OF OTHERS IN ESTIMATES OF RELATIVE STANDING

The purpose of study 3 was to increase the generality of our findings by extending our results beyond the difficulty effect, skill estimates, and skill-based product choices. As previously discussed, Prelec et al. (1997) showed that relatively tall participants selected a relatively long rain poncho from an array, while those who were relatively short selected a relatively short one, regardless of the lengths of the available ponchos. Unlike estimates of skill or probabilities for experiencing future events, which may be unknown or uncertain, one's physical characteristics such as height or weight are known or at least likely to be quite accurately estimated. As a result, it is unlikely that any manipulation would change an estimate of relative standing by influencing the estimate of one's own physical characteristics. Thus, from a theoretical perspective, showing that relative standing estimates for something as unwavering as one's height may be influenced by providing cues about the distributions of others provides strong support for our account of the difficulty effect.

Method

This experiment was a between-subjects design manipulating the availability of extreme others (short others vs. tall others). Two hundred and seventy-five people were recruited from an online panel representative of U.S. citizens over age 18 and were paid a prorated equivalent of \$6.00 per hour. Participants first provided their own height in feet and inches. Then they were exposed to a distribution manipulation that made extreme examples of height (very short or very tall) more available. Participants in the short con-

dition were told, "We asked people from prior studies to send in pictures of themselves. Here are some pictures of short people." Below this were four photographs of individuals standing next to doors that were much taller than each of them. Each person (two men and two women) had his or her name displayed and his or her height, "5.0, 5.1, 5.2, and 5.2." In the tall-people condition, the photos showed four people (two men and two women) standing next to doors that were close in height or slightly shorter than they were. In these, the heights of the individuals pictured were "6.2, 6.2, 6.3, and 6.4." Participants next estimated the distribution of heights of 100 other people participating in the study by indicating the number that would fall into each of seven categories: 5'2" or shorter, 5'3"-5'5", 5'6"-5'8", 5'9"-5'11", 6'0"-6'2", 6'3"-6'5", and 6'6" or taller. Participants also provided an estimate of relative standing—the number of people who were shorter than they are. As before, these last two questions were counterbalanced but showed no effect of order on relative standing estimates ($t(273) = .65$, $p = .514$). As a result, both orders were analyzed together, and no further discussion will be provided about order of eliciting relative standing and distributions. Finally, participants were asked to select from one of seven rain ponchos that were described as varying in length from 30 to 54 inches in 4-inch increments.

Results

Effect of Distribution Extremes on Participants' Height. There was no significant difference in the self-reported height of participants who had been randomly assigned to the conditions ($M_{\text{short}} = 67.3$ inches vs. $M_{\text{tall}} = 67.4$ inches; $t(273) = .029$, $p > .1$).

Effect of Distribution Extremes on Estimated Distributions. The distribution manipulations affected participants' estimates of the distribution of heights of others in the population (see fig. 3). The actual distribution, represented by bars, is classified as US (Nisbett and Kunda 1985).

In order to test our hypothesis that estimated distributions were overly dispersed, we again calculated miscalibration for each participant by taking the difference between his or her estimated number and the actual number of people in the two shortest height bins, the two tallest height bins, and the three middle bins. The data were analyzed in a repeated-measures ANOVA on the miscalibration scores with the height images manipulation as a between-subjects factor and the three height groups (short, medium, tall) as a within-subjects factor. A distribution manipulation by height group interaction confirms a difference in the pattern of miscalibration by height condition ($F(2, 546) = 12.32$, $p < .001$). Figure 3 shows that there was greater overestimation of the number of others who fell in the short height group for those who had been exposed to images of short people compared to those who saw images of tall people. Conversely, there was greater overestimation of the number who fell into the tall height group by those who had been exposed to images of tall people (for the tall and short height groups only,

contrasts between short and tall distribution manipulations were significant [p 's < .001]; the middle height group did not differ by distribution manipulation [$p = .06$].

Effect of Distribution Extremes on Estimates of Relative Standing. Participants who viewed photographs of short people estimated that more people in the study would be shorter than they ($M = 51\%$) compared to those who saw photos of tall people ($M = 45\%$; $t(273) = 2.04, p = .042$).

Asymmetric Weighting in Estimates of Relative Standing. Once again, we tested the hypothesis of asymmetric weighting. As in studies 1 and 2, in both conditions, estimates of relative height were significantly predicted by the derived relative standing measure, even when controlling for own height (see table 3). Therefore, estimates of the distribution of others must have played a significant role in estimates of relative standing.

Distributions and Accuracy. Overdispersion also explained miscalibration in relative estimates. We make the reasonable assumption that reported height is accurate. Regressing the squared difference between estimated and actual numbers of others who were shorter than the participant on the sum of squared error in distribution estimates explained relative miscalibration ($\beta = .181, t(272) = 2.53, p = .012$), even controlling for the distribution manipulation.

