A Robust Negative Relationship Between Self-Reports of Social Skills and Performance Measures of Social Intelligence

COLLECTION: INDUCTIVE REASONING IN SOCIAL CONTEXTS

RESEARCH

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ABSTRACT

Does claiming to be more socially skilled than average reflect above average social intelligence? People may define social skill subjectively or idiosyncratically, and feedback about one's own level of social intelligence or specific social skills may be inconsistent or biased. This suggests that people's self-evaluations of their own social skills may fail to predict—or even negatively predict—their performance on skill-based tests of social intelligence. In one exploratory study and one preregistered replication study (total N = 927), people who reported that they have better social skills than average performed worse on three skill-based tests of social intelligence than people who did not claim to be above average (d_{Study1} = 0.46, d_{Study2} = 0.58). This pattern did not emerge when considering participants' comparative judgments of their own general intelligence and performance on two tests of cognitive ability. We discuss the complexities of accurately assessing one's own social skills, the obstacles associated with measuring the construct of social intelligence using self-reports, and the strategic implications of claiming to be more socially skilled than average.

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Consider asking an acquaintance if they have better social skills than average. What would you conclude about the social intelligence of someone who responds that they do? Past research on self-knowledge accuracy suggests that the most likely outcome of this exercise is that this person's claim is accurate—that they are genuinely more socially intelligent than average. This is because self-evaluations of intelligence tend to be accurate, with meta-analytic estimates estimating a correlation of r = .28 to .37 between self-estimates of general intelligence and objective performance on ability tests of intelligence (Freund & Kasten, 2012).[1] A broader synthesis of effect sizes from 22 meta-analyses across a variety of domains finds a small but consistent positive relationship between estimated and actual ability, including cognitive ability (r = .29; Zell & Krizan, 2014). This research confirms that people tend to have insight into their own cognitive skills and abilities, but does this pattern hold for *social* skills? In this report, we investigate the differences between perceived social skills and social intelligence measured using skill-based tests—and the errors in judgement that may result. We focus on a novel question: whether people who report they have above average social skills perform above average on skill-based measures of social intelligence.

BACKGROUND AND ASSUMPTIONS

Before describing the logic underlying our research question, we provide an overview of our key terms and assumptions. Most importantly, we assume a connection between specific skills or abilities (i.e., the ability to infer emotions from faces as a social skill; the ability to solve logic puzzles as a cognitive skill) and the constructs of social and general intelligence. This assumption is generally accepted in the domain of general intelligence, and we refer to general intelligence as measured using cognitive skills and abilities. However, this link is not as well-established in the domains of social and emotional intelligence (Lievens & Chan, 2010). Whereas social intelligence consists of broad abilities, for example, accurately perceiving and understanding the verbal or nonverbal behaviors of others (Lievens & Chan, 2010), emotional intelligence is more narrowly defined as the ability to perceive, understand, and manage emotions in the self and others (MacCann et al., 2020). However, some abilities fit the requirements of both social and emotional intelligence (e.g., perceiving the emotional state of others; Speer, Christiansen, & Laginess, 2019) and these skills are also theorized to be part of a broader set of related, person-centered intelligences (Bryan & Mayer, 2021). These distinctions between construct labels reflect the broader problem in differentiating socioemotional abilities in psychological research (Olderbak & Wilhelm, 2020). Acknowledging that specific and accepted definitions of social and emotional intelligences are elusive, we consider research involving emotional intelligence as relevant to our focus on social intelligence in the present study.

Throughout this paper, we refer to social skills and social ability interchangeably. We refer to social and emotional intelligences as related but distinct constructs, with the focus of this work on the former. Finally, we refer to general intelligence as comprising cognitive skills that can be measured using ability-based tests.

HYPOTHESIS AND LOGIC

We hypothesize that broad comparative self-judgments of one's own social skills fail to reliably predict—or even negatively predict—social intelligence as measured using specific ability-based tasks. Why might claims about one's own social skills depart from skill-based measures of social intelligence, given that claims about cognitive abilities tend to be accurate (Freund & Kasten 2012; Zell & Krizan, 2014)? The meta-analytic effect sizes reported in past work suggest that self-evaluations vary in accuracy and may be affected by factors not directly related to one's actual ability. Environmental and individual difference factors may degrade the accuracy of individuals' perceptions of their own abilities (Howard and Cogswell, 2018): compared with general intelligence, self-estimates of other forms of cognitive ability tend to be less strongly related to performance on objective tests (Freund & Kasten, 2012). Unlike perceptions of numerical or verbal ability, which are informed by past academic performance or standardized test scores, there are relatively few instances where individuals receive valid feedback on their level of social intelligence or specific social skills, and even fewer instances of how they compare with others. Desirable skills and traits can be difficult for people to observe accurately in themselves or in others and these observations are often subject to self-enhancing biases (Alicke, 1985; Brown, 2012; Tappin & McKay, 2017; Watson et al., 2000), including in applied contexts like health (Miller et al., 2019). When viewing the process of evaluating oneself on socially desirable dimensions through a game theoretic lens, there is often little reason *not* to claim superiority (Heck & Krueger, 2016; Krueger, Heck, Evans, & DiDonato, 2020). Consistent with this reasoning, several studies have reported little to no correlation between self-evaluations and performance on social and emotional tasks in clinical (Jones et al., 2020; Silberstein et al., 2018) and non-clinical populations (Brannick et al., 2009; Joseph & Neuman, 2010; Mayer et al., 2020).

Another reason why self-reports of social skills may diverge from reality is that social intelligence is socially desirable—a characteristic that can propagate overconfidence or the self-enhancing belief that one is above average (Alicke, 1985; Brown, 2012). Research on overconfidence has rarely engaged with the topic of social intelligence, in part due to the measurement challenges described above, yet some studies have grappled with similar questions in the related domain of emotional intelligence. Brackett et al. (2006) observed overconfidence among participants who scored in the bottom two quartiles but placed themselves above the median on an objective emotional intelligence test—defined by these researchers as an ability to manage one's emotions. Sheldon et al. (2014) observed that participants who scored lowest on an emotional intelligence test were both the least aware of their own performance (i.e., Kruger & Dunning, 1999) and expressed the most doubt about the test's accuracy. A similar pattern emerged where participants overestimated their ability to perceive faces (Zhou & Jenkins, 2020). These examples suggest that overconfident beliefs can hinder self-judgment accuracy in domains related to emotional and social intelligence.

