Supplemental Materials for:

The Social Transmission of Overconfidence

ELECTRONIC SUPPLEMENTARY MATERIALS FOR 'THE SOCIAL TRANSMISSION OF OVERCONFIDENCE'

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SUPPLEMENTAL INTRODUCTION

COULD CONFIDENCE IN OTHERS SUPPRESS ONE'S OWN CONFIDENCE? AN ALTERNATE ACCOUNT

Although the cultural transmission account (described in the main text) offers compelling reasons for why one might expect overconfidence to transmit socially, as we described in text there are also theoretically guided reasons to, at the very least, consider the plausibility of an alternative prediction—that not only does confidence resist social transmission, but it may even suppress confidence in others.

This prediction flows directly out of an influential theoretical framework in social and personality psychology commonly referred to as *dominance complementarity*. This theory, which dates to early theorizing in personality research (Carson, 1969; Kiesler, 1983; Wiggins, 1982), proposes that agents in a social interaction engage in a coordinated interpersonal "dance". Complementarity refers to the predictable pattern of response wherein warm and affiliative behaviors are reciprocated with similar reactions from the interaction partner, whereas, dominant and high status behaviors evoke an opposite, reciprocal behavioral pattern that is characterized by submissiveness and deference (and hence termed dominance complementarity).

This elegant framework has gained substantial traction across different sub-disciplines in psychology, including the study of psychopathology and psychotherapy (e.g., DeVogue & Beck, 1978; Leary, 1957; Wiggins, Philips, & Trapnell, 1989), largely as a result of its perceived strengths in providing both parsimonious and comprehensive descriptions of observed behavioral patterns in the natural world. Note, however, that the acceptance of the ideas put forth by dominance complementarity extends beyond our own discipline and is compatible with well-established understanding in other fields. Biologists, for example, have long observed (and modeled mathematically) patterns of dominance-submission in competitive interactions across diverse species, which they term 'ritualized animal contests' (see Bernstein, 1981).

These ideas are beyond theoretical at this point; a large, growing body of evidence confirms the dominance complementarity theory, demonstrating that an individual's behavior affects his/her partner's behavior and psychology in accordance with the principles of complementarity. These include studies showing that expressions of dominance evoke submissiveness in one's interaction partner (Tiedens et al., 2007; Tiedens & Fragale, 2003; Zitek & Tiedens, 2012), complementing (in contrast to non-complementing) relationships foster coordination and success in working relationships (Halevy et al., 2011; Kausel & Slaughter, 2011; Kwaadsteniet & van Dijk, 2010; Ronay et al., 2012; Wiltermuth et al., 2015), and more broadly that applying principles of complementarity to the study of human interactions generates important insights for diverse phenomena (including: understanding the influence of narcissists, effective leadership, and group performance and creativity; Grant, Gino, & Hofmann, 2011; Grijalva & Harms, 2014; Smith, 2012; Wiltermuth, 2009). Collectively, these studies show that much of our behaviors in social interactions (e.g., Tiedens & Fragale, 2003), as well as our mental representations and expectations of how they unfold (Kwaadsteniet & van Dijk, 2010; Zitek & Tiedens, 2012), follow systematic, predictable patterns described by the dominance complementarity framework. All of this work explicitly adopts the dominance complementarity framework and its associated terminology.

What insights and empirical predictions regarding overconfidence emerge from the dominance complementarity framework? Overconfidence, or its demonstration to be precise, is a potent signal of prestige and knowledge, as a number of studies have revealed (Anderson et al., 2012a; Kennedy et al., 2013; Shipman & Mumford, 2011; Zarnoth & Sniezek, 1997). This means that, unlike many other phenomena that have been studied in social transmission research (e.g., cooperative mindset or behavior, or other relatively non-social behaviors and habits), overconfidence might be unique in producing complementary rather than transmission effects. Observing confidence in others—which is a signal of dominance—might actually *suppress* rather than heighten an observer's confidence—because muting one's confidence de-escalates conflict and signals submission. This reasoning, and the existing lines of evidence, thus suggests that overconfidence might not spread, but rather invite the opposite (underconfidence). Put precisely, if an individual in an interaction experiences and displays overconfidence, other members might feel less confident. This effect may be especially pronounced for overplacement, the specific form of overconfidence on which we focus and that involves socially comparing the self with other(s). Given the nature of these social comparisons, an individual who self-assesses to be superior to (i.e., relatively more capable than) all other members present may effectively instill feelings of inferiority among other members.

Despite the plausibility of this alternative account, however, we submit that there are good reasons why overconfidence may lead to social transmission rather than incite complementarity. While dominance complementarity is theorized to arise regularly in circumstances in which effective coordination, exchange, and cooperation are prioritized and antagonism is best suppressed, it is less clear whether divergent beliefs in ability (that results from complementary levels of confidence) vis-à-vis one's social partners and community provide the same coordination advantages and lead to successful cooperation and exchange. Moreover, it is not clear how this hypothesis can explain why there exists substantial similarity within and variation between groups, organizations, and societies, and why social groups possess confidence norms shared and enforced by the local community.