TABLE 3
STUDY 3: REGRESSION PARAMETERS PREDICTING PARTICIPANTS' ESTIMATES OF RELATIVE STANDING

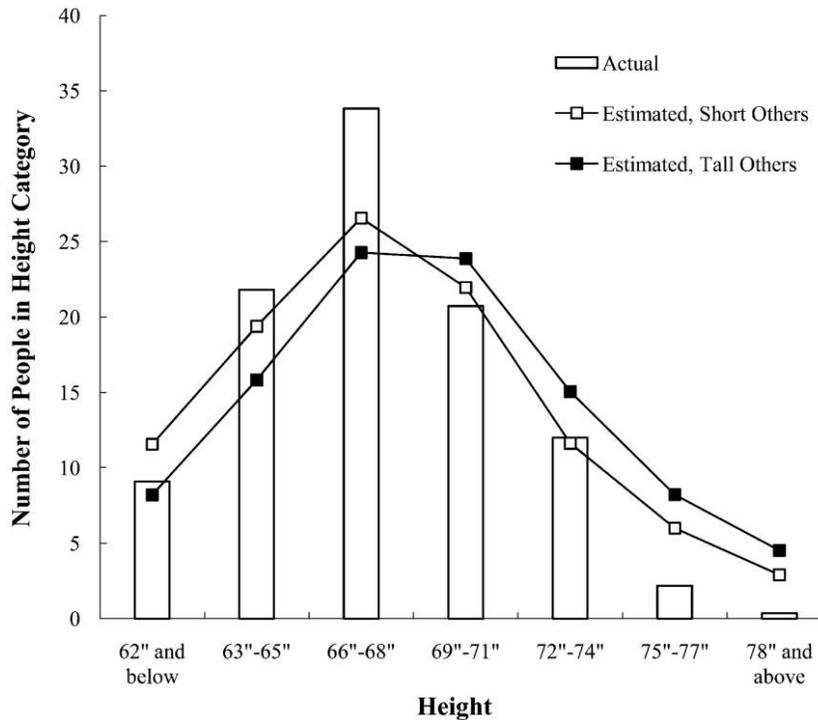
	Estimate of own height	Derived relative standing	R ²
Short others:			
Step 1	.816**66
Step 2	.467**	.399**	.70
Tall others:			
Step 1	.566**32
Step 2	.249	.377**	.35

NOTE.—Parameters are standardized betas.
**Significance at $p < .01$; R -squared change from step 1 to step 2 is significant at $p < .005$ in all cases.

Relative Standing and Product Preference. Finally, as in study 1, we examine whether estimates of relative standing play a role in product selection. Participants' product preferences differed by condition. In the short-others condition, where participants estimated that they were relatively taller, they selected ponchos that were longer ($M = 41.01$ inches long) than in the tall-others condition, where participants estimated that they were relatively shorter ($M = 39.02$ inches long; $t(273) = 2.54, p = .012$).

FIGURE 3

STUDY 3: ESTIMATES OF DISTRIBUTION OF OTHERS' HEIGHTS BY AVAILABILITY CONDITION VERSUS ACTUAL DISTRIBUTION



Critically, estimates of relative standing were directly related to product preferences. A regression with selected poncho as the dependent variable revealed significant effects for participants' estimates, mediating the effect of condition on poncho preference. The manipulation significantly predicted poncho choice ($B = -.256$, $SE = .101$; $t(273) = 2.54$, $p = .012$). Estimates of relative standing mediated the effect of the distribution manipulation on poncho preference (the effect of condition becomes $B = -.164$, $SE = .091$; $t(272) = 1.80$, $p = .072$ when participants' estimated relative standing measure was included in the regression [Sobel $Z = -1.98$, $p = .048$]).

Discussion

Manipulating availability of tall or short other people influenced distribution estimates and subsequent estimates of relative standing, even in this highly objective domain. As in the previous studies, we observed that estimates of how others are distributed played an important role in participants' estimates of their own standing relative to others beyond estimates of their own absolute status (in this case their own height).

GENERAL DISCUSSION

Failures to accurately assess relative standing have been well documented in the literature (Burson 2007; Chambers and Windschitl 2004; Kruger 1999). Prior work has attributed this to a failure to incorporate or consider information about others when making such estimates (see Chambers and Windschitl [2004] for a review), though some recent research challenges this claim (Burson and Klayman 2010; Moore and Small 2007). The studies reported here measure more than just a group average and indicate that people do consider others in estimates of their own relative standing and, more importantly, show that people err in their estimates of others in a systematic way. Both the difficulty effect bias and actual miscalibration in relative estimates are attributable at least in part to the error in estimates of how others are distributed.

Study 1 replicated the finding that people tend to overestimate their relative standing on an easy task and underestimate it on a difficult task, that one's own absolute ability could predict these estimates, and that relative assessments predict product preferences. Study 1 also found that in both easy and difficult tasks, participants estimated that others were more dispersed in their abilities than they actually are, and their relative standing estimates were better predicted by taking these flawed distribution estimates into account. The inaccuracy between estimated and actual relative standing was also explained by errors in distributional assumptions. Indeed, in study 3, it can be explained by flawed distributional assumptions (and perhaps also poor math skills) since actual height does not change. Thus, people are not guilty of ignoring their reference group. Rather, consideration but misestimation of the reference group led to errorful estimates.

