The Influence of Egocentrism and Focalism on People's Optimism in Competitions: When What Affects Us Equally Affects Me More

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Six experiments investigated people's optimism in competitions. The studies involved hypothetical and real competitions (course grades in Experiments 1 and 2, a trivia game in Experiments 3–5, and a poker game in Experiment 6) in which the presence of shared adversities and benefits (factors that would generally hinder or help the absolute performance of all competitors) was manipulated. Shared adversities tended to reduce people's subjective likelihoods of winning, whereas shared benefits tended to increase them. The findings suggest that when people judge their likelihood of winning, their assessments of their own strengths and weaknesses have greater impact than their assessments of their competitors' strengths and weaknesses. We identify egocentrism and focalism as two causes of the bias. The experiments revealed moderators of this bias, but also illustrated its robust nature across a variety of conditions.

Competition is ubiquitous. Whether applying for a job, vying for an "A" on a curved exam, or waging war, many of life's most consequential pursuits are competitive in nature. As such, success depends on relative strength. An applicant's chances of being hired is a function of not only her own ability to impress her potential employer, but also the ability of her fellow applicants to do the same. A student's likelihood of earning an "A" on a curved exam depends not only on his own knowledge of course material, but also the knowledge of his classmates. And a nation's ability to win a war depends not only on the nation's own arsenal and tactics, but also those of the competing nation. We suggest, however, that when people gauge their chances of success in competitions, they often pay far greater attention to their own strengths and weaknesses than to the strengths and weaknesses of their competitors.

We base this hypothesis, at least in part, on several recent findings from the growing literature on people's judgments of how their traits, abilities, and other attributes compare with others (Eiser, Pahl, & Prins, 2001; Klar & Giladi, 1999; Kruger, 1999; Price, Pentecost, & Voth, 2002; Weinstein & Lachendro, 1982). In particular, Klar and Giladi (1999) found that students' judgments of how content they were relative to other students tended to be highly correlated with their judgments about their own level of contentment, but largely uncorrelated with their judgments about other students' level of contentment (see also Diener & Fugita, 1997). Kruger (1999) found a similar relationship in social comparisons of ability; participants' ratings of their comparative abilities were based far more on their own skills than on the skills of the comparison group. As a result, participants rated themselves as better than average when asked about easy skills (e.g., operating a computer mouse), but worse than average when asked about more difficult skills (e.g., computer programming). These and related findings suggest that when people are asked to judge themselves relative to a reference group, self-assessments (How good am I?) have a greater impact than other-assessments (How good are others?).

Applied to competitions, this tendency to pay greater attention to self-assessments than to other-assessments leads to a key prediction about how people will react when facing what we call a *shared adversity*. Specifically, when a competitive situation presents an adversity shared by all competitors, the average optimism of the competitors will be reduced. For example, upon learning that the temperature at an upcoming marching band competition will be uncharacteristically cold, band members might tend to dwell on how frozen fingers would affect their own band's ability to sound impressive, and they might be less cognizant of how frozen fingers might hurt the performance of other bands. The net

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result would be an overall decrease in optimism across the competing bands. A similar logic suggests the opposite result for a *shared benefit.* For example, if candidates in a school board race are given 4 hr of free advertising by a local television station, each candidate might focus more on how the free advertising can advance the publicity of their own campaign rather than how it can advance the publicity of the other campaigns. The net result would be an overall increase in optimism across the candidates.

Of course, "shared" benefits and adversities do not always apply to all competitors equally. For example, a political candidate who is relatively unknown to voters might benefit more from free publicity than would an incumbent. As well, severe cold might cause all bands to sound terrible and nearly indistinguishable, hurting superior bands more than inferior ones. Hence, a "shared" benefit can appropriately lead some competitors to become more optimistic and others to become less optimistic, and the same is true for a "shared" adversity. However, an objective perspective dictates that the average likelihood of winning across the full set of competitors is unchanged by the introduction of a shared circumstance (the sum of the likelihoods would remain at 1.0 assuming there will be exactly one winner). Nevertheless, we predict that the average optimism displayed by competitors will increase in the case of a shared benefit and decrease in the case of a shared adversity.

Egocentrism and Focalism

Why might self-assessments and other-assessments have differential impact on optimism? We focus primarily on two possible reasons, although we also discuss a third viable reason later in the article. One reason stems from what might be best termed *egocentrism*, the notion that the self figures more prominently in judgments than do others (cf. Inhelder & Piaget, 1958; L. Ross & Ward, 1996; M. Ross & Sicoly, 1979). There are nearly inevitable differences in the quantity and accessibility of knowledge people have about themselves versus others and in the attention they devote to self-relevant versus other-relevant activities (see Fenigstein, 1984; Kuiper & Rogers, 1979; Markus, 1977; M. Ross & Sicoly, 1979; Srull & Gaelick, 1983). Hence, when a competitor learns of a shared adversity (or benefit), thoughts about how the adversity might affect his or her own performance would likely come to mind more easily and in a larger quantity than thoughts about how the circumstance might affect another's performance. For example, a band member who learns that there will be cold weather at a competition may spontaneously recall his or her poor performances at previous cold-weather competitions but not spontaneously recall similar instances regarding other bands. Relatedly, self/other differences may cause people to hold more confidence in estimating how a shared circumstance would affect themselves than their competitor. A band member may expect that everyone's fingers will stiffen in cold weather, but because of direct experiences in past cold-weather competitions, her expectations about her own fingers may be held with more confidence than her expectations about other people's fingers. Hence, our egocentrism account assumes that various components of self-other knowledge differences may cause people to base their subjective likelihood of winning on thoughts about how a shared circumstance will affect themselves more so than on thoughts about how the circumstance will affect their competitor.

A second reason why self-assessments might have greater impact than competitor-assessments stems from *focalism*. Focalism, broadly construed, is the tendency of people to focus on information relevant to one outcome and fail to adequately consider evidence or consequences relevant to other possible outcomes (Schkade & Kahneman, 1998; Wilson, Wheatley, Meyers, Gilbert, & Axsom, 2000). Recent research has shown that when people are asked to judge the likelihood that one outcome (the focal outcome) will occur rather than some other outcome, the evidence directly relevant to the focal outcome can often have disproportionately greater impact than the evidence directly relevant to the complementary outcome (see, e.g., Fox & Levav, 2000; Idson, Krantz, Osherson, & Bonini, 2001; Macchi, Osherson, & Krantz, 1999; McKenzie, 1998, 1999; Windschitl, 2000; Yamagishi, 2002). Applied to the present case, when people are asked to judge the likelihood that they will win a competition, their attention is drawn to that outcome (the self winning) rather than its complement (someone else winning), causing them to focus on how circumstances might influence their own performance more than how circumstances might affect their competitor's performance. The key difference, then, between the egocentrism and focalism accounts is that whereas the egocentrism account suggests that the greater impact of self-assessments relative to other-assessments originates in stable self/other differences, the focalism account suggests that the greater impact of self-assessments originates in the explicit focus of the likelihood question (i.e., the fact that the respondent is asked about his or her own likelihood of winning, not the competitor's likelihood of winning).

Overview of Experiments

To summarize, we have suggested that when people estimate their chances of winning a competition, they often pay greater attention to their own strengths and weaknesses than to the strengths and weakness of their competitors. Hence, we predicted *shared-circumstance effects*: People's optimism about winning a competition will be greater when a competitive situation introduces a shared benefit rather than a shared adversity. This prediction was tested in a variety of contexts across six experiments. We also tested the egocentrism and focalism accounts that we identified, as well as potential moderators of the shared-circumstance effects. In Experiments 1 and 2, we used scenarios about college courses to investigate the impact that shared adversities and benefits have on students' optimism about getting a desired grade in courses that use strictly relative grading. In Experiment 3, a trivia-game paradigm was used to investigate the predicted shared-circumstance effects when participants were about to engage in a real competition and when they faced only one competitor. For Experiment 4, we used the trivia-game paradigm to test whether mechanisms related to egocentrism, focalism, or both might underlie the shared-circumstance effect, and in Experiment 5 we used a similar paradigm to assess whether knowledge of one's competitor moderates the effect. Finally, in Experiment 6, we investigated the impact of shared circumstances on a behavioral dependent measure in a common gambling game—poker.

Experiment 1—Course Grades

College courses with high enrollment often use strictly relative grading in which the top X% percentage of students will earn As,

the next Y% earn Bs, and so on. Students in such courses are essentially competing for grades. Hence, a difficult exam (a shared adversity) or lenient scoring on an assignment (a shared benefit) should have no impact on the average optimism that students in the course have regarding the possibility of getting a good grade. However, our intuition and the theorizing discussed above suggested otherwise. Potential causes related to both egocentrism and focalism suggested that a shared adversity would deflate optimism and a shared benefit would inflate it.

To test this prediction in Experiment 1, we had our participants imagine they were in a large sociology class for which relative grading was used. The scenario that they read suggested that prior to the final exam, their coursework had been approximately average. The end of the scenario introduced either (a) a shared adversity that would make it difficult for students in the course to study for the final, (b) no new information, or (c) a shared benefit that would make it easy to study for the final. After reading the scenario, participants responded to questions related to optimism and predictions about their grade for the course. We expected average optimism to be lowest in the shared-adversity condition, higher in the control condition, and even higher in the shared-benefit condition.

Method

Participants and design. The participants were 46 students enrolled in a research methods course at the University of Illinois at Urbana–Champaign, who earned course-related credit for participating. They were randomly assigned to either an adversity, control, or benefit condition.

Materials and procedure. All participants started by reading the following main scenario:

> You are taking an introductory sociology class. Your grade for the class is based upon your performance on three exams (two regular unit exams and a final, cumulative exam). The class is graded on a curve. The top 15% of the students in the class will earn "As" for the course, the next 34% will earn "Bs," 40% will earn "Cs," 8% will earn "Ds," and the lowest 3% of students will earn "Fs." You think of yourself as an average student, and your scores in the class so far reflect that. You received a score in the middle of the class distribution for the first two exams. There is one more exam to take for the course before final grades are assigned. The final is a comprehensive exam. This means that any material covered since the beginning of the semester may be on the exam. According to the syllabus, the professor will pass out a sheet of 60 possible exam terms on the Monday before finals week (8 days prior to the final exam).

Participants in the control condition read nothing more than this main scenario. Participants in the adversity condition read the main scenario plus the following:

> On the Monday before finals (when the terms are supposed to be handed out), the professor announces that the class is further behind in the material than was planned. Instead of handing out the terms on Monday and using Wednesday and Friday for in-class review sessions, class time on these two days will be used for covering the course material of the final unit. The professor will make the sheet of possible exam terms available on the last day of class, but it will be up to students to review them on their own for the final.