TWO APPROACHES FOR CALCULATING THE DISCREPANCY BETWEEN SELF-ESTIMATED PLACEMENT AND ACTUAL PLACEMENT: DIFFERENCE SCORE AND RESIDUAL SCORE

Prior work on overplacement employs two major approaches to capture the discrepancy between self-estimated placement and actual placement. The first approach uses difference scores, in which actual placement is subtracted from self-assessed placement (Allison, 1990; Rogosa & Willett, 1983). This approach is appropriate for mean comparisons, such as across groups or experimental conditions. The second approach uses residual scores and regresses self-estimated placement on actual placement, retaining the residuals (Anderson et al., 2012b; Cronbach & Furby, 1970; Dubois, 1957; John & Robins, 1994a). These residual scores capture the deviation of expressed estimated placement from expected placement that is rooted in actual placement, and thus represent variability in beliefs that cannot be accounted for by actual capability. This approach is the method of choice for analyses aimed at examining covariation, such as that between overconfidence and another variable (e.g., is partner overconfidence correlated with participant overconfidence), because, in this case, difference scores may be confounded with variables that comprise the discrepancy index (Cohen et al., 2003; Cronbach & Furby, 1970; Griffin et al., 1999; John & Robins, 1994b; Johns, 1981; Lord, 1956; McNemar, 1958; Tucker et al., 1966).

Following these recommendations, throughout this paper we use difference scores to compute the discrepancy index for all tests of mean differences in overplacement, and residual scores for all tests of covariation involving overplacement. In Study 1, given our goal to test whether dyad members' overplacement positively covary, we regressed self-estimated placement before the dyadic component on actual placement, and retained the residuals. We computed post-collaboration overplacement similarly, but instead using self-estimated placement after the dyadic component. These measures of overplacement capture variability in self-assessed rank that actual performance rank cannot account for.

STUDY 1: OVERPLACEMENT SPREADS IN ASSIGNED DYADS IN THE LAB

SUPPLEMENTAL RESULTS

SOCIAL PARTNERS' OVERPLACEMENT CONVERGE AFTER (BUT NOT BEFORE) COLLABORATION: ROBUSTNESS CHECKS

Below we assess the robustness of our key finding reported in the main text that partners converge in their overplacement post-collaboration, but not pre-collaboration. Results indicate that this convergence effect is robust to controls for available observable characteristics.

TABLE S1. OLS REGRESSION OF ACTOR OVERPLACEMENT *PRE*-COLLABORATION ON PARTNER OVERPLACEMENT *PRE*-COLLABORATION (STUDY 1). SUBSEQUENT MODELS CONTROL FOR ACTOR GENDER, DYAD'S JOINT PERFORMANCE ON THE COLLABORATION COMPONENT (CENTERED), AND THE PARTNER OVERPLACEMENT × DYAD'S JOINT PERFORMANCE INTERACTION. VALUES ARE UNSTANDARDIZED REGRESSION COEFFICIENTS FOLLOWED BY 95% CONFIDENCE INTERVAL AND *P*-VALUE IN PARENTHESES. THE KEY RESULTS HIGHLIGHTED IN GRAY INDICATE THAT, BEFORE THE SOCIAL INTERACTION OCCURRED, THERE WAS NO ASSOCIATION BETWEEN ACTOR AND PARTNER OVERPLACEMENT, AS WOULD BE PREDICTED.

	Baseline Model	**Model with Covariates**	**Model with Covariates**	**Model with Covariates**
Partner Overplacement Pre-Collaboration (z-score)	-0.1123	-0.1629	-0.1498	-0.1888
	[-0.38,0.16]	[-0.43,0.11]	[-0.43,0.13]	[-0.48,0.11]
	(0.4038)	(0.2314)	(0.2882)	(0.2025)
Gender (1 = Male)		0.1642	-0.0803	0.1924
		[-8.65,8.98]	[-9.07,8.91]	[-8.84,9.22]
		(0.9702)	(0.9857)	(0.9659)
Dyad Joint Performance in Collaborative Task (centered)			3.5497	4.6865
			[-13.82,20.92]	[-12.91,22.28]
			(0.6823)	(0.5937)
Partner Overplacement Pre-Collaboration × Dyad Joint Performance				-0.5186
				[-1.66,0.62]
				(0.3637)
Constant	-0.3392	-1.2646	-1.1020	-1.6967
	[-4.53,3.86]	[-7.16,4.63]	[-7.11,4.90]	[-7.86,4.46]
	(0.8716)	(0.6674)	(0.7131)	(0.5812)
R^2	0.014	0.032	0.036	0.055
Adjusted R^2	-0.006	-0.012	-0.031	-0.035
AIC	431.5584	390.2324	392.0471	393.1129
BIC	435.4609	395.7828	399.4477	402.3636
Observations	52	47	47	47

+ $p < 0.10$, * $p < 0.05$, ** $p < .01$, *** $p < .001$

TABLE S2. OLS REGRESSION OF ACTOR OVERPLACEMENT *POST*-COLLABORATION ON PARTNER OVERPLACEMENT *POST*-COLLABORATION (STUDY 1). SUBSEQUENT MODELS CONTROL FOR ACTOR GENDER, DYAD'S JOINT PERFORMANCE ON THE COLLABORATION COMPONENT (CENTERED), AND THE PARTNER OVERPLACEMENT × DYAD'S JOINT PERFORMANCE INTERACTION. VALUES ARE UNSTANDARDIZED REGRESSION COEFFICIENTS FOLLOWED BY 95% CONFIDENCE INTERVAL AND *P*-VALUE IN PARENTHESES. THE KEY RESULTS HIGHLIGHTED IN GRAY INDICATE THAT, *AFTER* THE SOCIAL INTERACTION OCCURRED, ACTOR AND THEIR PARTNER CONVERGED IN DEGREE OF OVERPLACEMENT.

