Is Overconfidence a Motivated Bias?

Experimental Evidence

Jennifer M. Logg
University of California, Berkeley

Uriel Haran
Ben-Gurion University of the Negev

Don A. Moore
University of California, Berkeley

Author Note

Jennifer M. Logg, Haas School of Business, University of California, Berkeley; Uriel Haran, Guilford Glazer Faculty of Business and Management, Ben-Gurion University of the Negev; and Don A. Moore, Haas School of Business, University of California, Berkeley.

Thanks to Leif Nelson and Elizabeth Tenney for their helpful comments and insights and to Heather Yang and Silva Kurtisa for help running experiments. Thanks also to Sahaana Suri, Jonathan Wang, and Jennifer Georgevich for their assistance collecting data.

Correspondence concerning this article should be addressed to Jennifer M. Logg, Haas School of Business, University of California, Berkeley, 425 Student Services Building #1900, Berkeley, CA 94720-1900. E-mail: jenn_logg@haas.berkeley.edu
Abstract

Does the motivation to view oneself positively lead people to hold overly positive beliefs about themselves? Although many have assumed so, there is a shortage of experimental evidence for this causal claim. We sought to test this relationship by experimentally manipulating motivation. We only found an effect of motivation on self-reports when assessments were made about vague personality traits using vague measures (Experiment 1) but not using specific measures (Experiment 2). Given that overconfidence requires an objective standard, we did not find evidence that motivation affects overconfidence when such a standard is used (Experiment 3). The lack of an objective standard for vague traits allowed people to create idiosyncratic definitions and view themselves as better than others in their own unique way (Experiment 4). The results suggest motivation’s effect on overconfidence is neither strong nor simple, and emerges only on vague measures of vague self-assessments.

Keywords: self-perception, overconfidence, motivation, better-than-average effect, specificity

Abstract Word Count: 145/150
Is Overconfidence a Motivated Bias? Experimental Evidence

“Everyone suspects himself of at least one of the cardinal virtues, and this is mine: I am one of the few honest people that I have ever known.” - Nick Carraway in F. Scott Fitzgerald’s The Great Gatsby.

People view themselves as better than others on a variety of abilities and attributes, including honesty (Brown, 2012), leadership skills (Dunning, Heath, & Suls, 2004), popularity (Zuckerman & Jost, 2013), and safe driving (Svenson, 1981). A wide range of populations hold overly confident beliefs that they are better than others. Business people think their firms are better than the average firm (Cooper, Woo, & Dunkelberg, 1988; Larwood & Whittaker, 1977), engineers think their work is superior to their peers’ work (Zenger, 1992), and bungee jumpers believe that their jumps are less likely to result in injury than are others’ (Middleton, Harris, & Surman, 1996). What causes such beliefs?

Many researchers have assumed that flattering self-perceptions are driven by the desire to view oneself positively (Dunning, 2005; Fabricius & Büttgen, 2013; Greenwald, 1980; Kunda, 1990; Radhakrishnan, Arrow, & Sniezek, 1996; Sedikides & Gregg, 2008). Alicke and Govorun (2005) concluded that “the better-than-average effect is due, at least in part, to a desire to view oneself in a favorable light relative to one’s peers.” The better-than-average (BTA) effect is evident in results showing that when a set of people estimate their percentile rankings relative to others, the mean is above the 50th percentile (Benoît, Dubra, & Moore, 2015; Svenson, 1981). In fact, the assumption that flattering self-perceptions are motivated is so pervasive that some have claimed a “well-established connection between traditional optimism biases and motivated reasoning” (O’Brien, 2013) and that “the better-than-average bias is caused by our strong unconscious desire to maintain a positive self-view” (Chamorro-Premuzic, 2013).
Researchers have assumed that a motivation to view oneself in a positive light is the psychological mechanism driving better-than-average effects (Taylor & Brown, 1994). The more important the attribute, the greater one’s motivation to self-enhance on that attribute (Dunning, Leuenberger, & Sherman, 1995; Kunda, 1987). This account predicts that the more someone values an attribute or ability, the more positive their self-perceptions (Shepperd, Klein, Waters, & Weinstein, 2013). While it may make sense intuitively that the motivation to view oneself in a positive light drives excessively positive self-perceptions, causal evidence to support this claim is surprisingly sparse. Instead, existing evidence is primarily cross-sectional and correlational. To test the causal claim that motivation causes overconfidence, experiments that exogenously manipulate motivation are necessary (Dunning, 1999). Furthermore, a motivational account is but one of several potential causes of overconfidence.

Cognitive, non-motivational accounts of overconfidence enjoy substantial empirical support (Chambers & Windschitl, 2004; Moore, 2007). These cognitive theories do a good job accounting for important features of the empirical evidence, including underestimation of performance on easy tasks and worse-than-average effects on difficult tasks (Moore & Small, 2007; Moore & Healy, 2008). However, these cognitive theories do not rule out motivational influences on self-assessment. In this paper, we seek to document the role of motivational forces in causing overconfidence by employing experimental manipulations that allow us test a causal connection between motivation and overconfidence.

**Do Motives Matter?**

Existing evidence for the motivational origins of overconfidence suffers from three limitations: overreliance on correlational evidence, confounding commonness with importance,
and vague performance standards. We outline these shortcomings and how our experiments address them.

**Correlational evidence and confounds.** Correlational evidence shows that people are more likely to hold positive self-perceptions in domains they value (Alicke, 1985; Kunda, 1990; Mazar, Amir, & Ariely, 2008) including those valued distinctively by their own cultures (Sedikides, Gaertner, & Toguchi, 2003) and those useful for obtaining future goals (Dunning, 1995). These results leave open several different causal paths. People might overestimate skills in domains they initially value. Or they might assign greater importance to domains where they already consider themselves skilled. Or they might, quite sensibly, work to develop skills in domains they believe are important. Correlational evidence has trouble distinguishing these different causal accounts.

Two prior experiments merit special note. Dunning, Meyerowitz, and Holzberg (1989) found that people displayed a larger BTA effect for positive than negative traits. Specifically, participants ranked themselves in the 59th percentile for the traits “talented” and “athletic” but in the 39th percentile (below the median) for the traits “trouble handling money” and “socially anxious.” It is worth noting that, as with so much of the evidence, Dunning et al. did not exogenously manipulate motivation but instead examined different domains that varied not only in valence, but also in other aspects. It is, of course, possible, that Dunning et al. happened to pick negative traits which people thought they possessed to a lesser extent than others, perhaps because they are rare (Kruger & Savitsky, 2009).

In one study that did manipulate motivation while holding a list of traits constant, Brown (2012) found a larger BTA effect for traits described as “important and rare” than as “unimportant and common.” Unfortunately, the manipulation confounded importance with
commonness. As Chambers, Windschitl, and Suls (2004) reported, commonness appears to be a stronger driver of BTA beliefs than importance. People tend to think they are more likely than others to possess common traits and less likely than others to possess rare traits (Kruger & Burrus, 2004).