Participants in the benefit condition read the main scenario plus the following:

> On the Monday before finals, the sheet of possible exam terms is given to the class. The professor also announces that the final unit of material will be dropped from the course, freeing up two class days. Therefore, the class times on Wednesday and Friday will be used as review sessions in which the class will go over each of the terms on the review list.

Finally, participants responded to three key dependent measures. The first asked participants to predict their final letter grade for the course. For analyses, these responses were converted onto a 0–12 scale (0 = *F*, 1 = *D*−, . . . , 12 = *A*+). The second measure asked participants to estimate the likelihood that they would earn a B or better in the course (1 = *extremely unlikely*, 7 = *extremely likely*). The final measure asked students to estimate their final percentile rank in the course (from 0 to 99). This rank question included examples of what various possible responses would mean.

Results and Discussion

The means for our three key dependent variables—grades, likelihoods, and ranks—are displayed in Table 1. As part of an overall analysis, the data from these variables were *z* scored and then combined to form an optimism value for each participant. A one-way analysis of variance (ANOVA) on these optimism values produced a significant overall effect, $F(2, 43) = 4.28$, $p < .05$. A pairwise comparison between the adversity and benefits conditions revealed that, consistent with our main prediction, participants were significantly more optimistic when they read about a shared benefit than a shared adversity, $t(43) = 2.83$, $p < .05$. Additional comparisons indicated that, as expected, shared adversities reduced optimism relative to the control condition, $t(43) = 2.03$, $p < .05$, but our expectation that a shared benefit would increase optimism relative to the control condition was not supported, $t(43) = 0.78$, $p = .44$.

The results of planned comparisons on the raw data for each dependent variable are shown in Table 1. These comparisons lead to conclusions largely consistent with those described above. As can be seen from Table 1, participants' predictions of their final letter grades, their judged likelihood of getting a B, and their predicted final rank in the course were higher in the benefits condition than in the adversity condition.

Experiment 2—Grades Redux

The results of Experiment 1 provide initial evidence that average optimism is greater when a shared benefit rather than a shared adversity is introduced into a competition over grades. However,

Table 1
Mean Responses by Circumstance Condition in Experiment 1

	Circumstance condition					
	Adversity		Control		Benefit	
Dependent measure	*M*	*SD*	*M*	*SD*	*M*	*SD*
Final letter grade	7.00_a	1.51	7.80_b	1.15	7.80_b	1.08
Likelihood of B or better	4.44_a	1.09	5.07_{ab}	1.10	5.60_b	1.06
Rank in class	60.63_a	16.62	68.33_{ab}	12.20	71.27_b	11.07

Note. Within measures, means that do not share a subscript are significantly different at $p < .05$, one-tailed.

because that experiment involved a between-subjects design, the analyses tell us only how average optimism differed among the three conditions; the analyses provide only limited information about how individual participants reacted to the shared circumstances. It could be the case that the reduction in the average optimism in the shared-adversity condition was attributable to a minority of participants who had a particularly strong negative reaction to the adversity. In Experiment 2 we thus investigated the same general hypotheses that were investigated in Experiment 1, but we used a within-subjects methodology that allowed us to better understand how individual participants reacted to the news of a shared circumstance. Specifically, participants read a scenario that asked them to imagine being a student in a large course. They then read a list of hypothetical course-related events. For each event, the participants rated the extent to which it would hurt or help their chances of scoring in the top half of the course distribution. Some of these events constituted unique adversities or benefits (i.e., events that might influence their own performance in the course but not the performances of others), but the critical events constituted either shared adversities (e.g., the instructor squeezed 3 days of class material into one class period) or shared benefits (e.g., the instructor used an easy exam).

Not only did this methodology allow us to assess how individual participants would react to shared circumstances, but it also allowed us to assess reactions to several different types of shared adversities and benefits. This was important for two reasons. First, given that the shared benefit that was described in Experiment 1 did not significantly increase optimism relative to the control condition, it was important to test whether other possible operationalizations of shared benefits might show evidence of boosting optimism. Second, we wanted to include some highly conservative tests of shared-circumstance effects, in which the shared nature of the circumstance would be very clear to participants. Hence, we included circumstances such as "the instructor decides to deduct 10 points from everyone's final score."

Finally, it is important to note that in Experiment 2, we did not describe a relative grading scheme for the course. Instead, the materials simply asked respondents about how the course-related events would influence their chances of scoring in the top half of the class. We did this because we could not completely rule out the possibility that participants had personal conceptions or possible misunderstandings of how relative grading is implemented. The fact that the shared-circumstances manipulation in Experiment 1 had a robust influence on the rank-order responses suggests that personal misunderstandings of relative grading did not account for the general findings in Experiment 1, but additional support for this type of conclusion would be helpful.

Method

Participants. The participants were 89 students enrolled in a research methods course at the University of Illinois at Urbana–Champaign, who earned course-related credit for participating.

Materials and procedure. All participants read the same scenario that started with the following:

> Imagine you are taking a social psychology class at a large university. You really want to finish in the top half of this class. Students who finish in the top half (i.e., those who score higher than 50% of their classmates) become eligible for an upper-level course that involves field trips that are very interesting to you.

After reading additional details about the course, participants read a total of 19 course-related events. For each one, they estimated the influence of the event on a scale from –3 (*would greatly hurt my chances of scoring in the top half*) to +3 (*would greatly help my chances of scoring in the top half*). Of the 19 events, most constituted either a unique adversity or unique benefit. However, four critical events were shared adversities and three were shared benefits. These shared circumstances were located sporadically among the list of 19 events. Table 2 displays a representative sample of the unique circumstances, as well as all of the shared circumstances.

Results and Discussion

Table 2 also displays, for each circumstance or event, the number of participants who indicated that the event would hurt their chances of scoring in the top half of the class (responses of −3, −2, or −1), have no effect (a response of 0), or help their chances (responses of +3, +2, or +1). The table also indicates the mean response for each event and whether the mean response is significantly different from 0. As can be seen from Table 2, the results strongly support our hypotheses about how participants would respond to shared circumstances. For each of the four shared adversities, the mean response was significantly less than 0; participants tended to report that the shared adversities would hurt their chances of scoring in the top half of the class. For example, 70 participants thought that their chances would be hurt if an instructor "decided to use an exam that he/she typically uses for a graduate-level course," whereas only 2 thought this shared adversity would help their chances and 17 thought that it would not have an effect. For each of the three shared benefits, the mean response was significantly greater than 0; participants tended to report that the shared benefits would help their chances of scoring in the top half of the class.

In addition to these main findings, we wish to emphasize three conclusions from these results. First, unlike the results of Experiment 1, the findings indicate that shared-circumstance effects can operate in both directions. Not only do shared adversities lower the average optimism of a group of competitors, but shared benefits raise the average optimism.

Second, the distributions of responses for the events suggest that shared-circumstance effects—defined by shifts in average optimism—are not isolated to or driven by only a small set of respondents. The imbalances between the number of participants reporting that a shared circumstance would hurt their chances and number reporting that it would help were quite severe.

Third, even when an event description seemed to clearly highlight the shared nature of the shared circumstance, the hypothesized effects remained robust. Consider, for example, how our 89 participants responded to the following: "The instructor decides that the final exam is too hard and decides to add 10 points to everyone's final score." Although 42 of the 89 participants indicated that this would not affect their chances of scoring in the top half of the class, 45 indicated that it would help their chances and 2 indicated it would hurt. It is also interesting to note that even among participants who seemed to appreciate the shared nature of the above circumstance (i.e., the 42 participants who answered "0"), their responses to other shared adversities reflected the predicted bias. For example, of these 42 participants, 29 reported that if the instructor "decided to use an exam that he/she typically uses for a graduate-level course," this would hurt their chances, whereas the remaining 13 provided a "0" response.

Table 2
Frequencies and Means for Responses to Unique Adversities and Benefits as Well as Shared Adversities and Benefits in Experiment 2

Event type	Frequencies			*M*	*SD*
	−	0	+		
Unique adversities (examples)					
You lose your textbook the night before the final exam.	80	8	1	−1.51*	0.95
You miss three lectures and don't get notes.	86	2	1	−2.09*	0.89
Unique benefits (examples)					
A roommate of yours helps you with the course material.	0	4	85	1.79*	0.76
You took an advanced high school course on the same topic as the class.	0	2	87	1.71*	0.76
Shared adversities					
The instructor decides that the final exam is too easy and decides to deduct 10 points from everyone's final score.	43	42	6	−0.72*	1.08
On the comprehensive final exam, 40 questions come from material covered in the first half of the semester and only 10 questions come from material covered in the last half.	57	19	13	−0.76*	1.19
For the final exam, the instructor decides to use an exam that he/she typically uses for a graduate-level course.	70	17	2	−1.42*	1.15
Two class periods near the end of the semester are canceled due to weather, which forces the instructor to squeeze 3 days of material into one class period.	57	29	3	−0.90*	0.91
Shared benefits					
The instructor accidentally makes the final exam very easy. None of the students in the course misses more than 5 of the 50 multiple choice items.	28	19	42	0.60*	1.81
The instructor decides that the final exam is too hard and decides to add 10 points to everyone's final score.	2	42	45	0.98*	1.19
The instructor is so impressed with the special writing assignments . . . that he/she gives all students the maximum score for that assignment.	6	46	37	0.76*	1.21

Note. The response options for these events ranged from −3 (*would greatly hurt my chances of scoring in the top half*) to +3 (*would greatly help my chances of scoring in the top half*). Numbers in the "−" column indicate the frequencies of −3, −2, and −1 responses, and numbers in the "+" column indicate the frequencies of +3, +2, and +1 responses.
* Significantly different from 0 (all *p*s $< .01$).

Experiment 3—The Trivia Paradigm

The results of Experiments 1 and 2 provide evidence that in at least one competitive domain—the competition over grades—people are more sensitive to how circumstances affect their own potential to perform well than their competitors' potential to perform well. We attribute this finding to a general tendency of people to focus on self-assessments more than other-assessments (the egocentrism account), and the fact that the optimism question guides a person's attention more toward the self than toward the person's competitors (the focalism account).

There is, however, a third viable explanation, one that applies to cases in which a person faces more than just one competitor, as in Experiments 1 and 2. In these cases, other-assessments may have less impact than self-assessments because the other-assessments are compound assessments of a group of persons, whereas self-assessments concern just one person. It could reasonably be argued that compound assessments are made with less accuracy, ease, and/or confidence than are assessments about only one individual (e.g., the self). This type of *multiple-competitor account* follows from support theory, which suggests that evidence about a hypothesis (e.g., that the German hockey team will win the gold medal) is evaluated less effectively when the hypothesis is embedded with other hypotheses in a disjunction (e.g., that a European team will win the gold) than when the hypothesis is considered explicitly and singly (see Koehler, Brenner, & Tversky, 1997; Rottenstreich & Tversky, 1997; Tversky & Koehler, 1994). A related idea is that a group of competitors, as opposed to one concrete competitor like the self, can seem rather abstract or generalized, and therefore people might have some difficulty in appreciating the impact of events on that group of competitors (see Alicke, Klotz, Breitenbecher, Yurak, & Vredenburg, 1995; Klar & Giladi, 1997; Perloff & Fetzer, 1986).