	Baseline Model	**Model with Covariates**	**Model with Covariates**	**Model with Covariates**
Partner Overplacement Post-Collaboration (z-score)	0.3938*	0.3680*	0.3696*	0.4177*
	[0.06,0.73]	[0.02,0.72]	[0.02,0.72]	[0.05,0.79]
	(0.0222)	(0.0399)	(0.0397)	(0.0274)
Gender (1 = Male)		2.1477	1.5972	1.3655
		[-6.23,10.52]	[-6.90,10.09]	[-7.17,9.90]
		(0.6078)	(0.7062)	(0.7483)
Dyad Joint Performance in Collaborative Task (centered)			7.0187	3.1137
			[-8.85,22.89]	[-15.11,21.33]
			(0.3773)	(0.7318)
Partner Overplacement Post-Collaboration × Dyad Joint Performance				0.5816
				[-0.74,1.90]
				(0.3783)
Constant	1.3178	-0.4248	-0.1054	-0.0303
	[-2.61,5.25]	[-5.95,5.10]	[-5.69,5.48]	[-5.64,5.58]
	(0.5035)	(0.8775)	(0.9698)	(0.9913)
R^2	0.102	0.102	0.119	0.136
Adjusted R^2	0.084	0.060	0.056	0.051
AIC	415.3736	375.8693	377.0054	378.1239
BIC	419.2372	381.3552	384.3200	387.2671
Observations	51	46	46	46

+ p < 0.10, * p < 0.05, ** p < .01, *** p < .001

ALTERNATIVE EXPLANATION: DID JOINT PERFORMANCE ON THE DYADIC TASK PRODUCE CONVERGENCE IN OVERCONFIDENCE? NO

For our main finding that dyad members converge in overconfidence, one plausible alternative explanation to social transmission is members' joint task performance. This reasoning proposes that, because members share the same performance in the dyadic task, how well they think they had done (they did not receive direct feedback regarding their actual performance) might shape their perception of the difficulty of the task. To illustrate, suppose that a dyad had done well together. Both members may *independently* come to see the task as easy and subsequently become overconfident, producing a positive correlation between their self-assessments. Thus, while this alternative possibility makes the same prediction that partners will show a correlated level of bias, this convergence results from joint exposure to the contextual factor of shared performance, rather than from imitation or any form of social influence. In brief, actually doing well (or poorly) on the dyadic task may have boosted (or diminished) both individuals' confidence, producing similar levels of post-collaboration overconfidence within dyads.

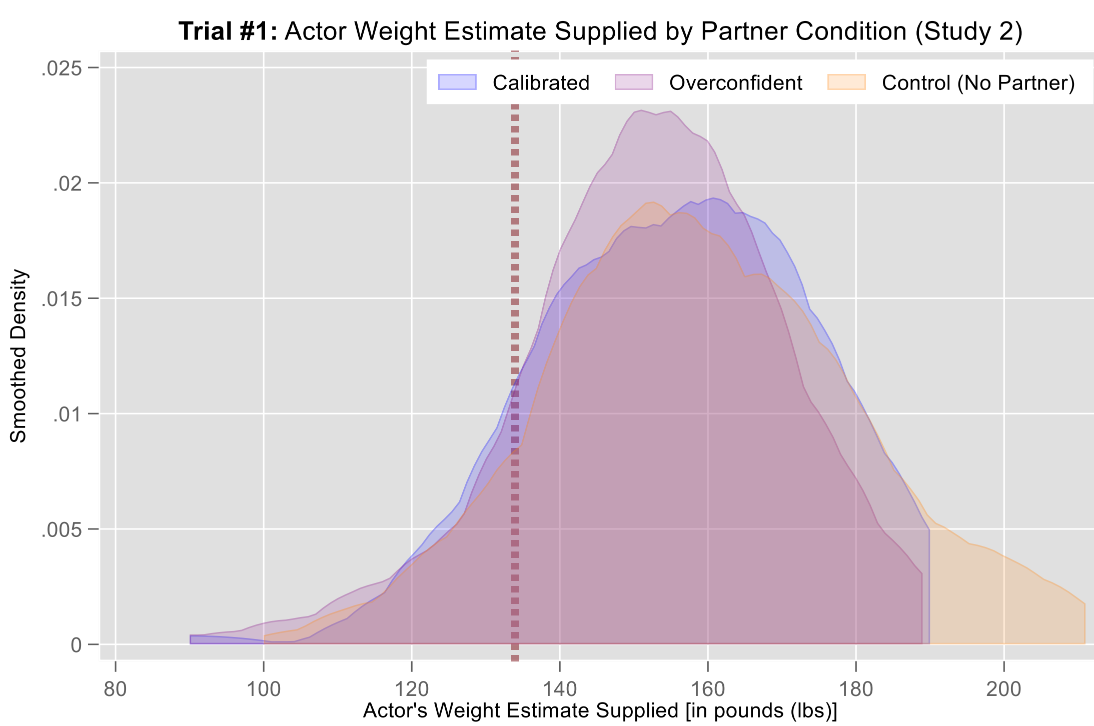
To examine this alternative explanation, we examined whether the association between dyad members' post-collaboration overconfidence differed as a function of the dyad's joint performance in the regressions conducted above (see Table S2). The non-significant main and interactive effects of dyad-level performance indicate that this similarity between partners in overconfidence did not result from better or worse joint performance. Together, this casts doubt on the alternative explanation that joint performance created similarity in overconfidence, and suggests that it is unlikely.