Vague performance standards. A third shortcoming of the extant literature is a focus on the use of primarily vague self-assessments (usually in the domain of personality traits) and vague measures, at the expense of ignoring more specific self-assessments (such as objective performance) and measures which are objectively verifiable. Self-serving attributions are stronger within vague contexts and for ambiguous traits compared with more precise contexts and traits (Dunning et al., 1989; Sloman, Fernbach, & Hagmayer, 2010). Ambiguous domains allow for idiosyncratic definitions and assessment criteria (Weinstein, 1980; Weinstein & Lachendro, 1982). Using idiosyncratic definitions of the trait to assess themselves allows everyone to (correctly) claim that they are better than the average person (van den Steen, 2004).

In addition to specifying the criteria of assessment, measuring an individual’s overconfidence, rather than the group’s, requires an objective benchmark of individual performance. While some work in the motivated cognition literature has manipulated the importance of a trait and measured either self-perceptions (Kunda & Sanitioso, 1989) or perceptions of others (Klein & Kunda, 1992), no research, to our knowledge, has manipulated motivation and then compared self-assessments with reality. Moreover, BTA measures only partially tell the story of how motivation affects flattering self-perceptions because they capture accuracy at the aggregate and not the individual level (Harris & Hahn, 2011). When the majority of people in a sample make BTA claims (more precisely, claims that they are better than the sample median), the sample as a whole appears biased because only half of the sample can
Overplacement compares beliefs to objective measures of performance (Moore & Healy, 2008), calibrating BTA beliefs against reality (Larrick, Burson, & Soll, 2007). Overconfidence is, by definition, confidence in excess of that justified by reality. An objective benchmark for assessing the accuracy of beliefs is therefore essential for measuring overconfidence. This objective benchmark has been absent in most of the prior research that has examined motivational effects on self-perceptions. In sum, the existing empirical record calls for further testing of the causal claim that motivation affects overconfidence.

Overview of Experiments

We contribute to the literature on self-perception by providing direct experimental tests of the hypothesis that motivation affects overconfidence. If overconfidence is motivated, people should display greater overconfidence for an ability or attribute that they see as important, compared with an unimportant one. Our experiments seek to address some of the shortcomings of prior work.

Addressing correlational evidence and confounds. Manipulating motivation, rather than measuring its correlates, is key to identifying a causal relationship between motivation and overconfidence. In Experiments 1 and 2, we manipulated motivation for a single trait. Manipulating the importance of one trait or ability allows us to minimize the potentially confounding role of other correlates. In Experiment 3, we manipulated the importance of performance on a single task and benchmarked participants’ beliefs against reality. We utilized
both manipulations adapted from prior research (Experiments 1 and 2) and novel manipulations of our own devising (Experiment 3).

**Leveraging vagueness of performance standards.** We systematically varied vagueness of assessments and examined its effects on the relationship between motivation and overconfidence. The experiments in this paper progress from vague to specific in their:

1) criteria of assessment (vague traits to trait clearly defined)

2) measures of assessment (verbally labeled scales for BTA measures to numeric elicitations for overplacement measures) and

3) domain of assessment (personality traits to test performance).

Consistent with extant literature, we compared assessments made on vague measures with verbally labeled scales regarding a vague versus behaviorally-defined personality trait (Experiment 1). We extended the literature by introducing a specific, numeric measure of assessment for a personality trait (Experiment 2) and then used both vague and specific measures of assessment within an objective domain, performance on a test (Experiment 3). Last, we varied vagueness within-subject, which allows us to observe the emergence of overconfidence, its reduction through clearer measures, and its subsequent re-emergence with measures that allow for idiosyncratic definitions (Experiment 4). See Table 1.

We extend the examination of motivation and overconfidence beyond the specificity of the domain or trait (Dunning et al., 1989) by also varying the specificity of the measure of the assessment. A subjective domain, such as personality traits, provides the most latitude for the emergence of overconfidence, increasing the leeway for motivation to influence it. An objective domain, such as test performance, increases the verifiability of assessments, which should suppress bias in self-perceptions. Measures with objective standards help address concerns
about how variations in perceived value can shift construal of vague scales. They also allowed us to measure overplacement, differentiating between individuals who accurately claim superiority from those who overplace themselves.

Our manipulations of motivation represent an attempt to understand how perceived importance affects overconfidence. But any such attempt would be incomplete without an examination of what people mean when they claim to be better than others. In Experiment 4, we tested whether the specification of assessment criteria decreases BTA beliefs by manipulating the specificity of a single trait within-subjects, and holding motivation constant. Varying ambiguity of traits within-subjects in Experiment 4 allowed us to examine whether people’s trait construals were idiosyncratic and whether these construals play a role in driving inflated relative self-perceptions. Our results challenge the simple claim that motivation leads to overconfidence.

**Experiment 1: Specifying Criteria of Assessments for Introversion**

Experiment 1 tests the causal effect of motivation on BTA beliefs. Knowing the power of trait vagueness to influence BTA beliefs, we varied the specificity with which the trait was described. Specifying traits may reduce variation in construals of performance, which should attenuate any potential effect of motivation on overconfidence.

Importantly, rather than using different traits that might vary in importance, we manipulated motivation for a single trait—introversion. Introversion has the advantage that it can be plausibly viewed as either desirable or undesirable (Cain, 2013). We manipulated the motivation to view oneself as introverted by varying its perceived importance and measured the extent to which people viewed themselves as more introverted than others.
Method

Participants

Two hundred and twelve people (109 women, 103 men; $M_{age} = 30.22, SD = 12.20$) completed two ostensibly unrelated surveys in the lab through the undergraduate participant pool at a West Coast university (compensated with course credit) and via Amazon’s Mechanical Turk (compensated with an average of $0.45$). Our sample size, determined ex-ante, sought two hundred and ten participants to detect a medium effect ($f = 0.25; d = 0.5$) with 95% power. For this experiment, and all that follow, we report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures (Simmons, Nelson, & Simonsohn, 2011).

Design

The experiment had a 2 (Importance: high, low) x 2 (Criteria specificity: vague, specific) between-subjects design. The dependent variable was the BTA effect, measured by the difference between participants’ ratings of introversion for themselves and others.

Procedure and Materials

Importance manipulation. Participants completed what were described as two separate surveys on personality traits and leadership. The first varied the importance of a trait using a manipulation adapted from Kunda and Sanitioso (1989). In the high importance condition, participants read that introversion may help people succeed. In the low importance condition, participants read that extroversion may help people succeed. Following the passage, participants were asked to list two examples from their lives which showed how introversion

---

1 Neither the BTA nor manipulation check measures differed between these samples, $t < 1.44, ps > .14$.
2 Two hundred and twenty-six Mechanical Turk participants pre-tested the effectiveness of our importance manipulation for $0.25$. Introversion was rated more important by those in the high ($M = 3.38, SD = 0.97$) than low importance condition ($M = 2.65, SD = 0.97$), $t(103) = 3.84, p < .001, d = 0.75$. 
[extroversion] made them or someone they know a good leader. A link to the survey, which is posted to the Open Science Framework website, is provided in the Appendix.