In the General Discussion, we elaborate on the role that the mechanisms of the multiple-competitor account might play in many shared-circumstance effects, such as the effects detected in Experiments 1 and 2. However, for Experiment 3, we wished to rule those mechanisms out as a critical (i.e., necessary) cause of shared-circumstance effects. Hence, for Experiment 3 we designed a paradigm in which each participant would play a trivia game against a single competitor. Both the egocentrism and focalism accounts led us to expect shared-circumstance effects even in this two-person game.

The competition that we developed had the following key components. Participants, tested in pairs, had a very short introductory conversation before being told that they were about to compete in a trivia-challenge game. The game was said to involve privately answering trivia questions from a small set of categories that would be randomly determined from a list, and the person who correctly answered the most questions from a category would be deemed the winner of that category. Prior to beginning the game, the participants were given a list of 30 possible categories, and they indicated the likelihood of winning each category if it were played.

The shared-circumstances manipulation took a slightly different form in Experiment 3 than it had in Experiments 1 and 2. Specifically, we manipulated the ease or difficulty of the 30 trivia categories. Fifteen of the categories had been identified through pilot testing as being perceived as generally easy by college undergraduates (e.g., fast food chains, current events), and 15 had been identified as being generally hard (e.g., home insurance facts, 1950s movies). The key question addressed in this study was whether participants would exhibit more optimism about winning an easy category than a hard category. From an objective perspective, each category—whether generally easy or hard—would have one winner, so average optimism should not differ for easy versus hard categories. However, we predicted that because people focus on their own rather than their competitor's ability to perform well, participants would be more confident about winning an easy category than a difficult category.

As an additional manipulation in this experiment, we had participants make responses on either a standard 100-point likelihood scale or a 7-point scale that had labeled endpoints. We included this manipulation because recent research has indicated that standard 100-point numeric measures and other types of likelihood measures (e.g., those involving a set of points anchored with verbal-likelihood labels) can be differentially sensitive to some likelihood-judgment phenomena (see, e.g., Kirkpatrick & Epstein, 1992; Teigen, 1988, 2001; Windschitl, 2000; Windschitl, Martin, & Flugstad, 2002; Windschitl & Wells, 1996, 1998).

Method

Participants and design. The participants were 50 students from elementary psychology classes at the University of Iowa, who participated to fulfill a research exposure component of the course. We used a 2 × 2 mixed design in which the category difficulty (hard or easy) was manipulated within subjects and response scale (standard 100-point or 7-point) was manipulated between subjects.

Procedure and materials. The participants, tested in pairs, began the experiment by having a 5-min conversation about an ice-breaker topic (only one pair had previously met). Then each participant received a questionnaire booklet and read the following information:

> In a few moments you will be playing a trivia game against the other research participant. Three trivia categories will be randomly chosen from the list below. You will be asked 5 questions in each of the categories for a total of 15 questions. Whoever answers more questions correct in a given category will be the winner. In the event of a tie for a given category, you will complete tie-breaker questions from that category until a winner is determined. On this page, you should indicate what you think your chances are of winning each category.

The instructions went on to describe either the 0%-to-100% scale or the 7-point scale (1 = *very unlikely I would win*, 7 = *very likely I would win*).

The list of possible categories contained 30 randomly ordered entries, 15 of which were easy categories and 15 of which were hard categories (see Table 3). We selected these categories from a larger pool of categories that were pilot tested by having undergraduate students rate how difficult most college undergraduates would perceive them.

After participants in the experiment provided their 30 likelihood estimates (one for each category), they indicated how difficult or easy they thought most college students would find each category (1 = *very difficult*, 7 = *very easy*), how desirable it would be to know more than most other college students about each category (1 = *very undesirable*, 7 = *very desirable*), and whether they felt they had more knowledge than other college students about each category (1 = *much less knowledge than others*, 7 = *much more knowledge than others*). Before the trivia game (the outcome of which was not recorded), participants also completed the Need for Cognition (NFC) Scale (Cacioppo & Petty, 1982), a five-factor personality measure (the Big Five Inventory [BFI]; John, Donahue, & Kentle, 1991), and a dispositional optimism scale (the Life Orientation Test [LOT]; Scheier, Carver, & Bridges, 1994). Finally, participants answered some demographics questions and questions about their general preference for trivia games and their beliefs about their competitor's knowledge of trivia.

Results

Manipulation check. The ratings of how easy most college students would find the categories served as manipulation checks for our preselected easy and difficult categories. Consistent with our pilot work, the average rating across our 15 easy categories ($M = 5.84$, $SD = 0.54$) was in fact significantly higher than the average rating across our 15 hard categories ($M = 2.49$, $SD = 0.60$), $F(1, 49) = 992.69$, $p < .001$.

Likelihood judgments. For our full analysis of the likelihood data, we converted each likelihood response to a participant-specific[1] *z* score and then used the *z* scores to compute each participant's mean optimism for the hard categories and for the easy categories. These means were then submitted to an ANOVA with category difficulty (hard or easy) as a repeated measure and response type as a between-subjects factor. This approach allowed us to compare and combine findings from 100-point and 7-point responses scales. However, for clarity and simplicity, we will primarily report means and analyses based on the raw data. The conclusions drawn from these analyses are identical to those drawn from the ANOVA on the *z* scores.

Table 3 shows the means of participants' likelihood estimates for each of the 30 categories. Our primary interest was whether participants' optimism about winning the easy categories would be greater than their optimism about winning the hard ones. As expected, participants providing their likelihood estimates on a 7-point scale expressed greater average optimism about winning the easy categories than the hard ones, $F(1, 25) = 313.10$, $p < .001$. This effect was also robust for participants answering on a standard 100-point scale, $F(1, 23) = 204.01$, $p < .001$. The interaction test from the above-mentioned ANOVA on *z* scores indicated that the hard/easy effect was not significantly moderated by response type, $F(1, 48) = 0.36$, $p = .55$.

[1] To convert a given response to a participant-specific *z* score, we used the mean and the standard deviation of that participant's responses for the 30 categories. The resulting *z* score indicates how optimistic the participant was about that category relative to his or her optimism about the full set of categories.

Table 3
Mean Likelihood Judgments From Experiment 3 by Category and Scale Type

Categories	100-point scale		7-point scale	
	M	*SD*	*M*	*SD*
Hard categories				
19th century French painting	11.67	18.10	1.65	1.29
50s movies	23.63	21.54	2.42	1.10
Ancient civilizations	23.46	28.21	2.44	1.36
Baroque music	3.75	6.75	1.81	1.58
Dates in history	38.83	28.94	2.38	1.36
Eastern philosophy	21.38	25.78	1.77	1.37
European politics	21.46	28.57	2.27	1.15
Famous composers	23.33	21.80	3.04	1.95
Famous rivers	44.79	16.78	3.77	1.39
History of Mesopotamia	6.13	9.95	1.42	0.76
Home insurance facts	7.00	14.15	1.81	1.23
Indigenous vegetation of Amazon	5.67	10.72	1.38	0.80
Latin American history	19.33	27.69	1.50	0.76
Russian literature	10.63	22.12	1.50	0.91
South American geography	26.04	26.54	1.81	0.94
Overall (hard)	**19.14**	**12.14**	**2.07**	**0.75**
Easy categories				
Adam Sandler movies	75.21	29.02	5.19	1.44
Brands of alcohol	63.13	25.99	5.19	1.88
Celebrities	55.83	19.21	4.15	1.32
Clothing fads	60.08	30.83	5.00	1.44
Current events	59.38	19.35	4.35	1.13
Driving rules	72.04	25.74	5.72	1.10
Famous cartoon characters	67.25	25.30	4.96	1.51
Fast food chains	70.42	15.87	5.27	1.12
Pop culture	59.63	21.88	4.69	1.44
Pop music	60.83	29.44	5.15	1.01
Rock 'n' roll	63.13	26.08	4.85	1.49
State capitols	60.17	29.55	4.50	1.68
TV sitcoms	69.58	20.58	4.92	1.44
U.S. geography	61.88	24.22	4.65	1.29
Using a personal computer	48.75	19.69	4.58	1.58
Overall (easy)	**63.15**	**11.96**	**4.88**	**0.63**

The magnitude and consistency of the shared-circumstance effect detected in this study is striking. As can be seen from a visual inspection of Table 3, each of the numeric-likelihood means for the 15 hard categories fell below 50%, whereas all but one of the numeric-likelihood means for the 15 easy categories fell above 50%. In other words, participants tended to be unrealistically *optimistic* about winning the easy categories, but unrealistically *pessimistic* about winning the hard ones. The means from the 7-point condition show a similar pattern. It appears that when participants were considering their chances of winning an "easy" category such as driving rules, they tended to feel quite optimistic about beating their competitor. The rub is that their competitors also tended to feel optimistic about beating them. In fact, of the 50 participants, only 3 gave a likelihood response for the driving rules category that was below the midpoint of the response scale they were using; 3 participants gave the midpoint response and the rest gave responses above the midpoint. Likewise, for a "hard" category like European politics, participants and their competitors tended to feel pessimistic; only 5 participants gave responses above the midpoint and 4 gave the midpoint as their response. Finally, it is instructive to note that of the 50 participants in this study, all expressed greater average optimism about winning the easy categories than the hard ones.

Other dependent measures. Analyses of participants' ratings of whether they knew more about a category than the average college student produced findings that are highly consistent with Kruger's (1999) findings regarding people's comparative assessments of skills. Namely, participants' ratings of how their knowledge compared with their competitor's knowledge were more favorable for the easy categories ($M = 4.50$, $SD = 0.71$) than for the hard categories ($M = 2.53$, $SD = 0.96$), $F(1, 49) = 136.18$, $p < .001$. To see if these ratings were related to likelihood judgments, we computed, for each participant, a correlation between his or her knowledge ratings and his or her likelihood judgments across the 30 categories. Not surprisingly, these correlations tended to be quite strong, the average of which was .81.

Knowledge-desirability ratings were also higher for the easy ($M = 4.65$, $SD = 0.99$) than the hard ($M = 3.73$, $SD = 1.34$) categories, $F(1, 49) = 13.89$, $p < .01$. Although this finding raises the question of whether differences in desirability account for the differences in optimism, such an explanation is implausible given findings from other analyses. For instance, we computed, for each participant, a partial correlation between category type (1 = *easy* and 0 = *hard*) and his or her likelihood ratings for the categories, controlling for his or her desirability ratings. Those partial correlations tended to be quite robust ($M_r = .66$) and only slightly

smaller than the zero-order correlations between the category type and likelihood ratings ($M_r = .75$).