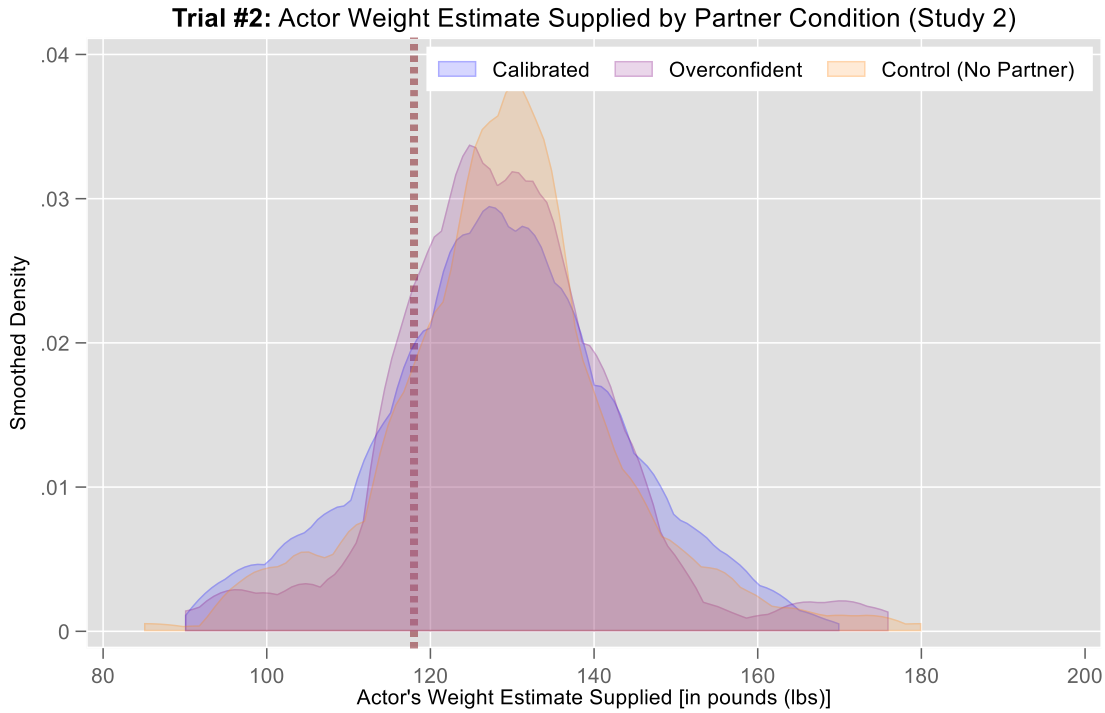
STUDY 2: OVERPLACEMENT SPREADS FROM PERSON TO PERSON

SUPPLEMENTAL RESULTS

WEIGHT ESTIMATE RESPONSE

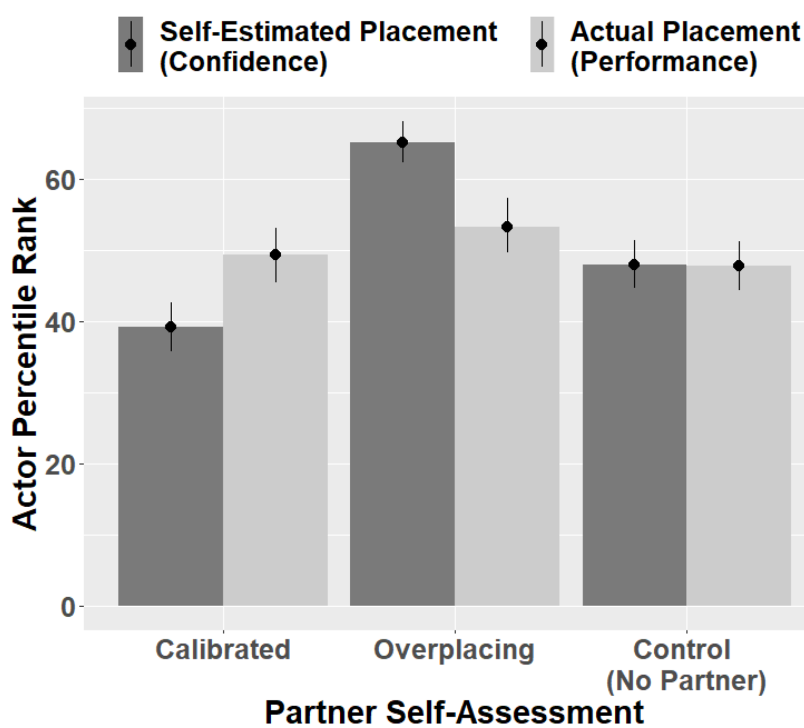
Below, density plots for the weight estimate supplied by actors (i.e., their answer to each trial of the weight-guessing game). These plots show the absence of difference across experimental conditions, suggesting that, while partners shift actors' confidence beliefs, they have little influence over actors' in-game responses and (by implication) performance. The dotted line marks the correct answer (i.e., the actual weight of the individual in the photo).





SELF-ESTIMATED AND ACTUAL PLACEMENT DESCRIPTIVES

Bar graphs showing that individuals exposed to an overplacing partner are more likely to themselves subsequently overplace. Error bars provide 95% CIs.