Specificity of criteria manipulation. On the following page, participants took an ostensibly unrelated survey in which they rated how well traits described them and others. In the specific condition, traits were specified with five relevant behaviors (e.g., introverted: I work alone when I can rather than with a group), whereas the vague condition included only the trait names (introversion, outgoing [extroversion], conscientious, imaginative, agreeable, and honest). Traits and behaviors are listed in Table 2.

BTA measure. Participants rated how well each trait described themselves and most other people on a verbally labeled scale ranging from 1 = Not at all to 5 = Very. Both the order of the traits and self- and other-ratings were randomized. We measured BTA beliefs indirectly by comparing two separate assessments (e.g., “How introverted are you?” and “How introverted are others?”). An indirect measure is a more conservative measure of BTA beliefs because a direct measure, which consists of one comparative assessment, produces stronger BTA beliefs (Chambers & Windschitl, 2004; Moore, 2007; Otten & van der Pligt, 1996).

Importance manipulation check. On the last page of the experiment, participants separately rated how important they thought it was for a person to possess the attributes INTROVERSION and EXTROVERSION on scale ranging from 1 = Not important at all to 5 = Extremely important.

---

3 We separately pretested the relevance of specific behaviors and listed them in Experiment 1 from most to less relevant. One hundred and thirty-two online and lab participants pre-tested the specific behaviors following an unrelated experiment. For each trait, participants read a list of ten behaviors (eleven for introversion) and rated five that best captured their idea of the trait. We included the five most frequently rated either a “1” or “2” in terms of highest relevance.
Results

Importance Manipulation Check

Seven participants failed the manipulation check in the high importance condition by rating introversion as not at all important. Five participants in the low importance condition failed the check by rating introversion as very important. These 12 participants were excluded from analyses. Including them did not materially alter the results on the dependent measures reported below.\(^4\) The importance manipulation was effective: participants in the high importance condition rated introversion as more important \((M = 3.05, SD = 0.71)\) than those in the low importance condition \((M = 2.58, SD = 0.92)\), \(t(195.43) = 4.10, p < .001, d = 0.57\), correcting for unequal variance.

Effects of Importance and Criteria Specificity on BTA Beliefs

In aggregate, participants displayed overconfidence on vague measures (BTA beliefs). Participants believed that they were more introverted \((M = 3.23, SD = 1.62)\) than others \((M = 2.77, SD = 0.90)\), paired \(t(199) = 3.5, p = .001, d = 0.35\). In order to compare the magnitude of BTA beliefs between importance conditions, we subtracted ratings of others from self-ratings for each condition. A 2 (Importance: high, low) X 2 (Criteria specificity: vague, specific) between-subjects ANOVA yielded significant main effects of both importance, \(F(1, 196) = 18.37, p < .001, \text{partial } \eta^2 = .09\), and criteria specificity, \(F(1, 196) = 3.93, p = .049, \text{partial } \eta^2 = .02\),\(^5\) and, most importantly, a significant interaction, \(F(1, 196) = 5.57, p = .019, \text{partial } \eta^2 = .03\). As

\(^4\) Including these participants in the analyses produces a non-significant effect of importance on the manipulation check (high: \(M = 2.91, SD = 0.87\); low: \(M = 2.69, SD = 1.03\)), \(t(209.23) = 1.70, p = .089, d = 0.23\), correcting for unequal variances. The number of participants who failed the manipulation check did not differ between the mTurk and lab samples, \(p = .306\), correcting for unequal variances.

\(^5\) Including all participants in this analyses produces a marginally significant main effect of criteria specificity, \(F(1, 208) = 3.86, p = .051, \text{partial } \eta^2 = .02\).
Figure 1 shows, high importance was associated with significantly higher BTA beliefs within a subjective performance domain, $F(1, 196) = 20.35, p < .001$, partial $\eta^2 = .09$, but this effect was fully attenuated when introversion was explicitly defined, $F(1, 196) = 2.03, p = .156$, partial $\eta^2 = .01$. Results held with ratings of self and others as a repeated measure.

**Discussion**

Within the subjective domain of personality traits, we found an effect of importance on BTA beliefs for introversion, which are elicited on vague measures, but only when introversion was vague and not when its criteria were specified. We replicated Dunning et al.’s (1989) effect of specificity on BTA beliefs themselves and built on this work by measuring how specificity influenced the casual relationship between motivation and BTA beliefs. In Experiment 2, we tested whether motivation also influences overconfidence when assessments were made on specific measures, allowing us to calibrate BTA beliefs against an individual benchmark.

**Experiment 2: Specifying Measures of Assessments for Introversion**

Experiment 2 tests whether the influence of motivation on overconfidence within a subjective domain depends on the specificity of the assessment measure. Whereas Experiment 1 found an effect of criteria specificity on BTA beliefs, in Experiment 2 we varied the specificity of assessment measures for a vague trait. While self-serving interpretations of a trait could flourish to produce flattering self-perceptions, we predicted that when BTA ratings were calibrated against a specific mode of assessment, these overly-generous self-ratings would diminish. Specifically, we compared people’s assessments of themselves and others to actual scores on an introversion questionnaire, thus measuring both BTA beliefs and overplacement—the direct measure of bias in relative self-perception.

**Method**
Participants

Three hundred and twenty-one people (142 women, 179 men; $M$ age = 32, $SD = 11.44$) completed two ostensibly unrelated surveys and a questionnaire via Amazon’s Mechanical Turk (compensated with $0.50$). We estimated that a sample size of two hundred and seventy-nine was necessary to detect a medium effect ($f = 0.25; d = 0.5$) with 95% power.\footnote{Due to seemingly low response rates over the summer, we re-posted the survey with an updated time for completing it based on past responses. At the time, Mechanical Turk did not cancel in-progress surveys and only provided a count for completed surveys. We underestimated the number of in-progress surveys, making our sample larger than anticipated.}

Design

The experiment had a 2 (Importance: high, low) X 2 (Order: questionnaire first, last) between-subjects design such that people estimated their own and others’ score on a questionnaire before or after they read the importance manipulation. As in Experiment 1, we manipulated motivation by manipulating the importance of introversion and measured BTA beliefs directly following the manipulation. Also like Experiment 1, we measured BTA beliefs as the difference between participants’ assessments of themselves and others. Additionally, comparing self-assessments with actual scores on an introversion questionnaire allowed for a measure of overplacement: the degree to which BTA beliefs represent individual bias. Thus, Experiment 2 included both vague and specific measures of assessment.

Procedure and Materials

The procedure and materials were the same as in Experiment 1 except for two major differences. First, participants rated themselves on vague traits only—we did not attempt to specify what introversion meant by enumerating its specific behaviors for them. Instead, participants completed McCroskey’s (1997) 18-item introversion questionnaire. Following the
questionnaire, participants estimated their own and others’ numerical introversion scores on the questionnaire. Half the participants completed the questionnaire before the motivation manipulation and BTA measure and half took it after. The questionnaire contained “18 statements that people sometimes make about themselves.” Participants answered how much each statement applied to them with a yes, undecided, or no (3, 2, and 1 respectively). Directly following the questionnaire, participants read that higher scores indicated higher introversion and estimated their own and others’ scores.