The question of accuracy in self-reports of social skills is also relevant in practical settings. Self-evaluations of social intelligence (often measured by asking about specific social skills) are commonly used in clinical, organizational, and educational contexts. These measures typically ask people to rate their own ability on specific social skills, using these skills as a proxy for social intelligence. They include the ability to effectively navigate social situations (e.g., Baron-Cohen et al., 2001a; Riggio, 1986), understand the perspectives of others (e.g., Davis, 1983), or detect other people's intentions and emotions (Silvera, Martinussen, & Dahl, 2001). Self-evaluations on these specific social skills correlate positively with job performance (Joseph & Neuman, 2010) and job satisfaction (Miao et al., 2017) and are used by some organizations for employee selection and development purposes. Clinical and developmental researchers also rely on self-reports to identify and assess autistic traits in clinical and non-clinical populations (English et al., 2021). However, past research has found that individuals can artificially inflate self-evaluations of social intelligence (Christiansen et al., 2010), even when performance feedback suggests that they should not (Dunning & Beauregard, 2000). Several scholars therefore caution against the use of self-report instruments when measuring social intelligence or similar constructs and instead prefer tests of specific social skills (Côté, 2015; Mayer, Caruso, & Salovey, 2016; Murphy & Lilienfeld, 2019). Understanding how self-report measures predict—or fail to predict—performance on skill-based tasks can contribute to the development of social intelligence measures in applied contexts.

APPROACH

In this report, we focus on self-ratings of social skills in comparison to the average person. In the framework of Moore and Healy (2008), such "overplacement" is defined as a positive difference between a person's self-rating and their rating of the average person. We measure calibration or accuracy as the correlation between self-judgments and performance across participants in a population (Krueger & Wright, 2011; Moore & Healy, 2008). Across the many frameworks for studying overconfidence, the better-than-average effect, and self-enhancement, there is consensus that it is productive to incorporate calibration or accuracy into how these constructs are measured (Heck & Krueger, 2015; Krueger & Wright, 2011; Krueger et al., 2017; Kwan et al., 2004).

We sought to study overplacement and calibration in social intelligence, a socially desirable domain where (1) people may claim to be above average and (2) an objective measure can support or refute this claim at the individual level. In sum, we asked whether people's claims to be more (or less) socially skilled than average are accurate. In an exploratory first study, we observed a negative relationship between claims to have above average social skills and performance on a social intelligence task. We pursued this finding in a preregistered, confirmatory second study with a positive control.

TRANSPARENCY STATEMENT

We confirm that study sample sizes and stopping criteria were determined in advance, and that no data were analyzed before data collection was completed for each study. We describe sample size justifications, data exclusions, and exclusion criteria in the Method section for each study.

Study 1 was not preregistered and was designed to answer an unrelated research question. Some data from Study 1 have been reported elsewhere (Brown et al., 2019; see https://osf.io/sqxy6 for survey exports and materials), but the measure of relative social skills and its relationships with key constructs we report here have not been reported in any previous publication. Exploratory analyses on the dataset from Study 1 revealed the result we focus on in this paper. Acknowledging that this result was found using strictly exploratory analyses, we conducted Study 2 to replicate and extend this finding, establishing its robustness using a positive control. This approach to Study 2 was preregistered; all deviations from this preregistration are labeled as "exploratory analyses."

Data and analysis scripts for Studies 1 and 2, preregistrations and survey exports from Study 2, and supplementary materials are available from https://osf.io/3gtzx.

STUDY 1

METHOD

Participants

We recruited US-based participants using Amazon's Mechanical Turk (MTurk) in 2018. Sample size for this study was determined based on a power analysis conducted for an unrelated research question (see Transparency Statement). With the goal of increasing the racial/ethnic diversity of the sample and thus the generalizability of findings on this dimension (Roberts et al., 2020), a subset of participants who reported a race or ethnicity other than white were recruited in parallel using TurkPrime (Litman et al., 2017). This group represented roughly 20% of the full sample (see Table 1 for demographics). Participants were paid $7.25. We removed 28 participants who did not respond correctly to four short attention check videos where participants were asked to identify a shape that did not appear in the video. This resulted in a final sample of 505 participants who completed the study (median completion time = 50 minutes).

Procedure and Tasks

All tasks were completed in an online Qualtrics survey (all data and analyses for both studies available at https://osf.io/3gtzx). Task order was the same for all participants. We report social intelligence and general intelligence scores as percent correct.

Objective social intelligence

Objective social intelligence comprised three distinct multiple-choice, skill-based assessments which were chosen to represent the breadth of methods and stimuli used to measure social intelligence in the research literature (Olderbak & Wilhelm, 2020).

First, participants completed the 23-item Social Shapes Test (SST; α = .67) which measures social attribution using short videos of animated shapes (Heider & Simmel, 1944; Ratajska, Brown, & Chabris, 2020). Brown et al. (2019) report a full account of the initial development and validity evidence for the SST among typically-developing adult samples and more recent research has demonstrated convergent validity with effective judgment in social situations in the workplace (Brown et al., 2022) along with performance on similar animated shape tasks (Brown et al., 2021). The validity of animated shape tasks has been demonstrated in clinical and developmental research, including in identifying difficulties in social attribution among individuals diagnosed with autism spectrum disorder or schizophrenia (Burger-Caplan et al., 2016; Brown, Heck, & Chabris, 2023; Johannesen et al., 2018; Martinez et al., 2019; Wilson, 2021). The SST therefore shows promise as an effective measure of social intelligence in clinical and non-clinical populations.