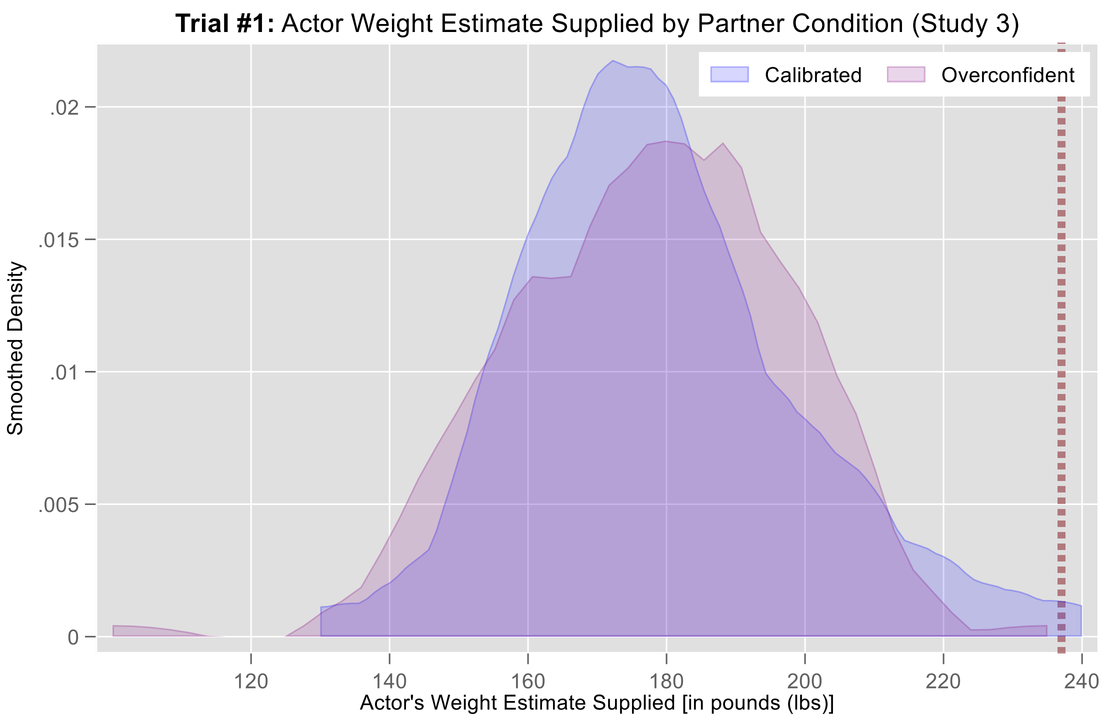


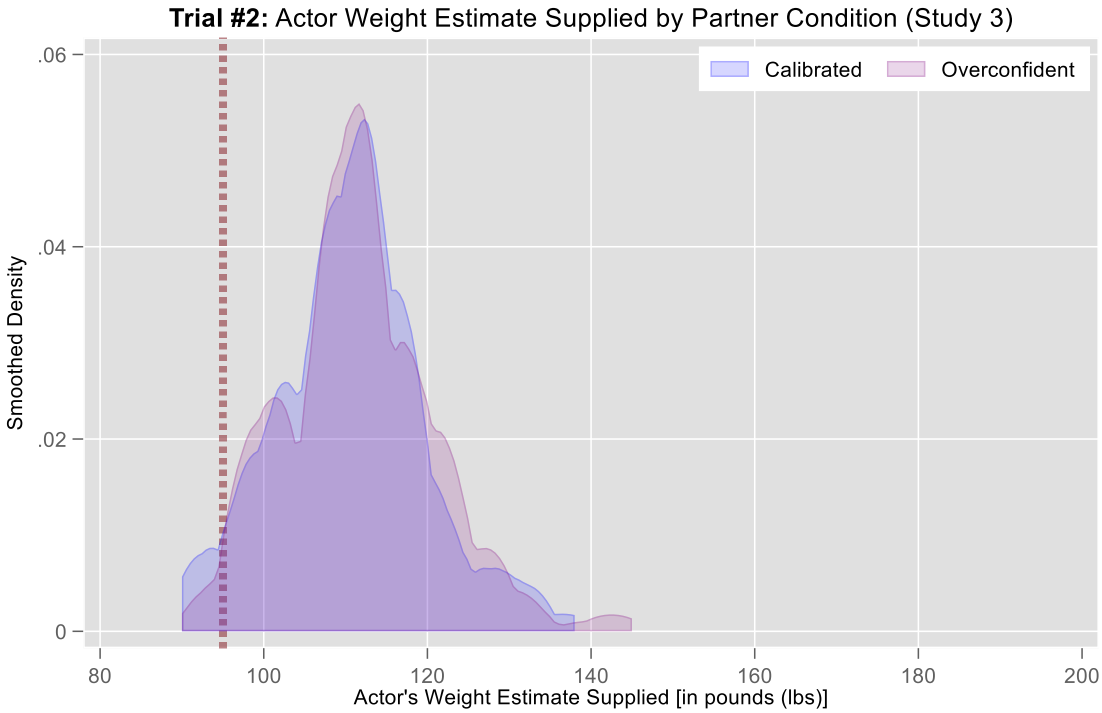
STUDY 3: OVERPLACEMENT SPREADS TO INDIRECT TIES