**Overplacement Measure.** To determine actual scores, we followed the scoring rule that accompanied the questionnaire: we subtracted the sum for the extroversion items from the sum for the introversion items plus 40. Following this calculation, the range of scores on the questionnaire extended from 12 to 36. Participants estimated their own and others’ scores. Within each condition, we subtracted the difference between participants’ own and others’ actual scores from the difference between the scores they estimated for themselves and others.

**Results**

**Importance Manipulation Check**

We excluded 14 participants from the high importance condition who failed the manipulation check by rating introversion as not at all important and three in the low importance condition who rated introversion as very important. Including these 17 participants neither affected the significance of the manipulation check nor dependent measures. The importance manipulation was effective: participants in the high importance condition rated introversion as more important ($M = 2.97, SD = 0.95$) than those in the low importance condition ($M = 2.49, SD = 0.91$), $t(302) = 4.48, p < .001, d = 0.52$. 
Effects of Importance and Specificity of Measure on Overconfidence

In aggregate, participants displayed overconfidence on the vague (BTA) but not specific (overplacement) measures. Participants believed that they were more introverted ($M = 3.50$, $SD = 1.19$) than others ($M = 2.62$, $SD = 0.78$) on the BTA measures, paired $t(303) = 10.35$, $p < .001$, $d = 0.87$, but did not believe they were more introverted than others ($M = 1.06$, $SD = 8.13$) than they actually were ($M = 0.03$, $SD = 6.54$) on the overplacement measures, paired $t(303) = 1.87$, $p = .063$, $d = 0.14$. We were interested in how the vague and specific measures compared with each other, and thus standardized each one by z-scoring it. We submitted ratings to a 2 (Importance: high, low) X 2 (Order: questionnaire first, last) X 2 (Specificity of measures: vague, specific) mixed ANOVA with specificity as a repeated measure. The results showed neither main effects nor 2-way interactions, $ps > .087$, nor a 3-way interaction between importance, order, and specificity, $p = .772$. The last order condition provided an opportunity to observe the effect of the manipulation on both the vague and specific measures, as both measures follow the manipulation in this condition. A simple effects analysis within the last order condition showed that, as in Experiment 1, participants displayed stronger BTA beliefs in the high than low importance condition, $F(1, 300) = 10.22$, $p = .002$, partial $\eta^2 = .03$. However, overplacement did not differ between importance conditions, $F(1, 300) = 2.80$, $p = .095$, partial $\eta^2 = .01$. See Figure 2. Overplacement was suppressed in both the high, one-sample $t(75) = 1.96$, $p = .054$, and low importance conditions, one-sample $t(56) = -.75$, $p = .458$.

Effect of Importance on Responses to the Introversion Questionnaire

Although the questionnaire is not a purely objective measure of introversion, as it is self-report, it served to clarify and quantify what introversion meant. For this reason, we checked whether our manipulation influenced actual scores. A 2 (Importance: high, low) X 2 (Order:
questionnaire first, last) between-subjects ANOVA produced a significant interaction between importance and order, $F(1, 300) = 6.53, p = .011, \text{partial } \eta^2 = .02$. A simple effects analysis showed that before the manipulation, there was no significant difference in scores between the importance conditions, $p = .094, \text{partial } \eta^2 = .01$. After the manipulation, participants scored non-significantly higher on introversion in the high than low importance condition, $F(1, 300) = 3.72, p = .055, \text{partial } \eta^2 = .01$.

**Discussion**

Experiments 1 and 2 tested the relationship between motivation and overconfidence in the subjective domain of personality traits. Personality traits, as usually studied, are vague enough that they allow people to construe the meaning of a trait in a way that provides little verifiability of assessment. Indeed, our importance manipulation even appeared to affect how participants reported their own introversion on the introversion personality scale. It is possible that high perceived importance of introversion led people to claim more introversion because they were trying to manage impressions of the experimenter. However, it is more likely that the importance manipulation influenced the way participants thought about what it meant to be introverted and identified more ways in which their behavior could qualify as introverted.

Within the subjective domain of traits, motivation affected overconfidence on vague (BTA) but not specific measures (overplacement) in Experiment 2. This result suggests that the specificity of self-assessment measures may suppress an effect of motivation on overconfidence, even within a subjective domain, and that the influence of motivation on overconfidence is tenuous. However, this effect is not strong enough to produce a significant two-way interaction between motivation and specificity. Experiment 3 moves beyond the subjective domain of traits
to understand motivation’s effect on overconfidence within a more objective domain, performance on a test.

**Experiment 3: Specifying Measures of Assessment for Test Performance**

Results of the first two experiments provided some evidence for a causal effect of motivation on overconfidence: when people perceived an individual trait as important, they rated themselves higher on this trait, relative to others. However, we also found that defining the trait more specifically or using objective, verifiable measures could reduce this effect. In Experiment 3, we tested whether the same patterns existed in assessments of objectively verifiable performance. Unlike judgments of personality traits, to which interpretation and subjectivity are inherent, task performance is objectively verifiable and measurable. Therefore, if the patterns we observed in Experiments 1 and 2 hold, the influence of motivation on overconfidence should weaken. In Experiment 3, participants completed a math and logic quiz and assessed their performance on it, using both vague and specific measures.

In this experiment, we manipulated both intrinsic and extrinsic motivation. We varied intrinsic motivation by describing the implications of performance on the quiz in terms of one of three levels of importance. We experimentally crossed importance of the performance with extrinsic motivation in the form of monetary incentives. As monetary incentives apply to performance, but not to possessing traits, this experiment allowed us to understand their unique influence on the effects we documented in previous studies. Furthermore, as monetary incentives are among the most commonly employed motivational tools by corporations, governments, and economic policies, it is worth separately examining the role of monetary incentives as a motivator.

**Method**
Participants

One hundred and eleven students and staff at a university in the eastern United States (51 women, 60 men; $M_{age} = 27, SD = 11.23$) completed this experiment. The sample size was based on the number of participants we expected to recruit in five experimental sessions and was determined prior to data analysis.

Design

The experiment had a 3 (Importance: high, medium, low) X 2 (Monetary Incentive: present, absent) between-subjects design. The main dependent variables included BTA and overplacement measures.

Procedure and Materials

Participants completed a 10-item test. They estimated their own and others’ performance before and after taking the test.

Motivation manipulations. We administered two types of motivation manipulations. Half of the participants received no monetary incentive and half received a chance to win a $50 prize, with entry into the raffle proportional to the number of correctly answered questions. We orthogonally manipulated the test’s perceived importance. Participants in the high importance condition read that they would take an intelligence test. This was followed by a detailed description of positive, consequential life outcomes periodicals have associated with high intelligence. Participants in the medium importance condition read that they would take a short version of an IQ test and those in the low importance condition read that they would pretest materials for use in another experiment (see Appendix for a link to survey materials).
Motivation manipulation check. In order to assess their motivation to perform well prior to the test, we asked participants to rate how motivated they were, how important it was for them to perform well, and how hard they expected to work from 1 = Not at all to 5 = Extremely.