Next, participants completed the 36-item Reading the Mind in the Eyes Test (RMET; Baron-Cohen et al., 2001b), a popular measure of social intelligence that is used widely in nonclinical and clinical populations. The RMET asks individuals to identify an emotion or mental state based on the image of a human face (α = .79). Despite some disagreement regarding the underlying construct assessed by the RMET (Higgins et al., 2023), performance on this task is closely related to other emotion perception tasks and has been found to predict performance in collective intelligence tasks conducted in both virtual and in-person groups (Engel et al., 2014). Meta-analytic studies have reported that the RMET detects individual differences in social intelligence due to autism spectrum disorder (Peñuelas-Calvo et al., 2019) or schizophrenia (Chung, Barch, & Strube, 2014). Others have found convergent validity between the RMET and other measures of emotion recognition (Kittel et al., 2021).

We also administered the 19-item short form (α = .59) of the Situational Test of Emotional Understanding (STEU-B; Allen et al., 2014). The STEU-B items ask individuals to identify the correct emotion or mental state based on a brief written scenario. This measure has been widely used to measure one of the six theorized narrow abilities thought to make up the construct of emotional intelligence (Elfenbein & MacCann, 2017). Researchers have observed strong convergent validity for the STEU with other measures of emotional intelligence including the MSCEIT (MacCann & Roberts, 2008) and the Geneva Emotional Competence Test (Schlegel & Mortillaro, 2019). This test also represents a wide range of tasks which use verbal descriptions of social or interpersonal interactions to assess emotional or social intelligence in various settings (e.g., Aldrup et al, 2020; Speer et al., 2019).

We created a unit-weighted social intelligence composite score across all three tasks (α = .85) by calculating the percent correct for each individual task, adding them together, and dividing the sum by the number of tasks.[2]

	STUDY 1	STUDY 2
Sex		
Female	221 (44%)	220 (52%)
Male	284 (56%)	197 (47%)
Prefer not to respond	N/A	4 (1%)
Race/Ethnicity		
White	325 (64%)	216 (51%)
Black	69 (14%)	70 (17%)
Hispanic	50 (10%)	44 (10%)
Asian	49 (10%)	100 (24%)
Other	12 (2%)	21 (5%)
Prefer not to respond	N/A	3 (1%)
Age (years)		
Mean	35.8	31.7
Standard Deviation	10.7	11.0

Table 1 Participant Demographics in Studies 1 and 2.

Note. Participants in Study 2 could select more than one race/ethnicity.

General intelligence

Participants completed two measures of general intelligence. We measured non-verbal matrix reasoning using four items (α = .56) designed to mimic one of the most widely used measures of cognitive ability: Raven's Progressive Matrices (RPM) (Raven et al., 1985). Other researchers have created their own validated versions of these matrix items based on the format of the RPM (e.g., matrix reasoning items in the International Cognitive Ability Resource in Condon & Revelle, 2014; the Sandia Matrices in Harris et al., 2020). In both examples, new items based on the format used in the RPM have shown reliable associations with other established measures of cognitive ability.

Verbal ability was measured using a five-item vocabulary test (items sampled from the Wordsum vocabulary test (α = .64) which has been administered as part of the General Social Survey; Cor et al., 2012; Malhotra, Krosnick, & Haertel, 2007). These items have been used as an indicator of cognitive ability in a variety of other research studies (e.g., Pennycook et al., 2015). A composite of these nine items had acceptable internal consistency (α = .67).

Self-reported social skills

After completing these five tasks, participants were asked to respond to the statement "I have better social skills than the average person" using a five-point response scale ranging from 1 = "*Strongly Disagree*" to 5 = "*Strongly Agree*" (3 = "*Neither Agree nor Disagree*"). This item entails a direct measure of social comparison, or the better-than-average effect (Zell, Strickhouser, Sedikides, & Alicke, 2020). Endorsement of this item would reflect overplacement in the framework of Moore & Healy (2008)—though it does not necessarily indicate the strength or degree of overplacement.[3] We used the term "social skills" because we considered this to be similar to the use of the term "intelligence" as a broad trait in previous studies (Heck, Simons, & Chabris, 2018; Trzesniewski, Donnellan, & Robins, 2008). "Social skills" can be broadly interpreted and is a phrase used in both the academic literature (e.g., Ferris, Witt, & Hochwarter, 2001; Riggio, 1986) and in everyday language. Although we acknowledge that asking participants to evaluate their social skill may not perfectly correspond to how they would evaluate their social intelligence, we accepted this tradeoff to provide a more interpretable and familiar measure to participants.

RESULTS

Table 2 displays all intercorrelations and descriptive statistics. Self-rated social skill was negatively correlated with social intelligence task performance (Pearson's $r(503)_{\text{composite SI}} = -.26$, $p < .001$, 95% CI [–.34, –.18]) (see Figure 1). This suggests that reports of having above average social skills were in fact more likely false than true, but by what margin? We answered this using the decision-theoretic analysis from Heck and Krueger (2015): among participants who explicitly agreed that they have better social skills than average (by choosing "somewhat agree" or "strongly agree;" 167/505), only 37% (62/167) outperformed the median sample score of 58 (46%, or 76/167, outperformed the mean sample score of 70.3). An exploratory analysis revealed that, consistent with Kruger and Dunning (1999), those individuals who were in the lowest quartile of SI performance reported the greatest relative social skills ($M = 3.3$, $SD = 1.0$) relative to quartile 2 ($M = 2.9$, $SD = 1.1$), quartile 3 ($M = 2.9$, $SD = 1.1$), and quartile 4, ($M = 2.5$, $SD = 1.2$) (quartiles obtained across the complete Study 1 sample).[4]

A similar negative correlation was observed between self-ratings of social skill and performance on each task in the social intelligence composite, SST ($r(503) = -.19$, $p < .001$, 95% CI [–.27, –.10]), RMET ($r(503) = -.25$, $p < .001$, 95% CI [–.33, –.16]), and STEU-B ($r(503) = -.20$, $p < .001$, 95% CI [–.28, –.11]), indicating that no single task or skill was driving the overall effect. To estimate an effect size, we performed a one-way ANOVA entering social intelligence task performance as the dependent measure and a binary version of self-rated social skill as the independent variable (participants who overplaced

	M	SD	1	2	3	4	5	6	7	8
1. Self-rated Social Skill	2.9	1.1	—							
2. SST	70.4	14.5	**–.19**	*.67*						
3. RMET	77.5	13.8	**–.25**	**.47**	*.79*					
4. STEU-B	62.9	14.6	**–.20**	**.48**	**.52**	*.59*				
5. SI Composite	70.3	11.6	**–.26**	**.77**	**.88**	**.77**	*.85*			
6. Vocabulary	80.7	24.5	**–.16**	**.35**	**.39**	**.44**	**.48**	*.64*		
7. Matrix Reasoning	54.6	30.7	**–.15**	**.48**	**.38**	**.47**	**.53**	**.34**	*.56*	
8. IQ Composite	69.1	22.4	**–.19**	**.51**	**.47**	**.56**	**.61**	**.82**	**.82**	*.67*

Table 2 Descriptive Statistics and Correlation Matrix for Study 1.