SUPPLEMENTAL RESULTS

WEIGHT ESTIMATE RESPONSE

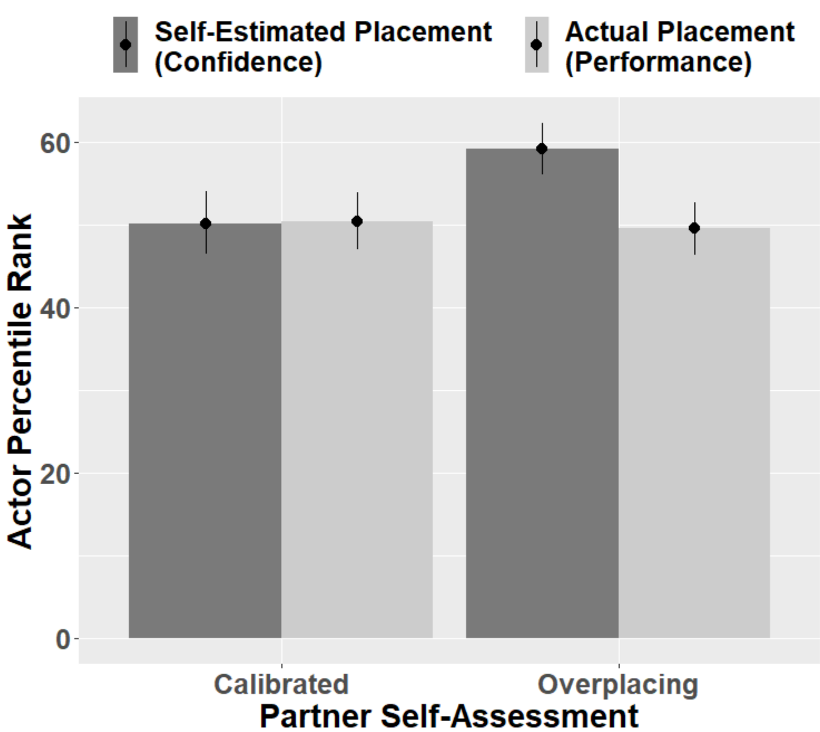
Below, density plots for the weight estimate supplied by actors (i.e., their answer to each trial of the weight-guessing game). These plots show the absence of difference across experimental conditions, suggesting that, while partners shift actors' confidence beliefs, they have little influence over actors' in-game responses and (by implication) performance. The dotted line marks the correct answer (i.e., the actual weight of the individual in the photo).





SELF-ESTIMATED AND ACTUAL PERFORMANCE DESCRIPTIVES

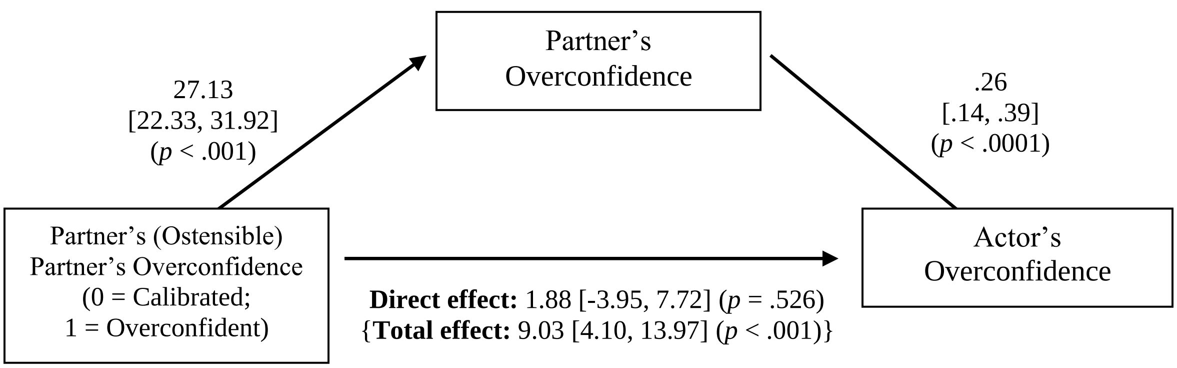
Bar graphs showing that individuals exposed to an overplacing partner are more likely to themselves subsequently overplace. Error bars provide 95% CIs.



DOES PARTNER OVERCONFIDENCE MEDIATE THE RELATIONSHIP BETWEEN PARTNER'S PARTNER OVERCONFIDENCE AND ACTOR OVERCONFIDENCE?

As another check on the proposed indirect spread of overconfidence, we conducted a mediation analysis to ascertain whether the indirect trace of the partner's partner's overconfidence on actors indeed occurred via a chain of direct pairwise effects—namely the effect of the partners' partners on partners, and the subsequent effect of partners on actors. Here, overconfidence was computed using the residual score approach to facilitate tests of associations. The results of this model, depicted in Figure S1 below, confirm this prediction. As expected, bias-corrected bootstrap analyses with 5,000 resamples indicated that partner's overconfidence fully mediated the relationship between the overconfidence (or lack thereof) expressed by the partner's partner and the actor's own overconfidence [indirect effect: *ab* = 7.15, 95% CI (3.34, 10.88)]. Approximately 80% of the total effect of a partner's partner on actors was mediated by the partner. These results are consistent with the predicted cascade network effect of overconfidence transmission.