BTA measures. Participants assessed their own and others’ performance on the test, both before and after taking it, from 1 = Very badly to 5 = Very well.

Overplacement measure. Participants also estimated their own and others’ scores on the test from 0 to 10. We counterbalanced the order in which participants responded to the BTA and overplacement measures.

Results

Motivation Manipulation Check

We averaged the three manipulation check items together to form one measure of motivation (alpha = .83) and submitted it to a 3 (Importance: high, medium, low) X 2 (Monetary Incentive: present, absent) between-subjects ANOVA. There was a main effect of importance on participants’ ratings of their motivation to succeed on the task, $F(2,105) = 5.40, p = .006, \text{partial } \eta^2 = .09$, with mean group ratings corresponding to the level of importance (high: $M = 3.98, SD = 0.68$; medium: $M = 3.50, SD = 0.89$; low importance: $M = 3.42, SD = 0.89$). Neither a main effect of monetary incentive on self-reported motivation, nor an interaction with our importance manipulation was observed, $Fs < 1.89; ps > .15$.

Effects of Motivation on BTA Beliefs and Overplacement

In aggregate, participants did not display BTA beliefs. Participants predicted that they would perform no better than others before the test, paired $t(110) = 0.70, p = .488$, and afterwards believed that they had performed worse ($M = 3.05, SD = 1.00$) than others ($M = 3.32, SD = 0.75$), paired $t(110) = -2.77, p = .007, d = -0.31$. There was minimal evidence of
overplacement before the test but no evidence of it after. People predicted that they would perform better than others ($M = 0.44, SD = 1.58$) than they actually did ($M = 0.00, SD = 1.84$) before the test, paired $t(111) = 2.23, p = .028, d = 0.26$, but not after (predicted: $M = -0.02, SD = 1.84$; actual: $M = -0.16, M = 2.01$), paired $t(110) = -0.94, p = .349, d = 0.07$. Note that one participant did not estimate his or her own score after the test.

Our main test was the effect of the importance and monetary incentives on BTA beliefs. We created the same BTA measure as in Experiments 1 and 2, subtracting self-ratings from ratings of others. We submitted BTA beliefs to a 3 (Motivation: high, medium, low) X 2 (Monetary incentive: present, absent) between-subjects ANOVA. Neither motivation nor monetary incentives affected BTA beliefs individually nor through an interaction before the test, $Fs < .35; ps > .70$. This held after the test as well, $Fs < .26; ps > .77$. The objectivity and verifiability of performance assessment appeared to suppress overplacement.

As in Experiment 2, we created difference measures, “estimated score difference” and “actual score difference,” to create a measure of overplacement. When we submitted the overplacement measure to the same 3 X 2 ANOVA, again, neither motivation nor monetary incentives showed individual effects nor an interaction either before the test, $Fs < 0.23, p > .72$, or after the test, $Fs < 1.23, p > .28$. Results all held when estimated and actual differences were analyzed as a repeated measure.

**Effects of Motivation on Performance**

We submitted actual performance to the same 3 X 2 ANOVA. The results revealed neither a main effect of motivation nor monetary incentive, $Fs < 0.60; ps > .44$ but did reveal a significant interaction, $F(2,105) = 4.21, p = .018, partial \eta^2 = .07$. As Figure 3 illustrates, in the absence of a monetary incentive, high motivation improved performance. However, the positive
effect of motivation on performance was eliminated or even reversed with the addition of a monetary incentive. This curious result might be attributable to people “choking” on an important task with money at stake (Ariely, Gneezy, Loewenstein, & Mazar, 2005; Benoît, Dubra, & Moore, 2015).

**Discussion**

Motivated overconfidence appears to heavily depend on subjectivity and ambiguity. In Experiments 1 and 2, specific definitions and measures effaced the effect. In Experiment 3, we found no effect of motivation on overconfidence in a performance domain, in which performance was objectively verified. In fact, we found little evidence of overconfidence whatsoever, regardless of the motivation manipulations. Despite assessing performance on verbally labeled BTA measures, people expected to perform no differently from others before the test and believed they had performed worse afterwards. Overplacement measures likewise revealed modesty. On average, people only slightly overplaced their scores relative to others prior to the test and did not overplace at all afterwards. The objectivity of the domain itself may have fostered modest self-perceptions by increasing perceived accountability. In Experiment 4, we return to the subjective domain of traits with vague measures, where motivational effects exhibited greater variance, to test a potential mechanism of the effect of motivation on BTA beliefs.

**Experiment 4: Idiosyncratic Construals**

How does vagueness allow for the construction of overly favorable self-perceptions? People may agree on a trait’s meaning but take advantage of vague standards to enhance their self-perceptions. Alternatively, they may base assessments on idiosyncratic criteria in the absence of specifically-defined ones, and construct these criteria in a way that emphasizes their
relative strengths. In Experiments 1 and 2, vagueness may have allowed for idiosyncratic construals of introversion, such that our manipulation of importance influenced those construals in a way that affected BTA beliefs. Experiment 4 examines the relationship between the BTA effect and the specificity of the trait—honesty. We tested whether people relied on idiosyncratic criteria or whether they agreed about what honesty means but indulged in rosy self-perceptions. A within-subjects paradigm also allowed us to examine whether or not people seemed aware of their application of idiosyncratic criteria.

We asked participants to assess their own and others’ honesty before and after specifying what honesty meant to them by rating the relevance of different dimensions of honesty to their definitions of the trait. While a vague attribute may allow for self-serving definitions and flattering self-perceptions, specifying the dimensions of the attribute should reduce idiosyncratic construals. Still, honesty is a complex trait and we measured whether people weighted the specific criteria differently from each other when given the opportunity to construct their own conception of honesty. Relative to when a trait was specified through behaviors which were implicitly weighted equally in terms of relevance to honesty, we expected stronger BTA beliefs both when people considered honesty as a vague trait (phase 1) and again when participants could adjust criterion weights (phase 3).

Method

Participants

7 A pre-test identified the ten behaviors most strongly associated with honesty. We surveyed eighty-seven students on the campus of a West Coast University and thanked them with candy. Participants read thirty-three behaviors and rated the five which were most relevant to honesty. Ten of these behaviors were rated within the top five for more than 50% of participants and thus comprised the list of behaviors we employed in Experiment 4.
One hundred and thirty-six undergraduate students at a West Coast university completed one fifteen-minute session for either credit or pay. We recruited as many participants as the end of the semester allowed prior to analyzing the data.

**Design**

The experiment had a 2 (Target: self, others) x 3 (Phase) within-subject design. The phase manipulation involved assessing BTA beliefs from each participant under three different conditions: (1) their own and others’ honesty, without clarifying what honesty meant; (2) the frequency at which they practice eleven specific honesty-related behaviors; and (3) the relevance of each of these behaviors to their own definition of the trait. We assessed BTA beliefs for each phase by comparing self and other ratings and then compared the magnitude of BTA beliefs between phases. To determine whether people defined honesty in a self-serving manner, we measured the correlation between how frequently people reported practicing each behavior with the relevance they assigned it for their definition of honesty.