Note. N = 505; All correlations are statistically significant at $p < .05$; Coefficient alpha is reported in italics on the main diagonal.

their social skill, i.e., those who selected "Strongly agree" or "Somewhat agree," versus the rest of the sample). We observed a statistically significant effect of claiming to be have above average social skills on task performance, $F(1,503) = 23.09$, $p < .001$, where overplacers scored 5.17 percentage points lower than participants who selected any other option, $d = 0.46$, 95% CI [0.27, 0.64].

Claiming to have above average social skills was also negatively correlated with general intelligence task performance ($r(503)_{\text{composite IQ}} = -.19$, $p < .001$, 95% CI [–.27, –.10]).[5] We used multiple regression to determine whether the negative relationship between self-ratings and social intelligence could be explained or attenuated by general intelligence (see Table 3). We regressed the social intelligence composite score on self-rated social skill and the general intelligence composite score. Self-ratings remained a significant, though directionally smaller, negative predictor of objective social intelligence ($\beta = -.16$, $p < .001$, 95% CI [–.22, –.09]) when controlling for general intelligence task performance. These findings continue to hold when controlling for available participant demographics (see supplement).

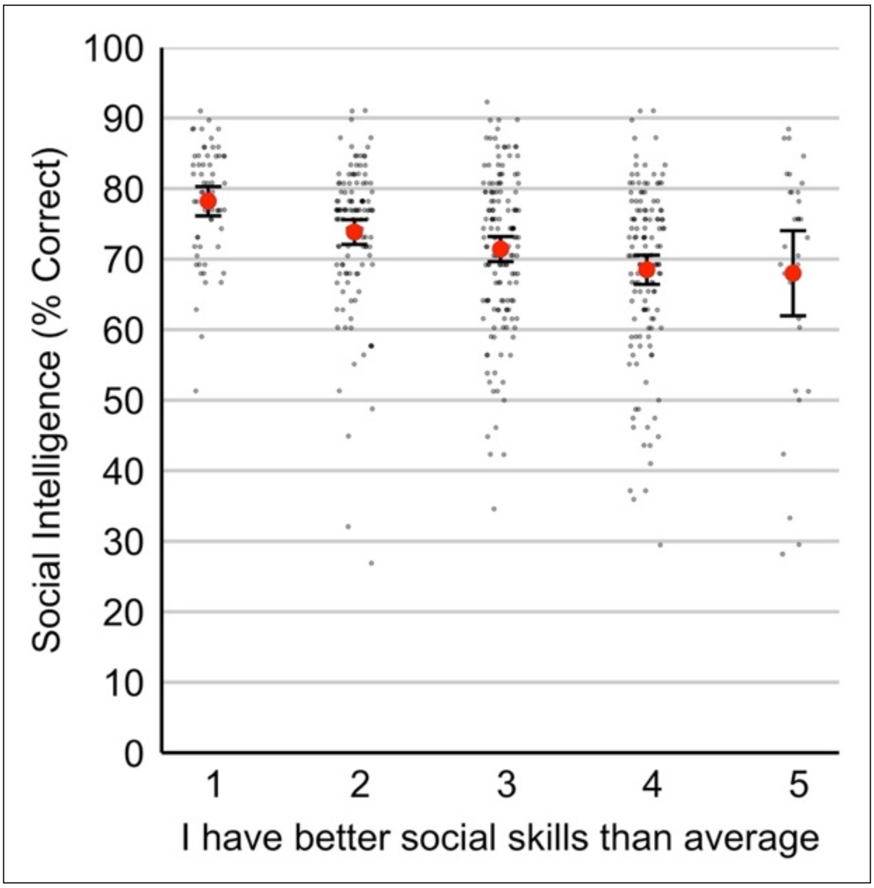

Figure 1 Social Intelligence Scores at Each Level of Self-rated Social Skill (Study 1; *N* = 505).
Note. Error bars display 95% CI.

OUTCOME: SOCIAL INTELLIGENCE TASK PERFORMANCE	B (SE)	β	95% CI	R^2
Study 1 (N = 505)				.40***
IQ Composite	0.30 (0.02)	.59***	.52, .65	
Self-rated Social Skill	–1.61 (0.37)	–.16***	–.22, –.09	
Study 2 (N = 422)				.45***
IQ Composite	0.47 (0.03)	.56***	.48, .64	
Predicted SI Score	0.17 (0.05)	.14***	.06, .21	
Self-rated Social Skill	–3.12 (0.57)	–.21***	–.28, –.13	

Table 3 Multiple Regression Results from Studies 1 and 2.
Note. Study 1: $F(2, 502) = 167.80$, $p < .001$; Study 2: $F(3, 418) = 115.70$, $p < .001$; β = standardized regression coefficient; B = unstandardized regression coefficient; SE = standard error; *** $p < .001$.

DISCUSSION

Study 1 revealed that claiming to have above average social skills negatively predicted skill-based measures of social intelligence even after controlling for general intelligence. This result is inconsistent with previous reports of positive relationships between broad self-perceptions and objective performance across various tasks, domains, and timepoints (Zell & Krizan, 2014). However, there are several limitations to conclusions drawn from this result. First, we did not predict a negative relationship before conducting Study 1 and the result was only discovered after conducting exploratory analyses. Second, Study 1 provides no evidence that this relationship is specific to social intelligence or to the tasks used to measure it. Study 1 was also limited by acceptable but generally low reliabilities for the two general intelligence tasks. We designed Study 2 to address these limitations.