FIGURE S1. MEDIATION ANALYSIS TESTING THE SOCIAL INFLUENCE OF PARTNER'S PARTNER ON ACTOR VIA PARTNER (STUDY 3). UNSTANDARDIZED REGRESSION COEFFICIENTS ARE FOLLOWED BY CONFIDENCE INTERVALS AND *P*-VALUES. RESULTS CONFIRM THAT AN OVERCONFIDENT PARTNER OF PARTNER LED TO INCREASED OVERCONFIDENCE IN THE PARTNER, WHO SUBSEQUENTLY BRED GREATER OVERCONFIDENCE IN THE ACTOR.



STUDY 4: THE TRANSMISSION OF OVERPLACEMENT FROM OVERPLACING AND (JUSTIFIABLY) CONFIDENT PEERS

SUPPLEMENTAL METHODS

METHODOLOGICAL DIVERGENCE FROM STUDIES 1-3

Beyond addressing these questions, Study 4 sought to increase the internal validity of our prior studies in several ways. To ensure familiarity with the task, actors first completed a number of practice trials before the experimental manipulation, in the form of partner information. In addition, we strengthened our manipulation by increasing the salience of the partner's decisions. Unlike in Studies 2 and 3, wherein actors viewed their partner's responses to two separate photos before they began the task, in Study 4 actors learned of their partner's decisions as they responded in the task trials. In each trial, actors reported their weight estimate and confidence, and then immediately observed the partner's responses to the same photo. This set-up provided actors with substantial information and constant reminders about their partners' cognitive style. Moreover, to derive a more precise estimate of actor overconfidence, we increased the number of task trials from 2 (used in Studies 2 and 3) to 9 and aggregated overconfidence expressed across trials to index overconfidence.

Finally, Study 4 extends the generalizability of our findings. We tested the overconfidence transmission effect in a demographically diverse adult sample that is more representative of the U.S. population. In addition, a manipulation check verified that the partner information elicited the intended perceptions of confidence and ability. Moreover, we removed the financial incentives applied in Studies 2 and 3 that were designed to encourage calibration over overconfidence. As discussed above, despite the utility of these incentives in increasing motivation and creating a context that parallels the real-world in which calibration can be beneficial, they may dampen overconfident responses.

MANIPULATION AND RANDOM ASSIGNMENT CHECK

MANIPULATION CHECK

To verify that our experimental manipulations produced the intended partner perceptions, we administered two self-reported items post-task to measure actors' perceptions of their partner's confidence and ability: "My partner was confident" (M = 3.60, SD = 1.31), and "My partner was competent at the task" (M = 3.89, SD = 1.21). Both were solicited on a 5-point scale (1 = *Disagree strongly*, 5 = *Agree strongly*). One participant did not complete the perceived confidence item.

We then compared actors' ratings of their partner's perceived confidence and ability by regressing these variables (as the outcome in two separate models) on the main effects of partner confidence and ability conditions (each were dummy coded). As anticipated, partners received higher confidence ratings when they expressed high confidence (M = 4.31; SD = .89) than low confidence {M = 2.82; SD = 1.25; $t(244)$ = 10.76, p < .001, d = 1.39, CI of mean difference = [1.20, 1.74]}, but did not differ significantly in their perceived confidence when performance was purportedly high or low {$t(244)$ = 1.69, p = .093, d = .26, CI of mean difference = [-.04, .50]}. Also as expected, partners were rated as more competent when they demonstrated high performance (M = 4.71; SD = .54) than low performance {M = 3.06; SD = 1.14; $t(245)$ = 14.58, p < .001, d = 1.85, CI of mean difference = [1.43, 1.88]}, but did not differ significantly in their perceived competence when expressed confidence was high or low {$t(245)$ = -.98, p = .329, d = -.01, CI of mean difference = [-.33, .11]}. These checks confirm that our manipulations were effective in creating the intended partner cognitive profiles. Actors paired with overconfident and underconfident partners were acutely aware of their partner's discrepant confidence and performance.

RANDOM ASSIGNMENT CHECK (OVERCONFIDENCE IN BASELINE PHASE)

In this study, participants were randomly assigned to one of four between-subject conditions. As such, if our random assignment procedure was successful, overconfidence should not differ across conditions in the baseline phase, before actors were exposed to their partners. To empirically verify this, we compared actor overconfidence in the baseline phase across conditions. We regressed baseline phase actor overconfidence on the main effects and interaction of partner confidence condition (0 = low, 1 = high) and partner performance condition (0 = low, 1 = high). The results of this regression are displayed in Table 3 (first column). As expected, there was no main or interactive effect of condition. This absence of detectible difference in actor overconfidence at baseline across conditions (*t*s range .28 to 1.33, *p*s range .185 to .780) confirms the effectiveness of the random assignment procedure.

WHY TEST THE TRANSMISSION OF OVERCONFIDENCE HYPOTHESIS USING BETWEEN-ACTOR EFFECTS, RATHER THAN WITHIN-ACTOR EFFECTS?

Our central hypothesis is about the effect of partner confidence on actor overconfidence. To test this, here in Study 4 (and also in Studies 2 and 3) we focus on comparing the degree to which actors make unrealistic assessments of their ability after observing input from different types of partner who vary in their self-assessments. This analytical approach is therefore aimed at examining *between*-actor differences post-partner input (the test phases) across our experimental conditions, rather than *within*-actor trajectory that captures change from pre-partner input (the baseline phase) to post-partner input (the test phase).