**Procedure and Materials**

**Phase 1: Vague traits.** Participants first rated how well the following ten vague traits described them: honest, kind, responsible, intelligent, competent, secure, conscientious, agreeable, imaginative, and outgoing on a scale from 1 = Does not describe me at all to 9 = Describes me very well. Then, they rated the average study participant on the same ten traits.

**Phase 2: Specific behaviors (consensus definition).** On the next page, participants reported how often they engaged in ten honesty-related behaviors, plus “other aspects of honesty not captured in the list above,” on a scale from 0% = I never do this to 100% = I always do this. After rating how frequently they engaged in each behavior, participants estimated their own and others’ overall honesty, as defined by the specific behaviors on the list. Because relevance of the
behaviors was not allocated differentially across the behaviors in this phase, this assessment encouraged people to implicitly consider the behaviors as equally important (a consensus definition).

**Phase 3: Relevance of behaviors to honesty (idiosyncratic definition).** In the third and final phase of the experiment, we explicitly re-introduced the opportunity to define honesty in a more personal way. Participants reported how relevant each specific behavior from phase 2 was to their interpretation of honesty on a scale from 0 = *Not at all important* to 100 = *Most important*. After rating each of the behaviors in terms of their relevance to honesty, participants used the weights (an idiosyncratic definition) to assess their own and others’ honesty on a scale from 0% to 100%. They rated both other students in general and the average participant in the study.

**Results**

**BTA Beliefs by Phase**

We first re-scaled phase 1 ratings to a 0 to 100 scale in order to make them comparable with those in phases 2 and 3. Participants expressed BTA beliefs in each phase. In phase 1, participants rated themselves as more honest ($M = 79.52, SD = 16.21$) than others ($M = 68.80, SD = 14.30$), paired $t(135) = 7.43, p < .001, d = 0.70$. In fact, they displayed BTA beliefs for every trait, $ps \leq .001$. In phase 2, participants rated their own and others’ honesty based on how frequently they thought they enacted the specific behaviors. Although they still rated themselves as more honest ($M = 78.07, SD = 15.39$) than others, ($M = 74.07, SD = 13.29$), paired $t(135) = 3.68, p < .001, d = 0.28$, the effect was weaker than in the previous phase, as evidenced by the smaller effect size. When participants applied their own idiosyncratic weights to the behaviors
in phase 3, the effect strengthened again (self: $M = 82.39$, $SD = 12.14$; other: $M = 74.90$, $SD = 13.87$), paired $t(135) = 7.27, p < .001, d = 0.57$.

**Effect of Phase on BTA Beliefs**

To examine BTA beliefs between phases, we submitted the standardized scores to a 2 (Target: self, others) X 3 (Phase) repeated measures ANOVA. There was a main effect of target, $F(1, 135) = 72.06, p < .001$, partial $\eta^2 = .35$, a main effect of phase, $F(2, 134) = 10.47, p < .001$, partial $\eta^2 = .14$, and, importantly, an interaction between phase and target, $F(2, 134) = 10.11, p < .001$ partial $\eta^2 = .13$. A simple effects analysis confirmed the above paired t-test results, that participants displayed BTA beliefs in each of the three phases, $ps < .001$. To directly compare BTA beliefs between phases, we submitted BTA beliefs to a repeated measures ANOVA with phase as the repeated measure. BTA beliefs were stronger in phase 1 when participants assessed honesty as a vague trait than when they considered specific behaviors in phase 2, $p < .001$. BTA beliefs were also stronger in phase 3, when participants assessed honesty based on their own definitions, than phase 2, $p < .019$. The difference in BTA beliefs between phase 1 and 3 was not significantly different, $p = .094$. 8 See Figure 4.

**Idiosyncratic Definitions of Honesty**

If each person thought that different behaviors related to honesty, then, according to their own definitions, each person could have correctly believed they were more honest than others. For each behavior, we computed a correlation between how frequently people claimed to practice it and how relevant they thought it was to honesty. The frequency and relevance ratings

---

8 Specificity affected ratings of others, such that in phase three, participants considered the average Experiment participant more honest ($M = 7.75$, $SD = 1.25$) than their fellow students in general ($M = 7.40$, $SD = 1.26$), $t(135) = 4.87, p < .001, d = 0.28$. This kinder assessment of the average participant implies a conservative test of BTA beliefs in phases 1 and 2.
correlated for each behavior, \( r_s > .25, p_s \leq .010 \). See Table 4. We are cautious to conclude from this correlational result that people weighted the relevance of behaviors in a self-serving manner; we cannot rule out the possibility that the more relevant people thought behaviors were to honesty, the more frequently they enacted them, and that people were aware that others had their own idiosyncratic construals.

We examined whether these seemingly self-serving definitions corresponded with self-perceptions of honesty. We multiplied frequency ratings by relevance ratings for each behavior, summed the product across behaviors, and correlated the product with self-assessments of honesty in phase 3. It appeared that as definitions became more flattering, so did self-assessments, \( r(136) = 0.68, p < .001 \). We also tested how similar definitions of honesty were in phase 3. For each participant, we computed the relative relevance of each behavior to the other behaviors and standardized the ratings to total 100%. We measured the correlation of each participant’s relevance ratings with every other participants’ ratings in order to assess their similarity. The five participants who did not provide any variance in their ratings, with each behavior rated as 10% relevant, were dropped from the analysis. The average of these correlations was low, \( r = .086 \), which suggests that people did not share one single definition of honesty.

**Discussion**

The results of Experiment 4 suggest that a vague trait allows people to rely on idiosyncratic criteria, which are related to positive self-perceptions. Utilizing specific criteria, people created definitions of honesty that appeared self-serving. Furthermore, the strength of BTA beliefs did not differ when assessments were based on a vague trait or on personal
definitions of the trait. This result suggests that people used idiosyncratic definitions when they originally assessed themselves on a vague trait.

Second, people appear capable of moderating their own BTA beliefs when a vague trait is clarified. BTA beliefs were stronger when the same people assessed their honesty as a vague trait rather than based on specific criteria. This result implies that specifying a trait can help people reduce BTA biases. A potential explanation for this result is that people felt the need to at least partially curb their flattering self-perceptions when they were reminded about behaviors that they may not enact. Although people were capable of moderating their BTA beliefs when assessments were specified, they also created what appear to be self-serving definitions for vague traits which related to more positive self-assessments.

**General Discussion**

The belief that we are better than others is pervasive. Our results help explain how people construct these self-perceptions. Although motivation appears to have a tenuous effect on overconfidence across our experiments, our results identify moderators of that relationship. The subjectivity of the domain or task itself can influence verifiability of assessments and appear to influence the relationship between motivation and overconfidence.