Individual differences. The magnitude of the shared-circumstance effect on likelihood judgments was not significantly related to participants' gender, grade point average, reported liking of trivia games, beliefs about their competitors' general knowledge of trivia, scores on the LOT, or scores on the NFC scale (all rs $< .20$). The effect was also not significantly related to scores on the Neuroticism ($r = -.20$), Conscientiousness ($r = .17$), or Openness ($r = -.06$) scales of the BFI. It was, however, significantly but only moderately correlated with scores on the Extraversion ($r = .39$, $p < .01$) and Agreeableness ($r = .32$, $p < .05$) scales. It appears that this effect is not unique to any particular type or special subset of participants delineated by these measures.

Discussion

The results of Experiment 3 extend those of the previous two experiments in several important ways. First, shared-circumstance effects were demonstrated in a substantially different competitive context—a trivia game rather than course grades. Second, the competition was real rather than hypothetical. Third, the results of Experiment 3 show that a shared benefit can cause people to be unrealistically optimistic and a shared adversity can cause people to be unrealistically pessimistic. Finally, the results of Experiment 3 show that the shared-circumstance effect is not unique to situations in which a person faces multiple competitors. Although this finding does not rule out the possibility that the mechanisms described by a multiple-competitor account might have causal relevance to some form of shared-circumstance effects, those mechanisms are not necessary for producing the effect.

Experiment 4—Trivia Redux: Egocentrism or Focalism?

Both the focalism and egocentrism accounts described earlier provide ready explanations for the results of Experiment 3. According to the focalism account, participants overestimated their chances of beating their competitor on an easy category such as "TV sitcoms" because they overweighted assessments related to the focal outcome (assessments about their own ability to perform well in that category) relative to assessments related to the alternative outcome (assessments about their competitor's ability to perform well). According to the egocentrism account, the overestimation occurred because of a general tendency of people to focus on self-relevant assessments at the expense of other-relevant assessments. We suspected that both focalism and egocentrism contributed to the results of Experiment 3. However, the contributions of focalism and egocentrism cannot be separately validated because the focal outcome and the egocentric outcome were confounded in Experiment 3.

In Experiment 4, we introduced a simple manipulation designed to unconfound the two outcomes. Specifically, we varied whether participants provided their own chances of winning the trivia categories (the self-target condition) or their competitor's chances of winning (the other-target condition). The self-target condition is simply a replication of Experiment 3, so we expected to observe a robust shared-circumstance effect in that condition. Consistent with our belief that both egocentrism and focalism can contribute to shared-circumstance effects, our predictions regarding the other-target condition had two key elements. First, we expected that, because of egocentrism, participants in that condition would tend to estimate their competitor's likelihood of winning as lower for easy categories than for hard ones. For example, when participants are asked about their competitor's chances of winning an easy category like "TV sitcoms," they would note that because they themselves know a lot of TV sitcoms, their competitor would be unlikely to win. Second, we expected that, because of the influence of focalism, the *absolute* magnitude of the shared-circumstance effect in the other-target condition would be smaller than that in the self-target condition. In the self-target condition, both egocentrism and focalism would bias the participants toward attending to self-knowledge rather than other-knowledge. However, in the other-target condition, although egocentrism would still exert a bias toward self-knowledge, focalism would exert a bias toward other-knowledge (because the focal outcome in the likelihood question would be the competitor winning). In other words, whereas focalism and egocentrism would combine to produce a large shared-circumstance effect in the self-target condition, focalism would partially mitigate the effect of egocentrism in the other-target condition.

In addition to the target manipulation (self or other), Experiment 4 involved two smaller changes designed to provide supplementary evidence regarding the mechanisms underlying the shared-circumstance effects. First, participants estimated their own and their competitor's knowledge of each category. These ratings allowed us to examine the relations among self-knowledge assessments, other-knowledge assessments, and likelihood judgments. Second, participants were asked to write explanations for two of their likelihood estimates. We suspected that the number of self-references (e.g., "I," "my") versus other-references (e.g., "she") might provide a rough index of a participant's tendency to base his or her likelihood judgment on self-relevant versus other-relevant information.

Method

Participants and design. The participants were 160 students from elementary psychology classes at the University of Iowa, who participated to fulfill a research exposure component of the course. The design was a 2×2 mixed design in which the category difficulty (hard or easy) was manipulated within subjects and the target of the likelihood questions (self-target or other-target) was manipulated between subjects.

Procedure and materials. The procedures and materials were similar to those of Experiment 3 except as noted here. After receiving the description of the trivia game they were about to play, participants made three sets of estimates for the 30 categories: (a) numeric likelihood estimates ranging from 0% to 100%, (b) estimates of their own category knowledge (1 = *very little knowledge*, 7 = *a great deal of knowledge*), and (c) estimates of their competitor's category knowledge (1 = *very little knowledge*, 7 = *a great deal of knowledge*).

For participants in the self-target condition, the likelihood questions read as follows: "The chance that you will win the X category is ___%." For those in the other-target condition, the questions read as follows: "The chance that the other participant will win the X category is ___%." Some participants completed the set of likelihood estimates before the knowledge estimates, whereas others did the reverse. Within this counterbalance factor, the order of self-knowledge and other-knowledge question sets was also counterbalanced. Neither of these counterbalancing factors interacted with the category-difficulty factor or the target factor on the likelihood measures, so the counterbalancing factors will not be further discussed.

Finally, before participants completed demographics questions and played a round of trivia, they were asked to write explanations for why they gave the chance estimates that they did for the "Rock 'n' Roll" category (a randomly selected easy category) and the "History of Mesopotamia" category (a randomly selected hard category).

Results and Discussion

Likelihood judgments. Table 4 displays the average likelihood estimates for hard and easy categories from participants in both the self-target and other-target conditions. It should be noted that likelihood values from the other-target condition reflect participants' assessments of their competitor's chances of winning.

The results from the self-target condition replicate those from Study 3. Participants in that condition were much more optimistic about winning the easy categories than the hard categories, $F(1, 158) = 184.66$, $p < .001$. The magnitude of this effect—expressed as the difference between the average likelihood estimate for easy and difficult categories—was 30.2%.

Recall that the data pattern from the other-target condition is critical for determining whether egocentrism, focalism, or both underlie the shared-circumstance effect. Consistent with our predictions for the other-target condition, participants' estimates for *their competitor's* likelihood of winning were significantly lower for the easy categories than the hard ones, $F(1, 158) = 16.77$, $p < .001$. The direction of this effect reflects the influence of egocentrism. It appears that, even when a likelihood question asks participants to indicate their competitor's chances of winning, self-knowledge assessments have more impact than other-knowledge assessments. Presumably, for an easy category like "driving rules," participants primarily noted that they themselves know a lot of driving rules, and therefore their competitor would be unlikely to win. For a hard category like "dates in history," they primarily noted that they know few dates in history, and therefore their competitor would be likely to win. Had focalism alone (i.e., no egocentrism) accounted for the shared-circumstance effect, then participants in the other-target condition would have primarily thought about how much their competitor did or did not know about the categories, and they would have therefore indicated that their competitor was more likely to win the easy categories than the hard ones.

Table 4
Mean Likelihood Judgments From Experiment 4 by Category and Question Target

	Self-target		Other-target	
Categories	*M*	*SD*	*M*	*SD*
Hard categories				
19th century French painting	21.01	21.88	60.06	26.82
50s movies	31.93	19.77	56.99	19.15
Ancient civilizations	27.69	21.34	55.63	20.05
Baroque music	17.71	20.99	56.36	27.05
Dates in history	34.65	20.26	54.53	20.88
Eastern philosophy	20.16	21.94	56.33	22.72
European politics	22.84	18.76	57.93	20.95
Famous composers	28.84	20.58	55.53	22.32
Famous rivers	41.63	19.78	50.20	16.36
History of Mesopotamia	19.24	19.75	57.43	24.87
Home insurance facts	21.14	19.71	58.81	23.62
Indigenous vegetation of Amazon	17.59	20.26	57.24	25.21
Latin American history	22.74	18.55	60.49	21.81
Russian literature	17.59	19.19	60.06	25.67
South American geography	26.93	20.53	56.94	19.93
Overall (hard)	**24.78**	**14.46**	**56.98**	**16.49**
Easy categories				
Adam Sandler movies	63.51	23.14	43.51	23.78
Brands of alcohol	52.86	23.76	47.55	21.64
Celebrities	54.20	19.94	51.05	18.24
Clothing fads	53.44	21.89	47.65	23.67
Current events	54.23	18.12	55.77	17.73
Driving rules	62.26	18.01	42.21	19.75
Famous cartoon characters	55.61	19.59	44.71	18.90
Fast food chains	61.21	15.58	46.98	16.60
Pop culture	53.59	21.44	48.45	18.76
Pop music	55.03	20.30	48.66	20.38
Rock 'n' roll	50.25	21.26	47.63	17.48
State capitols	57.04	21.12	45.68	23.72
TV sitcoms	56.93	21.14	47.50	21.47
U.S. geography	48.21	18.64	49.65	17.27
Using a personal computer	46.85	23.51	51.03	21.96
Overall (easy)	**55.01**	**11.22**	**47.87**	**11.06**

Note. Participants in the self-target condition estimated their own chances of winning; participants in the other-target condition estimated their competitor's chances of winning.

Also consistent with our predictions, the absolute magnitude of this shared-circumstance effect in the other-target condition (9.1%) was significantly smaller than that in the self-target condition (30.2%), $t(158) = 6.71$, $p < .001$. This finding supports the idea that focalism mitigated the impact of egocentrism in the other-target condition, but combined with the impact of egocentrism in the self-target condition. In the former condition, the focal outcome and egocentric outcome always differed, whereas in the latter condition they were always the same. Had egocentrism alone (i.e., no focalism) accounted for the shared-circumstance effect, then the direction of the shared-circumstance difference would have reversed in the other-target condition (as it did), but the absolute magnitude of the effect would have closely approximated that in the self-target condition (which it did not). In summary, the analyses of the likelihood data from Experiment 4 support both the egocentrism and focalism accounts for shared-circumstance effects.[2]

Self-report explanations. Participants' explanations for their likelihood responses (for the Rock 'n' Roll and Mesopotamia categories) were scored to create a self-referencing index. Specifically, two independent coders, unaware of experimental conditions, tallied the number of self-references (e.g., "I," "my," "me") and other references (e.g., "he/she," "his/her," "that guy") in the responses. From each coder's tallies for a given participant, we calculated the proportion of self-references relative to all self- and other-references. The correlation among the computed values from the two coders was high ($r = .88$), so we averaged those values to arrive at one "self-referencing" value for each participant.