We sought to replicate the negative relationship between claiming to have above average social skills and social intelligence in a preregistered study using a new sample of participants recruited from a different crowdsourcing service. We also aimed to more rigorously test the hypothesis that claims to have above average social skills are more likely false than true using a preregistered positive control. Specifically, we asked whether a similar negative relationship would also emerge for parallel measures of self-ratings and performance in the domain of general intelligence.

STUDY 2

METHOD

Participants

We recruited 461 US-based participants using Prolific (Peer et al., 2017) in 2020. A target sample of 450 was set to provide 95% power to detect a correlation as small as $r = .17$ ($\alpha = .05$, two-tailed test; Faul et al., 2009), which was the lower bound of the 95% confidence interval for the exploratory correlation observed in Study 1. To increase racial and ethnic diversity in our sample, we conducted two recruitment phases: one sampling of 300 participants with no racial or ethnic selection filters and a second, independent sampling of 150 participants with eligibility restricted to those who identified as any race or ethnicity other than white, including multiracial. The second phase was conducted immediately after the first, precluding any participants who completed the first phase from participating again. We did not examine the data until collection was complete. As in Study 1, the goal of this sampling approach was to improve the generalizability of our results relative to past studies (Roberts et al., 2020). Participants were paid $4.70 (median completion time = 31 minutes). We included one attention check item that asked participants to select a specific scale point and removed 39 participants who failed, resulting in a final sample of 422 participants (see Table 1 for demographics).

Procedure and Tasks

Objective social intelligence

As in Study 1, we administered three measures of social intelligence. Participants completed 10-item forms of the SST, RMET, and STEU-B. The ten RMET items were selected based on a short form developed by Olderbak et al. (2015). The ten SST and STEU-B items were selected using an item analysis of the response data in Study 1 based on item difficulty (proportion correct in the sample) and corrected item-total correlations from the full task batteries (Stanton et al., 2002). All three short forms yielded acceptable internal consistency (α ranging from .64 to .69). We again summed all three social intelligence task scores to form a unit-weighted composite score expressed as percentage of items answered correctly ($\alpha = .83$).

General intelligence

We measured verbal ability using the ten-item Wordsum vocabulary test ($\alpha = .78$). We also measured abstract reasoning using a ten-item test using matrix items like those used in Study 1 ($\alpha = .72$). These tasks had greater reliability than the intelligence tasks in Study 1. They were combined to form a unit-weighted composite of general intelligence with nearly identical reliability to the social intelligence composite ($\alpha = .81$).

Self-rated social skill

We used the same item from Study 1 ("I have better social skills than the average person"), using the same scale points (ranging from 1 = "*Strongly Disagree*" to 5 = "*Strongly Agree;*" 3 = "*Neither Agree nor Disagree*") to measure self-rated social skill. We also measured comparative self-ratings of intelligence by asking participants to agree or disagree with the statement "I am more intelligent than the average person" using the same response options.

Predicted SI and IQ task scores

New to this study, we asked participants to predict the number of items that they answered correctly (ranging from 0 to 10) on a separate page immediately following each of the five tasks.

RESULTS

Table 4 displays all intercorrelations and descriptive statistics. Any analysis that was not preregistered is identified as "exploratory." We replicated the negative relationship between self-rated social skill and social intelligence ($r(420) = -.37$, $p < .001$, 95% CI [–.45, –.28]) (see Figure 2, left). An exploratory analysis motivated by Heck and Krueger (2015) indicated that among those who "somewhat" or "strongly" agreed that they have above average social skills (41%; 171/422), only 33% outperformed the median score of 57 (46% outperformed the mean score of 68.0). An exploratory analysis replicated the finding that those in the first quartile of performance on the SI tasks once again had directionally higher social overplacement ratings (*M* = 3.7, *SD* = 1.1) than those in the second (*M* = 3.0, *SD* = 1.1), third (*M* = 2.7, *SD* = 1.2), and fourth sample quartiles (*M* = 2.8, *SD* = 1.2).

Was this negative relationship unique to *social* intelligence? Relative self-ratings of social skill and intelligence were moderately correlated ($r(420) = .41$, $p < .001$, 95% CI [.33, .49]), but there was no meaningful correlation between self-rated intelligence and the general intelligence composite score (see Figure 2, right) ($r(420) = -.04$, $p = .41$, 95% CI [–.14, .06]), nor with performance on either the individual discrete vocabulary ($r(420) = -.02$, $p = .69$, 95% CI [–.12, .08]) or matrix reasoning tasks ($r(420) = -.05$, $p = .35$, 95% CI [–.14, .05]). An exploratory analysis revealed that among those who claimed to have above average intelligence (54%; 226/422), 49% scored above the median (110/226). This positive control strengthened our confidence that the negative relationship between self-ratings and performance was specific to social intelligence, and this relationship could not be explained by participants' self-ratings or performance on general intelligence tasks.

Unique to Study 2, we examined how claiming to be above average compared to participants' numeric performance predictions on the social intelligence and general intelligence tasks. Recall that participants

	M	SD	1	2	3	4	5	6	7	8	9	10
1. Self-rated Social Skill	3.1	1.2	—									
2. Self-rated Intelligence	3.5	1.0	**.41**	—								
3. SST	70.2	22.7	**–.29**	**–.11**	*.69*							
4. RMET	70.2	21.8	**–.35**	**–.13**	**.53**	*.66*						
5. STEU-B	63.7	21.4	**–.28**	–.06	**.53**	**.56**	*.64*					
6. SI Composite	68.0	18.3	**–.37**	**–.12**	**.83**	**.84**	**.83**	*.83*				
7. Vocabulary	59.3	25.1	**–.26**	–.02	**.31**	**.37**	**.41**	**.43**	*.78*			
8. Matrix Reasoning	58.4	27.5	**–.28**	–.05	**.56**	**.47**	**.53**	**.62**	**.39**	*.72*		
9. IQ Composite	58.8	21.9	**–.32**	–.04	**.53**	**.50**	**.56**	**.64**	**.82**	**.85**	*.81*	
10. Predicted SI Score	67.0	14.4	**.16**	**.20**	**.15**	**.13**	.08	**.14**	.05	.07	.07	—
11. Predicted IQ Score	56.2	18.5	**.12**	**.29**	.04	.02	.01	.03	**.36**	**.24**	**.35**	**.52**

Table 4 Correlation Matrix and Descriptive Statistics for Study 2.
Note. $N = 422$; bold values indicate $p < .05$; coefficient alpha is reported in italics on the main diagonal.