In Experiments 1 and 2, people displayed stronger BTA beliefs on a vague measure when they were motivated to view themselves as introverted. The specificity of criteria moderated the effect of motivation on overconfidence in Experiment 1 and the specificity of the measure moderated the effect in Experiment 2. When the domain itself was objective and verifiable, as was test performance in Experiment 3, the effect again disappeared. In fact, the unambiguous domain appears to have suppressed overly flattering self-assessments altogether.
The results of Experiment 4 provide new insight into the psychological mechanisms behind the construction of subjective self-perceptions. Although people were capable of decreasing their BTA biases when criteria were made specific, idiosyncratic definitions also contributed to BTA beliefs. We cannot distinguish the degree to which these idiosyncratic trait definitions are the result of self-serving definitions or whether people simply work to enact those honesty-relevant behaviors they regard as most important. However, the results from Experiment 1 suggest that trait definitions were selected in self-serving ways, as reduction in biased beliefs about one’s introversion was observed after clarifying not only the trait’s measurement, but also what it means to be introverted. Moreover, Experiment 2’s manipulation of the perceived importance of introversion affected how people responded to an introversion questionnaire, suggesting it changed their construal of their own behavior.

**Theoretical Implications**

This paper contributes to the research on self-perception by providing an empirical examination of a widespread assumption in the field. Our results suggest that motivation affects overconfidence only in subjective contexts, which can help us better understand past correlational work on the relationship. In fact, extant literature rarely strays from subjective performance domains, examining motivation and overconfidence for vaguely-defined traits and abilities (Brown, 2012; Dunning, 1995; Dunning et al., 1989; Alicke, 1985; Mazar, Amir, & Ariely, 2008). Another contribution of this work is provided by the measure of overplacement, which compares BTA beliefs with reality, thus capturing individual bias rather than bias at the level of the sample. Examining different measures of bias is important to understand the psychology behind self-perceptions (Dunning & Helzer, 2014).
Examining the relationship between motivation and overconfidence likewise contributes to work on self-enhancement. Self-enhancement is the tendency to hold positive views of oneself. The term self-enhancement is regularly used to describe flattering self-perceptions, regardless of its accuracy. Some work has offered a motivational explanation for self-enhancement, similar to the proposed effects on overconfidence (Dunning, 2005; Gosling, John, Craik, & Robbins, 1998; Greenwald, 1980; Kunda, 1990; Sedikides & Gregg, 2008).

We expand these findings by directly measuring the extent to which people’s self-ratings are consistent with reality, as well as how they are affected by motivation. We should note that a motivational account of overconfidence differs from how researchers have measured “wishful thinking.” Wishful thinking has often been studied by manipulating desirability and measuring the perceived likelihood of future events (e.g., Lench & Ditto, 2008; Marks, 1951; Windschitl, Scherer, Smith, & Rose, 2013; Windschitl, Smith, Rose, & Krizan, 2010). Wishful thinking, like self-enhancement, need not benchmark against accuracy as overconfidence does. Researchers have compared participants’ estimated probabilities for positive and negative outcomes, rather than to actual likelihoods or outcomes. Furthermore, experimental evidence suggests that motivation does a poor job explaining empirical evidence of wishful thinking (Bar-Hillel & Budescu, 1995; Bar-Hillel, Budescu, & Amar, 2008; Krizan & Windschitl, 2007; Vosgerau, 2010).

**Practical Implications**

Are overconfident beliefs self-serving? For them to qualify as such, holding overconfident beliefs would have to benefit the individual holding them. However, it is easy to identify risks of overconfidence. Overconfidence, after all, can impair both performance and well-being. Overconfident people risk too much (Camerer & Lovallo, 1999; Odean, 1998). And
while there may be some pleasure in savoring a bright future (Loewenstein & Prelec, 1993), those who are most confident in their performance, and who therefore believe they need not try hard, can actually perform worse (Stone, 1994; Vancouver, Thompson, Tischner, & Putka, 2002). For instance, the employee who is overconfident about his presentation skills and therefore does not believe he needs to practice is unlikely to perform best.

Overconfidence in ones capabilities invites disappointment when reality turns out worse than expected (McGraw, Mellers, & Ritov, 2004; van Dijk, Zeelenberg, & van der Pligt, 2003). People seem aware of the disappointment that follows overconfidence when they display defensive pessimism. In fact, people who lower their expectations through defensive pessimism enjoy their success as much as optimists but are not as distraught by failure (Norem & Cantor, 1986). If self-flattering beliefs are self-interested, then people should display overconfidence in all of the domains they value. Yet, people often display underconfidence in domains they think are important (Blanton, Axsom, McClive, & Price, 2001; Kruger, 1999; Moore, 2007; Windschitl, Kruger, Simms, 2003), including what most everyone desires: prominence, respect and influence (Anderson, Srivastava, Beer, Spataro, & Chatman, 2006).

Our results suggest that one should not always expect greater motivation to beget greater overconfidence. When performance standards are quantitative and objective, our results imply there to be little motivational effect on overconfidence. How often is this the case? Within the workplace, success often depends on numbers. A company must turn a certain amount of profit to continue functioning, which often depends on the number of clients secured or products sold. However, even if motivation only affects overconfidence in purely subjective contexts, those contexts are not entirely uncommon. Obviously, objective criteria are not always readily available for some consequential outcomes. Mates are rarely chosen based on objective,
verifiable or measureable criteria. Assessment of academic papers depends on subjective assessments made by readers and reviewers. Employee evaluations are, to a great extent, driven by the subject assessment of the manager. Under these circumstances, we expect wider latitude for subjective construal of performance and stronger effects of motivation on overconfident beliefs.

High levels of overconfidence become more likely when one’s goals are not specifically defined, which holds important implications for individuals, managers, and organizations for whom overconfidence may contribute to unmet expectations. Yet, our results are hopeful in that they suggest roadmap towards more accurate self-assessments. Even within vague domains, providing clearly defined criteria for what makes a productive employee, an effective leader, and an efficient team, may help people more accurately calibrate their self-perceptions with reality.