The mean self-referencing value was significantly greater than 0.50 ($M = 0.85$), $t(150) = 70.08$, $p < .001$, indicating that, not surprisingly, participants made more self-references than other-references. Perhaps more interesting is the finding that self-referencing was higher in the self-target condition ($M = .91$) than the other-target condition ($M = .80$), $t(149) = 4.60$, $p < .001$. From a formal perspective, asking participants about their own chances or their competitor's chances should have no impact on the information considered by the participant. However, consistent with the focalism hypothesis, the balance of self and other considerations was influenced by the target of the question (self vs. other).

We also examined the relation between the self-referencing values and the magnitudes of the overall shared-circumstance effects exhibited by the participants. The correlation was .27 ($p < .05$) within the self-target condition and $-.22$ ($p = .06$) within the other-target condition. These findings are consistent with the notion that egocentrism played a role in the shared-circumstance effect. Specifically, they indicate that people who were relatively high in self-focus—as evidenced by greater use of self-references—showed a relatively strong bias toward expecting to win the easy categories but not the hard ones (or expecting their competitor to lose the easy categories but not the hard ones, which accounts for the negative correlation in the other-target condition).

Knowledge judgments. Participants' ratings of how much they knew about a category were significantly higher for easy ($M = 4.88$, $SD = 0.78$) than for hard categories ($M = 2.09$, $SD = 0.64$), $F(1, 157) = 1{,}895.96$, $p < .001$. Their ratings of how much their competitor knew were also significantly higher for easy ($M = 5.03$, $SD = 0.75$) than for hard categories ($M = 2.50$, $SD = 0.84$), $F(1, 157) = 1{,}018.44$, $p < .001$. These findings are important for ruling out one possible explanation for the shared-circumstance effects. It is not simply the case that participants believed that the hard categories would be hard for themselves but not for others, and that easy categories would be easy for themselves but not for others.

By examining how absolute knowledge judgments for the self and other related to likelihood judgments, we can conduct another test of the egocentrism and focalism accounts. The egocentrism account predicts that ratings of self-knowledge should account for more variance in participants' likelihood judgments than should ratings of competitor-knowledge—regardless of whether the target of the likelihood questions is the self winning or the competitor winning. The focalism account, in contrast, predicts that the relationship between these variables should depend on the target of the likelihood questions.

To test the accounts, we adapted a path-analytic approach that Klar and Giladi (1997) and Kruger (1999) used to assess the relations between various absolute and comparative judgments. Our data set allows us to examine how a given participant's self-knowledge and other-knowledge ratings across the 30 categories relate to his or her likelihood judgments across the 30 categories. Hence, we conducted a separate path analysis for each of our 160 participants. Figures 1A and 1B summarize those analyses. Specifically, Figure 1A displays the average values from the 80 path analyses in the self-target condition, whereas Figure 1B displays the average values from the 80 path analyses in the other-target condition. As expected, the results depicted in Figure 1A suggest that for participants who were asked to indicate their own likelihood of winning, their likelihood responses were primarily a function of how much knowledge they believed they had regarding a category; the assessment of their competitor's knowledge played little or no role. However, for participants who were asked to indicate their competitor's likelihood of winning (see Figure 1B), their likelihood responses were a function of both self-knowledge assessments and other-knowledge assessments. These findings are consistent with our proposal that both egocentrism and focalism are distinct contributors to shared-circumstance effects. In the self-target condition, egocentrism and focalism had the same effect of biasing the respondent toward attending to self-knowledge assessments. In the other-target condition, egocentrism biased the respondent toward attending to self-knowledge assessments, whereas focalism biased the respondent toward other-knowledge assessments, which reduced the asymmetry of the impact that self- and other-assessments had on likelihood judgments.

These findings parallel those reported by Eiser et al. (2001), who used a self–other focus manipulation in a study on people's predictions about comparative outcomes for an exam (see also Hoorens, 1995; Otten & van der Pligt, 1996). For participants who

[2] In addition to the analyses we describe in the main text, we conducted a more typical analysis for the type of design used in Experiment 4. A 2×2 repeated measures ANOVA revealed a significant shared-circumstance main effect, $F(1, 158) = 45.07$, $p < .001$, a significant target main effect, $F(1, 158) = 75.42$, $p < .001$, and a significant interaction, $F(1, 158) = 156.36$, $p < .001$. The results of this ANOVA are entirely consistent with the conclusions we draw from our other analyses, but because our main hypotheses were best tested by the other analyses, we did not organize our results section around the ANOVA results.

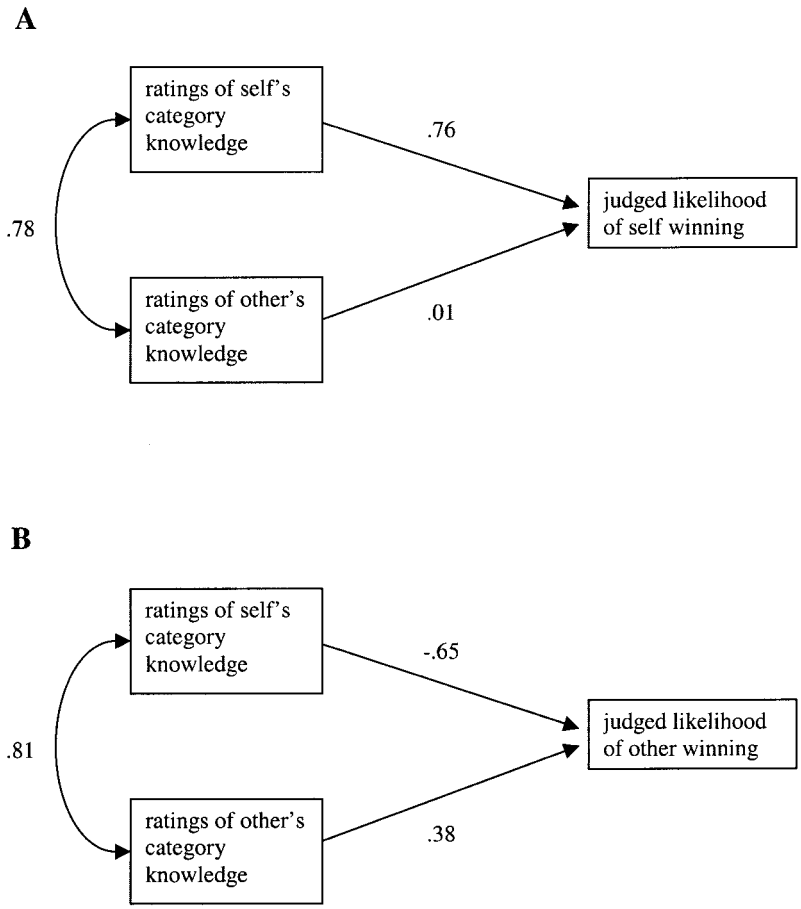

Figure 1. For each individual participant, a path analysis was conducted predicting his or her likelihood judgments from self-knowledge ratings and other-knowledge ratings for the 30 categories. Figure 1A shows the average path-analysis values from participants in the self-target condition (asked to judge the likelihood that they themselves would win). Figure 1B shows the average values from participants in the other-target condition (asked to judge the likelihood that their competitor would win). Average standardized path coefficients appear on the straight arrows. Average correlations appear on curved arrows.

were asked how well they would do on an exam compared with other typical students, their comparative responses were significantly related to absolute judgments about the self but not absolute judgments about other typical students. For participants who were asked how well other typical students would do compared with them, their comparative responses were significantly related to both types of absolute judgments in the expected directions.

Experiment 5—Trivia With a Stranger or Friend

One characteristic shared by all of the preceding experiments is that the competitor(s)—hypothetical students in Experiments 1 and 2 and a fellow research participant in Experiments 3 and 4—were unknown to participants. Although participants in the trivia studies engaged in a short conversation with their competitor prior to the trivia game, the fact remains that participants knew very little about the person with whom they were to compete. This raises the question of whether the shared-circumstance effects observed thus far are moderated by the knowledge people have of their competitor, and even whether the effects are unique to situations in which competitors are strangers. Not only would answers to these questions address a potential moderator and boundary condition of the effect, but also the generalizability of the research. In everyday life, we often compete with individuals we know.

Thus, Experiment 5 investigated the influence that knowing one's competitor has on the shared-circumstance effect. Instead of facing their actual competitor in a trivia contest, participants in Experiment 5 imagined playing the trivia contest. The critical manipulation was that half of the participants imagined playing a stranger whereas half imagined playing someone they knew very well.

Method

Participants and design. The participants were 54 students from elementary psychology classes at the University of Iowa, who participated to fulfill a research exposure component of the course. The design was a 2×2 mixed design in which the category difficulty (hard or easy) was manipulated within subjects and the type of imagined competitor (stranger or friend) was manipulated between subjects.

Procedure and materials. The materials were contained in one questionnaire booklet. When participants read the first page of the booklet—at which point they knew nothing about the specific nature of the questionnaire—they were asked to identify a person. More specifically, participants in the stranger condition were asked to think about a specific person about their age who they had recently seen (e.g., on a bus, in a class) but was essentially a stranger. Participants in the friend condition were asked to think about someone about their age who they knew quite well, such as a high school or college friend. On the second page of the booklet, participants were informed that the person they selected on the first page would be called "Person X" for the remainder of the booklet. Participants then answered three simple questions that were designed to give them practice at thinking of their friend or the stranger whenever they read "Person X."

The third page of the booklet described the same type of trivia game that was described in Experiments 3 and 4. However, participants were told to imagine that they were about to play Person X in the trivia game, and they then estimated the numeric likelihood (0% to 100%) that they would win the various categories. Next, they provided sets of category knowledge ratings (1–7) for themselves and Person X. The order of these sets was counterbalanced between subjects.