Studies 2 and 3 showed the malleability of estimated distributions. In both studies, factors that influenced distribution estimates were manipulated (rather than just measured). Estimates of relative standing were affected by these manipulations. Just bringing to mind people at the extremes of a distribution influenced distribution estimates and thereby affected estimates of relative standing, and in every case, participants significantly considered their reference group when estimating their relative standing.

Theoretical Implications

We have suggested an additional explanation to those provided in prior research on errorful judgments of relative standing (e.g., Chambers and Windschitl 2004). Our results are consistent with findings on assimilation and contrast effects (Sherif and Hovland 1961) that show that attitude and trait judgments about self and referent others tend to diverge on objective scales such as ours (Biernat, Manis, and Kobrynowicz 1997) as well as research showing overdispersion of others' attitudes and behaviors (Nisbett and Kunda 1985): we find that people generate flawed estimates of their reference group. However, we extend these past observations by showing that people incorporate those flawed estimates when making relative standing judgments. Using the derived relative standing measure, we believe that we better capture participants' perceptions of others, thereby more accurately defining the distribution. Though we cannot say with certainty that participants consider and incorporate distributional assumptions into their estimates of relative standing in situations where they are not explicitly prompted for their impression of that distribution, we believe that the results observed in our studies are representative of the process consumers spontaneously employ. We asked some participants to provide a relative estimate before an estimate of the distribution and other participants to do the opposite. Nevertheless, their relative estimates were the same, strongly suggesting that measuring the imagined distribution did not alter the relative estimation process.

Of course, errors in estimates of relative standing are likely to have multiple sources (see Chambers and Windschitl 2004). Errors in estimates of the reference group as well as errors when one must estimate one's own absolute performance and even calculation errors may all play a role. However, overly dispersed distributional estimates are at the very least sufficient to produce difficulty effects, as can be seen in study 2, where a difficulty effect persists in the absence of changes in participants' own estimated scores. Turning our attention to simple miscalibration, it was possible that both errors in estimates of absolute performance and errors in distributional estimates contributed to errorful estimates in relative standing in studies 1 and 2. However, our analyses reveal that only errors in distributional estimates explain relative miscalibration. Furthermore, in study 3, we find that systematically biased relative estimates are strongly predicted by errors in distributional estimates using an objective measure about self, where it is unlikely that one would err.

Practical Implications

The studies also shed light on the process behind previously documented biases in relative estimates and suggest ways for marketers to help debias consumers' estimates. The results indicate that marketers have a fairly straightforward method of improving customers' choices—make extreme instances from the distribution more accessible. Thus, marketers need not necessarily dumb down products or rewrite manuals to ensure satisfied customers, nor tighten return policies to discourage refunds. Instead, marketers can ensure better consumer product matching by simply adjusting consumers' relative self-perceptions through their impressions of others. Take the poncho example once again. It may often be cumbersome and expensive for marketers to manipulate product arrays to influence choice. Furthermore, most consumers know their height, so it is unlikely that a marketer could change their perception of it. However, consumers are likely to have a less well-defined conception of how tall others are, a concept marketers can leverage.

There are multiple, even simultaneous, ways to help consumers choose appropriate levels of products. For instance, in domains where consumers hold overly positive views of their relative standing, it might be simplest to recategorize products such that amateur products are labeled "advanced" and so on. However, in many domains the direction of a particular person's bias is not always clear. Furthermore, any consumers who are in fact accurate in their relative assessments may be led astray by array manipulations. For instance, a consumer who thinks that a task is easy for himself and for others will correctly infer that he is average and choose products appropriate for his standing: providing feedback that others found the task easy will not lead him astray. However, providing this feedback to a consumer who believes that the task is easy only for her but less so for others will help her adjust her relative assessment and purchase. Because it does not assume that all consumers are miscalibrated or that they are biased in a certain direction and because it can debias an errorful consumer without leading an accurate one astray, we think that the distribution-manipulation approach is a particularly valuable tool for marketers. In fact, Kruger and Dunning (1999) found that (at least some) participants corrected their errorful percentile estimates after examining a representative sample of reference group performances.

Finally, the present research adds to a growing body of literature suggesting that individuals may be more sensitive to others in estimates of relative standing than previously reported. In particular, for tasks involving ability or skill, people may have better information about themselves than others. So it may be rational to attend mainly to estimates of one's own ability and discount errorful estimates about others (Burson and Klayman 2010; Windschitl et al. 2008). Moore and Small (2007) also argue that estimates of an average person may be systematically errorful and regressed toward the mean of a scale. Indeed, overdispersion in estimates of distributions of others would also result in an average person being closer to the mean. However, our re-

sults suggest that even when one's own score and the median of others' scores are held constant, the estimated dispersion of the others influences estimates of relative standing.

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