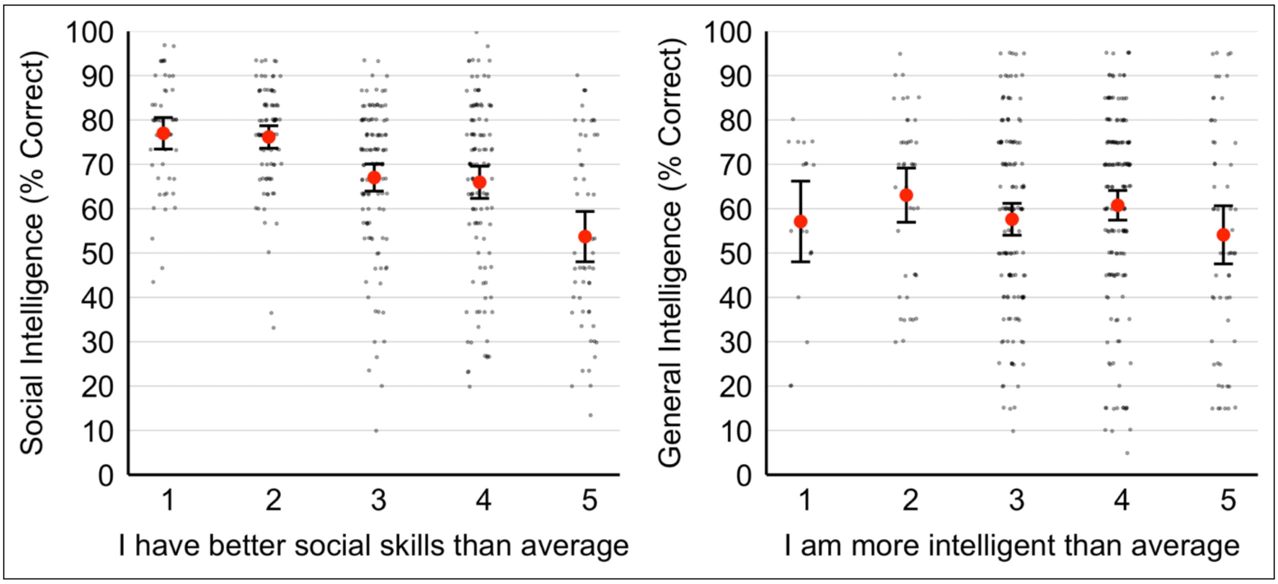

Figure 2 Social and General Intelligence Scores at Each Level of Comparative Self-Rating for Social Skills and Intelligence (Study 2; $N = 422$).
Note. Error bars display 95% CI.

predicted the number of items they believed they answered correctly immediately after completing each task. These predicted scores were positively related to social intelligence composite scores ($r(420) = .14$, $p = .003$, 95% CI [.05, .24]) despite also being positively correlated with self-reports of social skills ($r(420) = .16$, $p = .001$, 95% CI [.06, .25]). This suggests that participants were able to predict their social intelligence test scores with some accuracy, albeit low, when these estimates were made (1) immediately following each task and (2) using numeric response options. This relationship was more than twice as large for general intelligence tasks ($r(420) = .35$, $p < .001$, 95% CI [.27, .43]), suggesting that participants were better judges of their explicit performance on cognitive tasks than on social tasks.

How large was the difference in social intelligence scores between those who claimed to have better social skills than average and those who did not? An exploratory one-way ANOVA revealed that participants who "somewhat" or "strongly" agreed with the item (selecting option 4 or 5) scored 10.1 percentage points lower on the social intelligence tasks than participants who selected any other option (1, 2, or 3), $F(1,420) = 33.59$, $p < .001$, $d = 0.58$, 95% CI [0.38, 0.77]. However, a similar analysis revealed no association between claiming to have above average intelligence on composite intelligence scores ($F(1,420) = 0.02$, $p = 0.90$, $d = -0.01$, 95% CI [−0.20, 0.18]) or on social intelligence scores, $F(1,420) = 0.66$, $p = 0.42$, $d = 0.08$, 95% CI [−0.11, 0.27].

As in Study 1, we conducted an exploratory regression using self-rated social skill and composite general intelligence scores to predict composite social intelligence scores, this time also entering the new variable of how many social intelligence items participants predicted they answered correctly (see Table 3). Self-rated social skill remained a reliable negative predictor of objective social intelligence scores ($\beta = -.21$, $p < .001$, 95% CI [–.28, –.13]) when controlling for general intelligence composite scores, for the narrowly predicted score on each task, and for participant demographics (see supplement).[6] Interestingly, participants' predictions of how many social intelligence task questions they answered correctly positively predicted objective social intelligence beyond self-rated social skill ratings and general intelligence scores ($\beta = .14$, $p < .001$, 95% CI [.06, .21]). This exploratory result suggests that participants were partially accurate when reporting how well they performed on the specific social intelligence tasks but remained inaccurate when reporting a broad comparison of their social skills relative to the average person's.

GENERAL DISCUSSION

We found evidence that the belief that one has better social skills than the average person was negatively associated with performance on three skill-based tests of social intelligence. We observed this effect in an exploratory analysis conducted in Study 1 and replicated it in a different and more diverse population using preregistered confirmatory testing with a positive control in Study 2 (combined total $N = 927$). Moreover, this pattern was unique to social intelligence and did not emerge for a comparable self-report measure and objective tests in the domain of general intelligence. The negative relationship between self-rated social skill and social intelligence emerged despite the observation that participants were able to estimate their scores on both the social and general intelligence tasks with some accuracy, and it remained robust after controlling for (1) these specific task performance estimates, and (2) participants' general cognitive ability.