Results and Discussion

Likelihood judgments. Table 5 displays the average likelihood estimates. We conducted a 2 (category: hard or easy) × 2 (competitor: stranger or friend) repeated measures ANOVA, with average likelihood estimates for the hard and easy categories as the repeated measure. Not surprisingly, the shared-circumstance main effect was significant, $F(1, 52) = 58.45$, $p < .001$, whereas the stranger/friend main effect was not, $F(1, 52) = 2.23$, $p = .14$. The critical finding was a robust interaction, which reflects that the shared-circumstance effect was stronger in the stranger condition than in the friend condition, $F(1, 52) = 14.22$, $p < .001$. It appears that the magnitude of the shared-circumstance effect does indeed depend on the knowledge of one's competitor. It is important to also note that although the shared-circumstance effect was significantly reduced in the friend condition relative to the stranger condition, the simple effect tests of the shared-circumstance effect were significant within both conditions, $F(1, 52) = 7.24$, $p < .01$, and $F(1, 52) = 67.67$, $p < .001$, respectively. Even when one is

Table 5
Mean Likelihood Judgments From Experiment 5 by Category and Competitor Type

	Stranger		Friend	
Categories	*M*	*SD*	*M*	*SD*
Hard categories				
19th century French painting	24.39	26.02	38.19	25.72
50s movies	27.86	21.87	38.13	27.45
Ancient civilizations	43.00	24.33	56.15	29.06
Baroque music	14.68	20.79	41.23	33.47
Dates in history	37.75	22.95	55.96	30.17
Eastern philosophy	26.36	28.20	47.38	29.83
European politics	31.79	27.85	41.73	29.32
Famous composers	36.11	24.92	45.42	28.66
Famous rivers	49.11	23.61	55.38	24.16
History of Mesopotamia	30.61	25.92	43.65	31.89
Home insurance facts	26.75	23.85	37.65	26.43
Indigenous vegetation of Amazon	22.96	20.07	41.81	25.28
Latin American history	32.14	26.39	45.96	29.97
Russian literature	17.57	19.72	34.31	24.09
South American geography	36.39	30.37	52.19	28.02
Overall (hard)	**30.50**	**15.02**	**45.01**	**18.45**
Easy categories				
Adam Sandler movies	73.07	18.01	48.42	26.82
Brands of alcohol	54.71	25.62	56.77	27.20
Celebrities	54.14	21.90	44.81	28.20
Clothing fads	53.57	32.62	57.88	29.74
Current events	59.93	22.35	53.46	26.71
Driving rules	72.11	16.52	67.69	25.07
Famous cartoon characters	71.39	23.25	54.15	22.28
Fast food chains	60.82	19.74	52.69	18.93
Pop culture	54.86	24.91	55.96	24.37
Pop music	69.46	20.61	54.12	26.61
Rock 'n' roll	63.00	24.34	50.77	25.33
State capitols	68.82	22.28	70.77	22.30
TV sitcoms	66.46	25.31	55.77	26.67
U.S. geography	52.75	22.31	61.35	29.00
Using a personal computer	58.71	30.21	52.19	34.78
Overall (easy)	**62.25**	**9.86**	**55.79**	**12.30**

Note. Participants in the stranger condition estimated their chances of winning against a stranger; those in the friend condition estimated their chances of winning against a friend.

quite familiar with a competitor, judgments about one's chances of winning are significantly more optimistic for the easy categories than the hard ones.

Knowledge judgments. Participants' ratings of how much they knew about a category were significantly higher for easy ($M = 5.10$, $SD = 0.74$) than for hard ($M = 2.45$, $SD = 0.81$) categories, $F(1, 52) = 391.83$, $p < .001$. Their ratings of how much their competitor knew were also significantly higher for easy ($M = 4.83$, $SD = 0.74$) than for hard ($M = 2.41$, $SD = 0.83$) categories, $F(1, 52) = 334.79$, $p < .001$. Knowledge ratings regarding competitors did not significantly differ as a function of the type of competitor ($F < 1$), and the Competitor × Category interaction was not significant, $F(1, 52) = 2.40$, $p > .10$.

As was the case in Experiment 4, the main question of interest regarding the knowledge ratings was how they related to likelihood estimates. Again, our data set allows us to examine how a given participant's self-knowledge and other-knowledge ratings across the 30 categories relate to his or her likelihood judgments across the 30 categories. Hence, we conducted a separate path analysis for each of our 54 participants. Figure 2A displays the average values from the 28 path analyses in the stranger condition, and Figure 2B displays the average values from the 26 path analyses in the friend condition. As expected, the results depicted in Figure 2A suggest that for participants in the stranger condition, their likelihood responses were primarily a function of how much knowledge they believed they had regarding a category; their assessment of their competitors' knowledge played a far lesser role. However, for participants in the friend condition (see Figure 2B), their likelihood responses were a function of both self- and other-knowledge assessments.

Experiment 6—Poker With and Without Wild Cards

Experiments 1–5 provide compelling demonstrations of how shared circumstances influence self-reported optimism about winning in competitions involving grades and trivia challenges. Although we presume that these effects are strong enough to extend to actual behavior in real-time competitions, Experiments 1–5

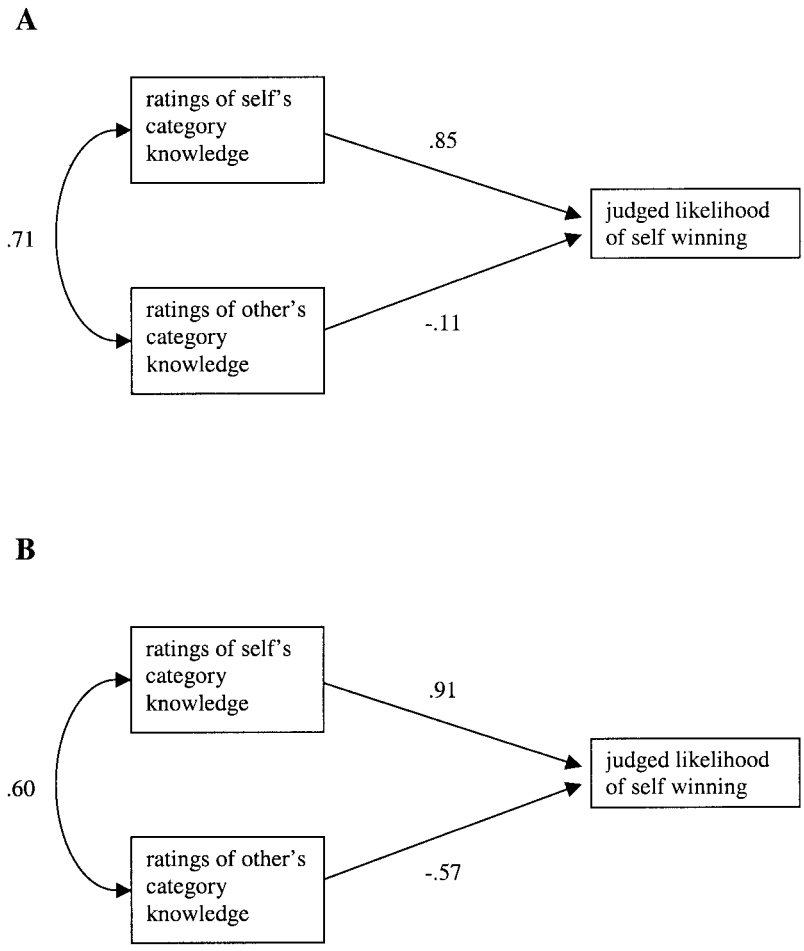

Figure 2. For each individual participant, a path analysis was conducted predicting his or her likelihood judgments from self-knowledge ratings and other-knowledge ratings for the 30 categories. Figure 2A shows the average path-analysis values from participants who imagined playing a stranger. Figure 2B shows the average values from participants who imagined playing a friend. Average standardized path coefficients appear on the straight arrows. Average correlations appear on curved arrows.

provide no direct evidence of this. In those experiments, participants provided private likelihood judgments or predictions on questionnaires. Furthermore, the competitions in Experiments 1, 2, and 5 were imagined. Although the competitions in Experiments 3 and 4 were real, participants were unaware of which categories would be selected for the competition. Hence, a skeptic might wonder whether shared-circumstances would really have much influence on the behavior of people in the midst of a competition, when the salience of their competitors is naturally quite high. Experiment 6 addressed this concern by testing whether shared circumstances would influence actual betting behavior in the midst of a common competitive task—poker.

In small groups, participants played each other in slightly modified rounds of poker, some involving wild cards. Holding a wild card in poker is a good thing; it allows a player to treat that card as a substitute for any card the player wishes, thereby improving the strength of the player's hand (set of cards). Participants learned prior to each round whether wild cards would be active or inactive. They then placed bets (i.e., chips) before seeing their cards and also during the round itself. Although any player might be lucky enough to receive a wild card, we suspected that participants would tend to focus on the ways in which a wild card could help them and would neglect to adequately consider the ways in which a wild card could help their competitors. As a consequence, we expected players to bet more in the rounds in which the wild cards were active rather than inactive.

Method

Participants. The participants were 97 University of Illinois at Urbana–Champaign students, recruited in groups of 15–17 that were split into small groups for playing poker. They earned course credit in an upper division research methods course for participating.

Procedure. Upon arrival in the lab, participants were told that they would be playing a game involving several hands of five-card-draw poker. To ensure that participants were familiar with the rules of the game, the experimenter led participants through a brief (15 min) tutorial.

In the tutorial, the experimenter explained that each player would start with 50 chips, and that the object of the game was to finish with as many chips as possible. The game would involve several "hands." At the start of each hand, each player would contribute a single chip, or "ante," to the "pot." Then, participants would be dealt 5 cards face down from a 52-card deck. Before looking at the cards, the player to the dealer's immediate left would be given an opportunity to bet. Specifically, the player would have three options: to bet (contribute 1–5 chips to the pot), pass (defer the opportunity to bet to the next player), or fold (quit the hand). If the player chose to pass or fold, the next player would be given the same three options. This would repeat until a player placed a bet, at which point the options available to the other players would change: to call (match the previous bet by contributing the same number of chips to the pot), fold, or raise (place a bet in excess of the previous bet). For a single betting period, a total of two raises would be allowed, and no single bet could exceed five chips. Once the first round of betting was complete, each player would then look at his or her own cards, and a second round of betting would commence with the same rules as the first. Then each player would be given the opportunity to exchange 0–4 cards for new ones in the hopes of improving their hand. Once complete, the third and final round of betting would begin. Finally, each remaining player would reveal his or her hand, and the player with the best hand would win the pot. The experimenter then explained with several examples the rank strength of every type of hand in poker, from a "pair" to a "royal flush."

After the tutorial, the groups of 15–17 were split into several smaller groups of 3–5 people each (median = 4), and a dealer was solicited from each group. The group then played two practice hands, the first without betting and the second with betting, while the experimenter answered questions as needed.

When the practice hands were complete, the actual game began. Participants played a total of four hands lasting approximately 30 min, although the precise number of hands involved was unknown to participants. The key independent variable was whether wild cards were active or inactive on a given hand. For half of the hands (the even hands for half of participants and the odd hands for the other half), *3*s, *9*s, and "one-eyed jacks" were wild.