Conclusion

We have sought to test a widely held belief that overconfident beliefs are driven, in part, by the motivation to view oneself positively. We directly manipulated motivation and measured overconfidence, examining what inflates and deflates positive self-perceptions. We found only limited evidence for motivational influence on overconfidence. The most important implication of our findings is the insight into when motivation has an effect and when it does not. Within an objective domain, verifiability of the domain appeared to suppress overconfidence, even on a vague measure. Within a subjective performance domain, motivation had influence when assessments were made on vague but not specific measures, and only when the trait was vague and not when it was clearly defined. Indeed, whenever clarity of criteria, measures, and domains allowed for us to compare self-reports with verifiable truth, we found little evidence of overconfidence.
References


### Tables

**Subjectivity Systematically Varies Across Experiments**

<table>
<thead>
<tr>
<th>Experiment</th>
<th>Domain</th>
<th>Measure</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Subjective (Introversion)</td>
<td>Vague (with vague &amp; specific criteria)</td>
</tr>
<tr>
<td>2</td>
<td>Subjective (Introversion)</td>
<td>Vague &amp; Specific (consensus definition)</td>
</tr>
<tr>
<td>3</td>
<td>Objective (Test)</td>
<td>Vague &amp; Specific</td>
</tr>
<tr>
<td>4</td>
<td>Subjective (Honesty)</td>
<td>Vague &amp; Specific (idiosyncratic definition)</td>
</tr>
</tbody>
</table>
Table 2

*Behaviors by Trait in Experiment 1*

<table>
<thead>
<tr>
<th>Trait</th>
<th>Behavior</th>
</tr>
</thead>
<tbody>
<tr>
<td>Introverted</td>
<td>I do not express my happiness outwardly.</td>
</tr>
<tr>
<td></td>
<td>I work alone when I can rather than with a group.</td>
</tr>
<tr>
<td></td>
<td>I am comfortable with silence.</td>
</tr>
<tr>
<td></td>
<td>I am quiet in large groups of people.</td>
</tr>
<tr>
<td></td>
<td>I think about what I am going to say before I say it.</td>
</tr>
<tr>
<td>Agreeable</td>
<td>I cooperate in most situations.</td>
</tr>
<tr>
<td></td>
<td>I get along well with others.</td>
</tr>
<tr>
<td></td>
<td>I avoid arguments.</td>
</tr>
<tr>
<td></td>
<td>I think about other people's issues.</td>
</tr>
<tr>
<td></td>
<td>I allow people the chance to explain themselves.</td>
</tr>
<tr>
<td>Conscientious</td>
<td>I pay attention to details.</td>
</tr>
<tr>
<td></td>
<td>I am careful when I make decisions.</td>
</tr>
<tr>
<td></td>
<td>I create goals for myself.</td>
</tr>
<tr>
<td></td>
<td>I plan ahead.</td>
</tr>
<tr>
<td></td>
<td>I check my work.</td>
</tr>
<tr>
<td>Imaginative</td>
<td>I find inspiration easily.</td>
</tr>
<tr>
<td></td>
<td>I have a lot of ideas to share.</td>
</tr>
<tr>
<td></td>
<td>I find it easy to think of lots of different kinds of ideas for a project.</td>
</tr>
<tr>
<td></td>
<td>I approach problems differently from most people.</td>
</tr>
<tr>
<td></td>
<td>I am curious about alternate outcomes for everyday situations.</td>
</tr>
<tr>
<td>Outgoing</td>
<td>I seek out social situations.</td>
</tr>
<tr>
<td></td>
<td>I try to connect and develop relationships with most people I meet.</td>
</tr>
<tr>
<td></td>
<td>I put myself in situations where I am likely to meet new people.</td>
</tr>
<tr>
<td></td>
<td>I introduce myself to people I don't know.</td>
</tr>
<tr>
<td></td>
<td>I initiate conversations.</td>
</tr>
<tr>
<td>Honest</td>
<td>If I make a mistake, I own up to it.</td>
</tr>
<tr>
<td></td>
<td>When I make a promise, I keep it.</td>
</tr>
<tr>
<td></td>
<td>I do not say things I know to be untrue.</td>
</tr>
<tr>
<td></td>
<td>I do not purposely deceive others.</td>
</tr>
<tr>
<td></td>
<td>I fulfill my obligations and do what I say I will do.</td>
</tr>
</tbody>
</table>
Table 3

*Frequency of Enacting Behavior in Experiment 4*

<table>
<thead>
<tr>
<th>Behavior</th>
<th>Frequency</th>
</tr>
</thead>
<tbody>
<tr>
<td>I do not cheat on my boyfriend/girlfriend. (5)</td>
<td>89.43</td>
</tr>
<tr>
<td>I fulfill my obligations and do what I say I will do. (4)</td>
<td>86.99</td>
</tr>
<tr>
<td>I do not steal. (8)</td>
<td>86.10</td>
</tr>
<tr>
<td>When I make a promise, I keep it. (1)</td>
<td>86.04</td>
</tr>
<tr>
<td>If I find something of value I do my best to return it to the owner. (10)</td>
<td>83.89</td>
</tr>
<tr>
<td>I live according to my own values. (7)</td>
<td>83.57</td>
</tr>
<tr>
<td>If I make a mistake, I own up to it. (6)</td>
<td>82.50</td>
</tr>
<tr>
<td>(Other) I am honest in ways that the above statements fail to capture.</td>
<td>82.04</td>
</tr>
<tr>
<td>I do not pretend to be something I am not. (9)</td>
<td>76.04</td>
</tr>
<tr>
<td>I do not purposely deceive others. (3)</td>
<td>75.78</td>
</tr>
<tr>
<td>I do not say things I know to be untrue. (2)</td>
<td>74.59</td>
</tr>
</tbody>
</table>

*Note:* Behaviors are listed in order of the magnitude of the better-than-average effect. The number next to the trait is the order in which the behavior was presented to participants.
Table 4

*Correlations between the Frequency and Relevance Weights in Experiment 4*

<table>
<thead>
<tr>
<th>Behaviors</th>
<th>Correlation</th>
<th>p</th>
</tr>
</thead>
<tbody>
<tr>
<td>I do not steal. (8)</td>
<td>0.597</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>I do not cheat on my boyfriend/girlfriend. (5)</td>
<td>0.587</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>If I find something of value I do my best to return it to the owner. (10)</td>
<td>0.576</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>I do not say things I know to be untrue. (2)</td>
<td>0.546</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>(Other) I am honest in ways that the above statements fail to capture. (11)</td>
<td>0.517</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>When I make a promise, I keep it. (1)</td>
<td>0.455</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>I do not pretend to be something I am not. (9)</td>
<td>0.433</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>I do not purposely deceive others. (3)</td>
<td>0.417</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>I fulfill my obligations and do what I say I will do. (4)</td>
<td>0.393</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>If I make a mistake, I own up to it. (6)</td>
<td>0.348</td>
<td>&lt; .001</td>
</tr>
<tr>
<td>I live according to my own values. (7)</td>
<td>0.242</td>
<td>&lt; .01</td>
</tr>
</tbody>
</table>

*Note:* Behaviors are listed in order of the magnitude of their frequency and relevance rating correlation. Numbers in parentheses correspond with the order in which behaviors were presented.
Figure 1. The magnitude of better-than-average beliefs as a function of experimental importance (motivation) and specificity conditions in Experiment 1. Error bars represent standard errors.
**Figure 2.** The magnitude of overconfidence for introversion as a function of experimental importance condition (motivation) and specificity of the measure (vague: better-than-average, specific: overplacement) in Experiment 2, for participants who saw the manipulation before making self-assessments.
Figure 3. Test performance as a function of experimental motivation and monetary incentive conditions in Experiment 3.
Figure 4. The better-than-average effect (self and other difference) within each phase in Experiment 4.
Appendix

Please view anonymized experimental materials, data, syntax, and supplementary analyses at the Open Science Framework web page through this link:

https://osf.io/qayhz/?view_only=8797f615f120438194cb1f526c23fac2