These results depart from, and may be an exception to, meta-analytic research that finds a reliably positive correlation between self-report measures and performance across a variety of domains (Freund & Kasten, 2012; Zell & Krizan, 2014). Additionally, the effect sizes we observed were substantial: a correlation of −.37 between self-report and performance in a preregistered replication study, with $d = 0.58$ in the comparison of performance scores between those who agreed that they have better social skills than average and those who did not. Given the positive accuracy correlation between self-estimates of ability and actual ability (estimated to be $r = .29$ in a meta-synthesis of 22 meta-analyses; Zell & Krizan, 2014), and the positive correlation between estimated intelligence and IQ ($r = .33$; Freund & Kasten, 2012), the fact that we observed and replicated effects as large as these but with a sign change merits explanation and future study.

EXPLAINING THE RELATIONSHIP BETWEEN SELF-REPORTED SOCIAL SKILLS AND SOCIAL INTELLIGENCE

There is greater disagreement among scholars about the structure of social intelligence than the structure of general intelligence (Lievens & Chan, 2010), and it is no surprise that people vary in their interpretation of what constitutes social skills (Mayer, Salovey, & Caruso, 2004; Riggio, 1986). This may help explain our results. Given that people can selectively or idiosyncratically define what it means to have "above average social skills," this assessment might fail to positively predict performance on a static measure. Participants who claim to be above average may in fact be accurate according to their own favored definition of what "social skills" entail, or based on a notion of an "average" person that produces a favorable comparison for them.

Our exploratory results were partially consistent with the Dunning-Kruger effect, a phenomenon of social judgment where the lowest performers overestimate their performance by the widest margin (Kruger & Dunning, 1999). Those who perform poorly on social intelligence tasks may lack the metacognitive awareness necessary to accurately evaluate their standing, but our data do not provide evidence for or against the metacognitive account. Any future tests of the metacognitive account must take care to rule out statistical confounds (McIntosh, Moore, Liu, & Della Sala, 2022; Gignac & Zajenkowski, 2020) and consider testing for deviations from a noise-plus-bias model of comparative judgment (e.g., Burson, Larrick, & Klayman, 2006; Krueger & Mueller, 2002). Still, it is novel and noteworthy that we observed a *negative* relationship between self-evaluations and objective social intelligence test performance. Most empirical patterns that are consistent with the Dunning-Kruger effect entail greater overconfidence at the low end of the skill distribution but ultimately uncover a positive correlation between self-ratings and reality across ability levels.

Finally, self-evaluations may have been distorted or biased by the level of social desirability participants assign to being socially skilled. We did not measure social desirability, but past research has indicated that emotional intelligence is perceived as modestly more socially desirable—and therefore more subject to socially desirable responding—than general intelligence (Gignac, 2018). Other research found that individuals who are high in emotional intelligence are more likely to engage in socially desirable responding, even after controlling for self-esteem and overclaiming tendency (Mesmer-Magnus, Viswesvaran, Deshpane, & Joseph, 2006). Beyond emotional intelligence, Grieve and Mahar (2013) reported a positive but modest relationship between scores on a self-report measure of social intelligence (the Tromsø Social Intelligence Scale) and the Marlowe-Crowne Social Desirability scale. Despite this observation, it is noteworthy that participants in our second study were more likely to claim above average intelligence than above average social skills. We recommend conducting research to examine the relationship between social intelligence, self-evaluation accuracy, and socially desirable responding.

For general intelligence, we did not expect (and did not find) a negative relationship between comparative self-ratings and performance. One possible explanation for this is that individuals can draw from many prior life experiences when evaluating themselves. Valid feedback points may include past grade point averages from secondary or post-secondary education, performance on standardized tests, and feedback they have received from peers and relatives. Many of these cues happen to be strongly correlated with one another and with performance on intelligence tests (Brown et al., 2021; Frey & Detterman, 2004). Additionally, individuals who are accurate in self-assessments of their own general intelligence tend to be more open and conscientious (Mayer, Panter, & Caruso, 2020), raising the possibility that they are more attentive or receptive to these cues. This is part of the explanation for why researchers expect and often find positive relationships between self-estimates and performance in many domains (Zell & Krizan, 2014). In contrast, social skills potentially include a broader range of attributes which many people perceive as distinct from academic or general intelligence (Sternberg et al., 1981). Elements in this broad range may involve, for example, comfort in social situations (e.g., extraversion), concern for others (e.g., agreeableness), and the ability to detect other people's mental states (e.g., Theory of Mind). Not only are these characteristics weakly correlated with one another, relative to aspects of general intelligence, but there are also fewer unbiased sources of feedback regarding these abilities. Further complicating matters, individuals who are low in social intelligence are necessarily less capable of accurately perceiving and understanding their surrounding social environment (Mayer et al., 2004). This was the reasoning we used to develop the positive control in our second study, which confirmed that the negative accuracy correlation we observed was unique to social intelligence.

IMPLICATIONS FOR MEASUREMENT AND STRATEGIC BEHAVIOR

Past research on self-presentation and social judgment has viewed self-enhancement through a game theoretic lens (Krueger et al. 2020; Heck & Krueger, 2015; Grüning & Krueger, 2022). There are some related implications of our results both for strategic interpersonal behavior and construct measurement. The former entails a self-presentation paradox. Past research on the hubris hypothesis (Hoorens et al., 2012) and conflicting motives of self-presentation (Heck & Krueger, 2016) suggests that claiming inferiority might be an optimal social strategy in some contexts. Inasmuch as humility is a socially desirable and adaptive trait (Feather, 1994; Wosinka et al., 1996), and that *false* or insincere humility (e.g., "humblebragging") can be reputationally costly (Sezer et al., 2018), the process of deciding whether to claim above average social skills is one that may itself require social skill. People who perform the best on social intelligence tasks may be so socially intelligent that they humbly claim to be inferior to others specifically for the benefits that doing so confers (Heck & Krueger, 2016), or because they believe that doing so would be socially desirable (Mesmer-Magnus et al., 2006). This may also help explain our overall lack of a mean-level better-than-average effect on the social skills item—in both studies, the average response to this item was near the scale midpoint. We therefore cannot rule out the possibility that those who claimed to have below average social skills in our studies did so as a socially intelligent demonstration of humility.