Results and Discussion

Our first analysis focused on overall betting (i.e., collapsed across the three betting rounds for a given hand). For each player, we averaged the amount bet during the odd hands, and we averaged the amount bet during even hands. For half the participants, the odd hands were the wild-card hands, whereas for the other half the opposite was true. Thus, we performed a 2 (odd hands vs. even hands) $\times$ 2 (wild cards-in-odd-hands vs. wild cards-in-even-hands) repeated measures ANOVA with the first factor as the within-subject variable. Table 6 displays the relevant means for this analysis. A significant odd–even main effect revealed that participants bet more on even than odd hands, $F(1, 95) = 35.16$, $p < .001$. Results for the other main effect indicate that bets from participants for whom the even hands were wild and bets from participants for whom odd hands were wild were not significantly different, $F(1, 95) < 1$. Of central importance, this analysis also yielded a robust interaction, $F(1, 95) = 29.24$, $p < .001$.[3] As Table 6 reveals, the amount bet by participants depended considerably on the status of the wild cards. For odd hands, participants bet 23% more chips when the wild cards were active rather than inactive, $F(1, 96) = 5.79$, $p < .05$; for even hands, they bet 29% more chips when the wild cards were active, $F(1, 96) = 7.56$, $p < .01$.

Supplemental analyses revealed that this pattern held for each of the three betting rounds—before players saw their cards, after they looked at their cards, and after they exchanged cards. Separate 2 (odd hands vs. even hands) $\times$ 2 (wild cards-in-odd-hands vs. wild cards-in-even-hands) ANOVAs revealed significant interactions for the first round of betting, $F(1, 95) = 18.11$, $p < .001$, the second round of betting, $F(1, 95) = 23.77$, $p < .001$, and the third, $F(1, 95) = 5.58$, $p < .05$. Figure 3 illustrates the wild card effects for each of the three betting rounds. The significant effect on the betting within the first round is particularly noteworthy. During that betting round, participants were unaware of whether they would be lucky enough to receive a wild card in the upcoming hand. During the second and third rounds, participants who received cards that were in fact wild may have miscalculated the unique benefits of holding wild cards, because they underestimated the odds of a good hand when 10 out of 52 of the cards are wild.

[3] The effect size for this and all subsequent analyses were similar when the data were analyzed at the level of the group ($f = .56$, $n = 25$) rather than at the level of the individual ($f = .55$, $n = 97$). For this reason, we elected to report the data with the more familiar individual as the unit of analysis than with the group as the unit of analysis despite some interdependence inherent in the individual-level analyses.

Table 6
Means for the Number of Chips Bet During Poker Hands in Experiment 6, Collapsed Across the Three Betting Rounds

	Wild-card activation					
	In odd hands		In even hands		Overall	
Hand	*M*	*SD*	*M*	*SD*	*M*	*SD*
Odd	3.15	1.16	2.57	1.21	2.87	1.21
Even	3.22	1.64	4.16	1.73	3.68	1.74
Overall	3.19	1.25	3.37	1.31	3.27	1.28

For instance, a player holding *three of a kind*, may have not realized just how unremarkable that hand is when nearly 20% of the cards are wild. Although we suspect that such mistaken estimations may have influenced some betting in the second and third rounds, the fact that wild cards enhanced the betting that occurred even before players saw their hand suggests that the key reason for the wild-card effect was that participants were more focused on how a wild card might help them than on how a wild card might help their competitors. A second reason to doubt the role of the misestimation interpretation is the fact that betting behavior was unrelated to self-professed poker experience, which was assessed at the end of the experiment on a scale ranging from 0 (*none*) to 10 (*a lot*). All correlations were below .10 and nonsignificant.

General Discussion

This research produced robust evidence consistent with our primary prediction: The average optimism of a set of competitors increases in the face of a shared benefit and decreases in the face of a shared adversity. This was true regardless of whether the competition was real (Experiments 3, 4, 6) or imagined (Experiments 1, 2, 5), whether a participant faced a single competitor (Experiments 3–5) or multiple competitors (Experiments 1, 2, and 6), and whether optimism was assessed through self-report measures (Experiments 1–5) or behavior (Experiment 6). It was also true—but to a lesser extent—when the participant had a great deal of knowledge about their competitor (Experiment 5) and even when the shared nature of a shared circumstance was relatively transparent (Experiment 2).

The shared-circumstance effects detected in our experiments reflect patterns of responses that, from an objective observer's vantage point, are clearly problematic. For example, the set of responses to one of the shared benefits in Experiment 2 suggests that if a professor announced to his or her class that an exam would be extra challenging, about 3 of every 4 students would expect that exam to lower their chances of ranking in the top half of the class. In Experiment 3, although participants knew that each category would have 1 winner—which requires that the likelihood estimates for the 2 participants in a pair should average 50%—the average estimate for the easy category "fast food chains" was 70%, whereas the average estimate for the hard category "home insurance facts" was 7%. Although it is impossible to determine which individual data points in our experiments reflect mistaken beliefs, group-level analyses clearly indicate that systematic biases introduced error at the level of individual responses.

One way of describing shared-circumstance effects is to say that people's optimism about winning tends to be sensitive to absolute assessments (Will a hard exam hurt my exam score? Do I know much about home insurance facts?) even though their optimism

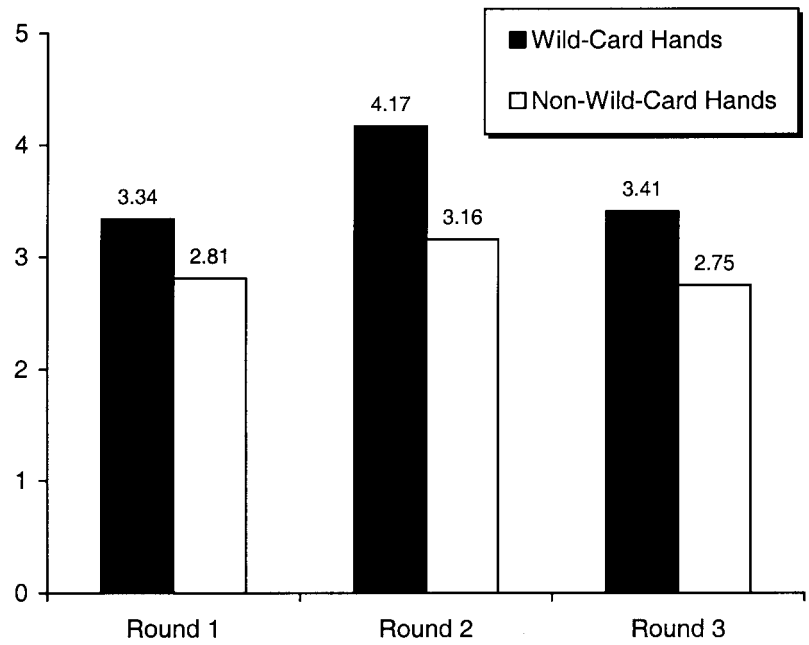

Figure 3. Mean number of chips bet in Rounds 1 (before players saw their cards), 2 (after they looked at their cards), and 3 (after they exchanged cards) as a function of whether wild cards were active or inactive in a given hand.

should be determined by relative assessments (Will a hard exam hurt my exam score more than those of other students? Do I know more about home insurance facts than does my competitor?). Indeed, people might often have difficulty separating their responses to the absolute or intrapersonal standing of some performance (e.g., their performance relative to a personal goal or general standard) from their responses to the comparative or interpersonal standing of a performance (their performance as compared with the performance of other competitors). This might be true because the absolute and comparative standings of performances in everyday life are generally correlated. For example, a student who scores 15 points higher than his or her usual exam performance will probably also rank higher than he or she typically does relative to other students.

Whereas the general correspondence between the absolute and comparative standing of performances might suggest a distal cause of shared-circumstance effects, a main goal of the present article was to identify some of the more proximal causes and to identify when they are most influential. We focused on two possible explanations for the shared-circumstance effects: egocentrism (the tendency to base optimism on self-relevant assessments more so than other-relevant assessments) and focalism (the tendency to overweight assessments relevant to the focal rather than complementary outcome). There is evidence that both mechanisms contribute to the effects. In Experiment 4, we varied whether trivia game contestants predicted their own chances of winning or their competitor's chances. The pattern of results for the likelihood judgments suggested that egocentrism and focalism combined to produce strong shared-circumstance effects in the self-target condition, whereas in the other-target condition, focalism partially mitigated the impact of egocentrism to produce a weaker but still reliable shared-circumstance effect. Path analyses provided additional evidence that when participants judged their own likelihood of winning, they based those judgments on their assessments of their own knowledge of the trivia categories more than their assessments of their competitor's knowledge. However, as illustrated by Figures 1A and 1B, this asymmetry was greatly reduced for participants who were judging their competitor's likelihood of winning, a finding consistent with the focalism account.

Additional evidence for focalism comes from related research by Moore and Kim (2002), who conducted a study in which participants took either a very difficult or simple 10-item quiz. Some participants then placed a bet on the possibility that their score was better than a randomly selected person's score. Other participants placed a bet that a score from one randomly selected person was better than that from another randomly selected person. Whether participants were betting on their own score or the score of a randomly selected person, they bet more in the simple-quiz condition than the difficult-quiz condition. Whereas egocentrism or focalism could explain the simple/difficult effect when respondents bet on themselves, only focalism could explain the simple/difficult effect when respondents bet on a randomly selected individual.

We suspect that egocentrism and focalism are not the only mechanisms that produce shared-circumstance effects. We described a multiple-competitor account that applies to situations in which a person faces two or more competitors. As described earlier, that account assumes that assessments of how a shared circumstance would influence a set of competitors are made with less accuracy, ease, and/or confidence than are the assessments about only one individual (e.g., the self), causing the former assessments to have less impact than the latter on optimism judgments. We ruled out the multiple-competitor account as an explanation for the shared-circumstance effects in Experiments 3–5 (because participants faced only one competitor), but this does not discredit the multiple-competitor account as a possible explanation for some shared-circumstance effects. In fact, findings related to a general theory of subjective probability, called *support theory* (Tversky & Koehler, 1994), provide grounds to assume that facing multiple competitors instead of just one can contribute to shared-circumstance effects (e.g., Koehler et al., 1997; Teigen, 1974, 1983; Tversky & Fox, 1995; Van Wallendael & Hastie, 1990; Wallsten, Budescu, & Zwick, 1993; Windschitl, 2000; Wright & Walley, 1983).[4]

Relation to Support Theory

A central claim of support theory is that the judged probability, $P(A, \bar{A})$, that a focal hypothesis holds rather than any of its alternatives, which are collectively referred to as the residual hypothesis, depends on support or perceived evidence for the focal hypothesis, $s(A)$, and the residual hypothesis, $s(\bar{A})$:

$$P(A, \bar{A}) = \frac{s(A)}{s(A) + s(\bar{A})}.$$

The theory also suggests that support for a hypothesis is more effectively assessed when that hypothesis is explicitly stated in a focal position than when it is implicitly part of a multihypothesis residual. For example, evidence regarding the strengths of an Italian soccer team may be more salient and accessible when people are asked explicitly about the Italian team's probability of winning a tournament (involving Italy, France, Germany, and Spain) than when asked about France's chances of winning the tournament. With these assumptions and a linear discounting assumption described by Koehler et al. (1997), support theory predicts that, when there is a multihypothesis residual, simultaneously adding supportive evidence to all the hypotheses can lead to an inflation of the judged probability of the focal hypothesis. This is analogous to adding a shared benefit to a competition that has more than two competitors; the impact of the added benefit on the focal competitor is more effectively evaluated than is the impact of the added benefit on the nonfocal competitors considered as a group (the residual). Hence, the shared benefit will tend to increase the judged likelihood of the focal competitor winning.