Second, our findings provide further evidence that self-report measures of social intelligence—especially those that entail comparison to others—cannot substitute for objective, performance-based measures. As previously reported (e.g., Brackett et al., 2006; Brannick et al., 2009; Murphy & Lilienfeld, 2019; Realo et al., 2003), the correlation between self-report and objective social and emotional intelligence measures is weak. In both studies reported here, however, we observed a reliably negative relationship. This inconsistency may be problematic for research on social intelligence that relies on self-reports alone, as self-report and performance-based measures appear to cohere weakly or, as we observed, negatively. Compared to self-evaluations of general ability, broad self-ratings of social ability may be more biased by overconfidence, self-enhancement, or other characteristics not representative of the skills measured by social intelligence tests. It is noteworthy that in Study 2, participants were able to predict their social intelligence test scores with positive but limited accuracy (r = .14) when making specific, numeric estimates immediately after completing each test. The observation that concrete, numeric task score predictions positively predicted performance, while broadly measured comparative self-reports negatively predicted performance, is theoretically and practically intriguing.

LIMITATIONS AND FUTURE RESEARCH

Our studies were limited in several ways. In this research, we assume a positive association between social intelligence and participants' interpretation of "social skills," a term which trades technicality for familiarity. We chose to measure beliefs by asking about social skills specifically with the expectation that people would find it interpretable, and because tests of social intelligence rely on tasks that evaluate specific skills. We also used only a single item to measure comparative self-ratings, which increases the risk that participants interpreted this construct differently than we intended (see Zell, et al. 2020 for meta-analytic differences between one-item and multi-item measures of the better-than-average effect, both of which continue to be employed by researchers).

It is also unclear whether our result is driven by the comparative nature of the self-report question, by variation in self-ratings alone, or by variation in ratings of the average person (Klar & Giladi, 1999). We observed that claiming above average general intelligence was *not* correlated with intelligence performance tasks—it is possible that the comparative nature of the question may attenuate the correlation between self-perceptions and ability (Gignac, 2018; Krueger et al., 2017). Future research might therefore consider measuring social comparison using the indirect approach (i.e., eliciting self-ratings and ratings of the average person using separate items) or using multi-item, scale-based measures of self-enhancement.

Finally, we note that our social and general intelligence measures tended to be highly correlated with each other, which may have partially undermined their discriminant validity. This was not surprising given the testing environment: a self-administered, online task can be expected to produce shared method variance (e.g., attentive participants are likely to score higher on both tasks than inattentive participants). Nevertheless, we confirmed that the relationship between claiming to have above average social skills and social intelligence held even when controlling for general intelligence test performance.

CONCLUSION

Our results suggest that across a wide range of measured social intelligence, people were unaware of how their social abilities compare to others'. Given that a person believes they are more socially skilled than average, our results suggest that this person is likely to underperform on objective tests of social intelligence. Although it is not clear what thoughts and experiences connect claims to have above average social skills with comparatively low performance on social intelligence tasks, the pattern we observed was reliable, sizable, and distinct from beliefs about general intelligence. We recommend that future research on self-knowledge in social intelligence consider the relationship between self-estimates, social comparisons, and real-world social ability.

TRANSPARENCY AND DATA ACCESSIBILITY STATEMENT

We reported how we determined the sample size and the stopping criterion. We reported all experimental conditions and variables. We report all data exclusion criteria and whether these were determined before or during the data analysis. We report all outlier criteria and whether these were determined before or during data analysis. Data are available at https://osf.io/3gtzx.

NOTES

1 Using estimates of r = .3–.4 as inputs to Rosenthal's (2014) Binomial Effect Size Display (BESD), we can estimate that claiming to be above average should result in 35%–40% greater likelihood of actually being above average (i.e., a hit rate of 65% for r = .3 and of 70% for r = .4).

2 For example, a participant who answered 20/23 (87%) SST items correctly, 30/36 (83%) RMET items correctly, and 15/19 (79%) STEU-B items correctly would receive a composite score of (20 + 30 + 15)/78 = .83, or (87% + 83% + 79%)/3 = 83%.

3 For discussion of the direct approach to measuring self-enhancement, overplacement, and the better-than-average effect, see Humberg et al., 2018, Klar and Giladi (1999), Krueger and Wright (2011), and Krueger, Heck & Asendorpf (2017).

4 We report these means to show a descriptive pattern; we did not test for significant differences between them.

5 However, this relationship is no longer significant when controlling for composite score on the social intelligence tasks.

6 We also conducted a similar exploratory regression analysis on whether comparative self-rated general intelligence negatively predicted general intelligence performance. After controlling for social intelligence score and the predicted number of correct responses to general intelligence test items, general intelligence self-ratings were directionally negatively related to intelligence composite scores but this estimate did not meet our criteria for statistical significance (β = –.07, p = .05, 95% CI [–.14, .00]). This predictor was also not statistically significant after controlling for participant age and sex.

COMPETING INTERESTS

The authors have no competing interests to declare.

AUTHOR CONTRIBUTIONS

Conceptualization: PRH, MB, CFC
Data Curation: PRH, MB
Formal Analysis: PRH, MB
Funding Acquisition: N/A
Investigation: PRH, MB, CFC
Methodology: PRH, MB, CFC
Project Administration: PRH, MB, CFC
Resources: PRH, MB
Software: PRH, MB
Supervision: CFC
Validation: PRH, MB, CFC
Visualization: PRH, MB, CFC
Writing – Original Draft: PRH, MB
Writing – Review & Editing: PRH, MB, CFC

AUTHOR NOTES

All studies were determined to be exempt from review by Geisinger's IRB. All authors have no conflicts of interest to report. The views expressed here are those of the authors and do not represent the views of the Consumer Financial Protection Bureau or the United States.

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