Although support theory can account for shared-circumstance effects in multiple-competitor cases, it does not—as originally proposed—account for shared-circumstance effects when a person faces only one competitor, such as in Experiments 3–5. For binary cases such as these, the theory is symmetric in the sense that it would predict that assessments of self-knowledge and other-knowledge should have equal weight in determining likelihood judgments. The egocentrism and focalism accounts require asymmetry in the impact of the two inputs. Other researchers have noted

[4] See Brenner, Koehler, and Rottenstreich (2002) for a pertinent discussion of potential connections between support theory and comparative judgment effects.

the need for a likelihood judgment theory to include this asymmetry to account for focalism, and some have proposed such modifications to support theory (see Brenner & Rottenstreich, 1999; Fox & Levav, 2000; Idson et al., 2001; Macchi et al., 1999; Windschitl, 2000; Yamagishi, 2002).

It is also worth noting that whereas asymmetries in weighting for binary cases have traditionally been small or hard to find, the asymmetry suggested by the results of the trivia-game experiments was quite large. For example, for the hard categories in Experiment 4, participants reported, on average, that they had a 24.77% chance of winning and that their competitor had a 56.98% chance of winning. These numbers sum to only 81.75% instead of 100%, indicating quite strong nonadditivity and suggesting that the balance of the weighting of self and other evaluations changed as function of the question-target manipulation. Overall, the present results provide some of the most robust evidence to date that information about a focal hypothesis has greater weight in determining probability judgments than does information about an alternative hypothesis, even for binary cases.

Relation to Social Comparison

Our research also extends recent findings from the social comparison and self-assessment literature. As mentioned earlier, a number of researchers have found that when making comparative judgments of traits, abilities, or other attributes, individuals often give little or no consideration to the reference group (e.g., Eiser et al., 2001; Klar & Giladi, 1999; Kruger, 1999; Price et al., 2002; Weinstein, 1980; Weinstein & Lachendro, 1982). As a consequence, the answer to the question "How do I compare with my peers" is based considerably more on "I" than "my peers."

The present research extends these findings in two ways. First, the present research confirms that the differential impact of self- and other-evaluations is not simply a unique feature of responses on explicitly comparative questions. Instead of asking participants about comparative judgments, we asked about outcomes, which ultimately depended on comparative strength in the competitions. When participants were asked to estimate course grades, make numeric likelihood estimates about trivia outcomes, or place bets on poker games, their responses reflected the same type of self–other bias as have responses to explicitly comparative questions.

A second extension to this literature provided by the present work concerns the mechanisms thought to underlie these previously demonstrated effects. Prior researchers have emphasized two types of accounts of why, for example, drivers base their judgments of their comparative driving ability on their own skills more than the driving skills of the comparison group. One type, which shares commonalities with the focalism and multiple competitor accounts, suggests that the self–other bias is a product of a more general processing bias in which absolute assessments of any individualized target (e.g., the self or another specific person) will have more impact than absolute assessments of a more generalized or abstract referent (e.g., "your peers," "the average student"; see Klar and Giladi, 1997, 1999). Another account, the egocentrism account, suggests that people have a stable tendency to overweight self-relevant assessments relative to other-relevant assessments (see Kruger, 1999). If the present research is any indication, we suspect that both types of mechanisms underlie previously documented effects involving comparative judgment (e.g., the "above-average effect"), although further work is necessary to fully evaluate the veracity of this contention.

Remaining Issues

Although we have revealed a number of important findings regarding shared-circumstance effects, these findings also raised a number of additional questions that are worthy of further investigation. For instance, one issue that requires more investigation is whether the influence of shared benefits and adversities is symmetrical. In Experiment 1, although there was robust evidence that a shared adversity could reduce optimism relative to the levels found in a control condition, no evidence was found that a shared benefit would increase optimism. However, in Experiment 2, participants tended to indicate that their chances of scoring in the top half of the class would be helped by the shared benefits that were tested. Also, the findings of Experiment 6 suggest that the introduction of a shared benefit (wild cards) can boost optimism. Furthermore, although there was no control condition in Experiment 3, the fact that probability estimates for the hard categories fell significantly below 50% and the fact that estimates for the easy categories were significantly above 50% suggests that shared adversities can make people more pessimistic than they should be and that shared benefits can make people more optimistic than they should be. Therefore, it is clear from our findings that shared-circumstance effects can operate in both directions, but additional studies would be necessary to determine whether the severity of the effect tends to be systematically stronger in one direction than in another direction.

Another issue that should be further explored concerns possible differences in how individual participants react to shared circumstances. Our experiments have primarily studied shared-circumstance effects at a group level. We can conclude from our experiments that participants, on average, tend to become less (more) optimistic when a shared adversity (benefit) is introduced. There are also various findings from our studies that provide information about how the full distribution of participants (not just the "average" participant) reacted to shared circumstances. For example, in Experiment 2, only 2 of 89 participants reported that their chances of scoring in the top half of the class would be helped if the instructor used a graduate-level exam (whereas 70 reported that the graduate exam would hurt their chances). In Experiment 3, 50 out of 50 participants reported greater optimism about winning the easy categories than the hard ones. Hence, although there are types of students who would actually be more likely to outperform their competitors when answering questions from harder rather than easier exams and trivia categories, almost no one provided likelihood judgments that suggested they appreciated this fact—at least in our experiments. Surely, however, there are instances in which people in real-world competitions realize that even though an adversity will hurt their own performance, they nevertheless become more optimistic about winning because they realize the adversity will hurt their competitors' performance more profoundly. For example, an experienced bicycle racer who has a relatively small physique would likely welcome a headwind—knowing that the headwind hurts the performances of taller and less streamlined racers more than his or her own performance. Future research should seek to understand why (and how) some people develop a strategically advantageous awareness of how

shared-circumstances influence relative performance whereas other people sometimes do not.

There are also several task or circumstance variables that should be tested as potential moderators of shared-circumstance effects. One such variable might be the extent to which a given circumstance has a clear and easily interpreted impact on the performance of the people in a competition. It is possible that a shared-circumstance effect might be largest when an introduced circumstance—say a shared adversity—has several possible interpretations for how it could influence performances of people in the competition. When the consequences of an adversity are unambiguous and easy to spot for both the self and for one's competitors, biases due to egocentrism, focalism, and multiple-competitor mechanisms may have less impact (for a related idea regarding the above-average effect, see Dunning, Meyerowitz, & Holzberg, 1989).

A second task variable that could be a potential moderator concerns the nature of the competition. Consider two extremes regarding different forms of competition. In competitions at one extreme, all competitors engage in somewhat independent performances, and the person who has the best performance wins (e.g., 100-m dash, essay competitions, Academy Awards, trivia games). In competitions at the other extreme, the performances of the various sides of a competition are less independent; a strong performance by one side of the competition must come at the expense of the other side's performance (e.g., soccer, wrestling, tennis, war, civil cases). The competitions we studied in our experiments fall closer to the former extreme than to the latter. It is an open question whether participants in competitions at the latter extreme tend to have a heightened awareness that any circumstance affecting their performance would also likely affect their competitor's performance.

A third potentially important moderator is related to the timing and salience of the introduction of a shared circumstance. We suspect that the impact of a novel shared circumstance would substantially decline over time. When a shared adversity, for example, is first introduced, that adversity would be more salient and consume more attention than it would a few minutes, hours, or days later (see Wilson et al., 2000). Additional time would also give a person a better chance to ponder how a novel shared adversity might impact their competitor's performance. This is important if one assumes that pondering how a shared circumstance might affect the self has temporal priority over pondering how it would affect a competitor.

Finally, in addition to testing various moderators of the shared-circumstance effects, future research should be designed to further differentiate various subtypes of egocentrism, focalism, and multiple-competitor accounts. Consider, for example, the egocentrism "account." There are many potentially distinct reasons why people have a tendency to base optimism on self-relevant assessments more so than other-relevant assessments. One reason might be that self-relevant information garners more attention (or earlier attention) as compared with other-relevant information, thus giving it more weight in optimism judgments. However, another reason could be that people have more reliable information about how shared circumstances affect the self than how they affect others, so assessments of the latter type are given less weight in the judgment process. For example, in the trivia paradigm, perhaps participants made assessments about the self's knowledge of a category with greater confidence than assessments about their competitor's knowledge, which influenced the extent to which these assessments shaped judgments about the likelihood of winning. This possibility suggests a potential rational basis for some—but not all—shared-circumstance effects.[5] Given the various nontrivial differences between accounts *within* the egocentrism, focalism, and multiple-competitor categories, there is much to be gained from research that further teases these accounts apart.

What Is the Harm in a Little Egocentrism and Focalism?

Our primary dependent variable, likelihood judgment, is a key component in many theories within social and cognitive psychology, including an entire class of expected-utility style models. Hence, it is reasonable to assume that the misguided optimism or pessimism that can result from the processing biases studied here can mediate a variety of consequential decisions and behaviors. Examples of types of decisions and behaviors that could be affected include: whether or not a person decides to engage in a competition, the amount of effort and resources a person invests in an outcome, the strategy used to achieve the outcome, and a person's anxiety about and actual performance in a competition. The biases may also affect people's willingness to accept rule changes and other shared circumstances in competitions; because competitors are more sensitive to how a rule change would affect their own performance than their competitor's, they may be relatively reluctant to embrace a proposed rule change that tends to lower or restrict the performance of all sides in competition (e.g., strengthen campaign finance reform; moving the 3-point line farther from the hoop in college basketball), but happy to embrace a rule change that tends to improve the performance of all sides in competition (e.g., allowing speed-enhancing, full-body suits at swim meets). In conclusion, given the robust nature of our findings, and the key mediational role of optimism in decision making and behavior, and the fact that competitions abound in everyday life, the biases we have identified in this article likely play a substantial role in behavior within many everyday contexts.

[5] This rational explanation might apply to some shared-circumstance effects caused by egocentrism. However, it cannot explain why many participants in Experiment 2 reported that, if an instructor added points to the scores of all students in a course, this would help their own chances of scoring in the top half of the class. In addition, there is no rational basis for the focalism mechanisms that contributed to the shared-circumstance effects in Experiment 4.

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