Modeling between-person differences, rather than within-person changes, is optimal for these data. Deploying within-person analyses would yield ambiguous results complicated by two features of our stimuli and experimental design. The first feature, as mentioned in the main text, is trial-to-trial variation in difficulty. Existing work indicates that degree of overconfidence varies with task difficult, decreasing in difficult tasks and increasing in easy tasks (Moore & Healy, 2008). Consistent with this, among the current trials actors' weight estimates in the test trials were on average off target by as few as 8.43 lb for one target (Trial 11) and as high as 22.44 lb for another target (Trial 13; *SD*s = 5.88 and 13.99, respectively), confirming the existence of considerable variation in trial difficulty. This means that, even if propensity towards overconfidence remained stable and is unaffected by partner input, actors are expected to overestimate their performance to a greater degree on easy trials (such as Trial 11) compared to difficult trials (such as Trial 13), because trial difficulty will alone create differences in mean level overconfidence across trials. As such, not only do person-level trajectories fall short of documenting meaningful change in overconfidence over successive trials, but moreover any conclusions drawn from interpreting these within-actor trajectories across trials, both within or across experimental conditions, would be misleading.

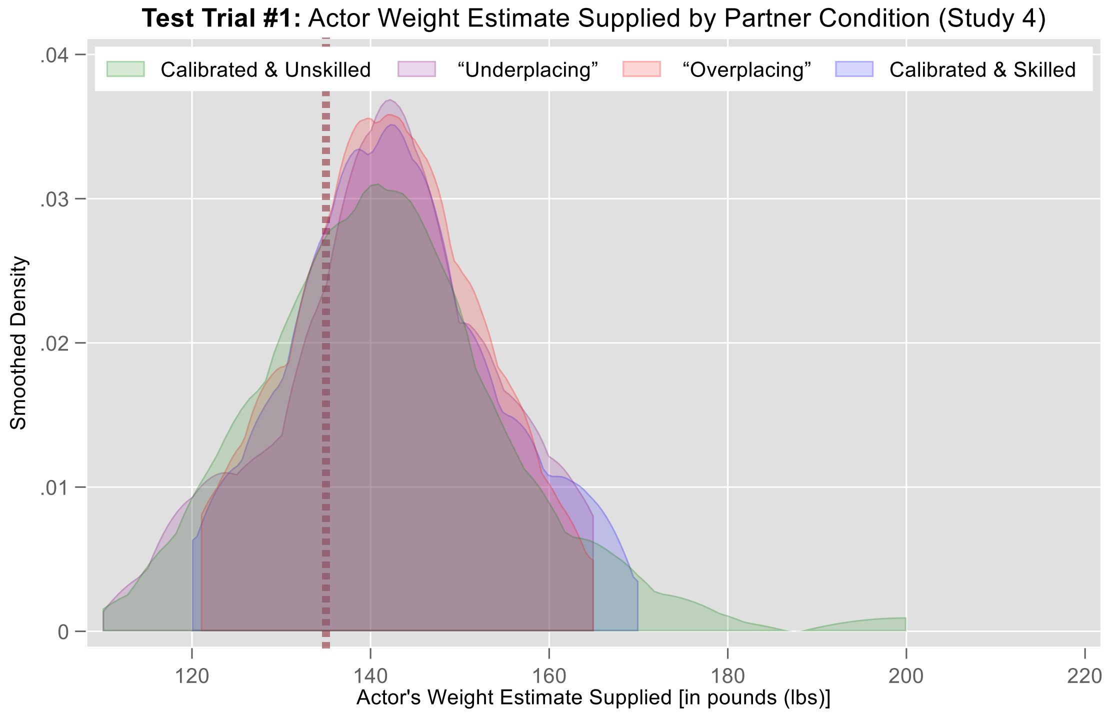
Apart from task difficulty, a second reason why within-person analyses may generate false conclusions is that overconfidence tends to be also reduced by task experience and feedback (Ehrlinger et al., 2008; Gervais & Odean, 2001; Hertzog et al., 1994; Koriat et al., 2002; Kruger & Dunning, 1999; Menkhoff et al., 2006; Muthukrishna et al., 2018); although some other evidence indicates that overconfidence can persist despite increased experience; Chen, Kim, Nofsinger, & Rui, 2007; Heath & Tversky, 1991; Lipko, Dunlosky, & Merriman, 2009). For example, in Paese & Kinnaly's (1993) study on the effects of confident peers, the authors observed that "relative to initial mean overconfidence… there was a large reduction in overconfidence among all subjects, except those who received overconfident input". Although, as we caution above, interpreting within-person trends is potentially problematic due to its possible contamination by task difficulty, it may nevertheless be worth pointing out that our current data yields a very similar trend. In Figure 4 in the main text, which plots raw mean overconfidence trial-by-trial separately for each of our four partner conditions, we observe the same pattern of declining overconfidence over successive trials, except actors who received confident partner input; these actors' degree of overconfidence appear more or less comparable before and after partner input. Although this trend might appear puzzling at first glance, as one may expect overconfidence to be higher in post-partner trials than pre-partner trials, according to the transmission of overconfidence hypothesis. However, the fact that the same high degree of overconfidence is observed in the post-partner trials among actors who witnessed confident norms, whereas all other actors exhibit a sharp decline typified by past studies, suggests that confident partners managed to effectively counteract the anticipated decline in bias over time. Nevertheless, that overconfidence trajectories tend to be influenced by task experience remains problematic for drawing firm conclusions.

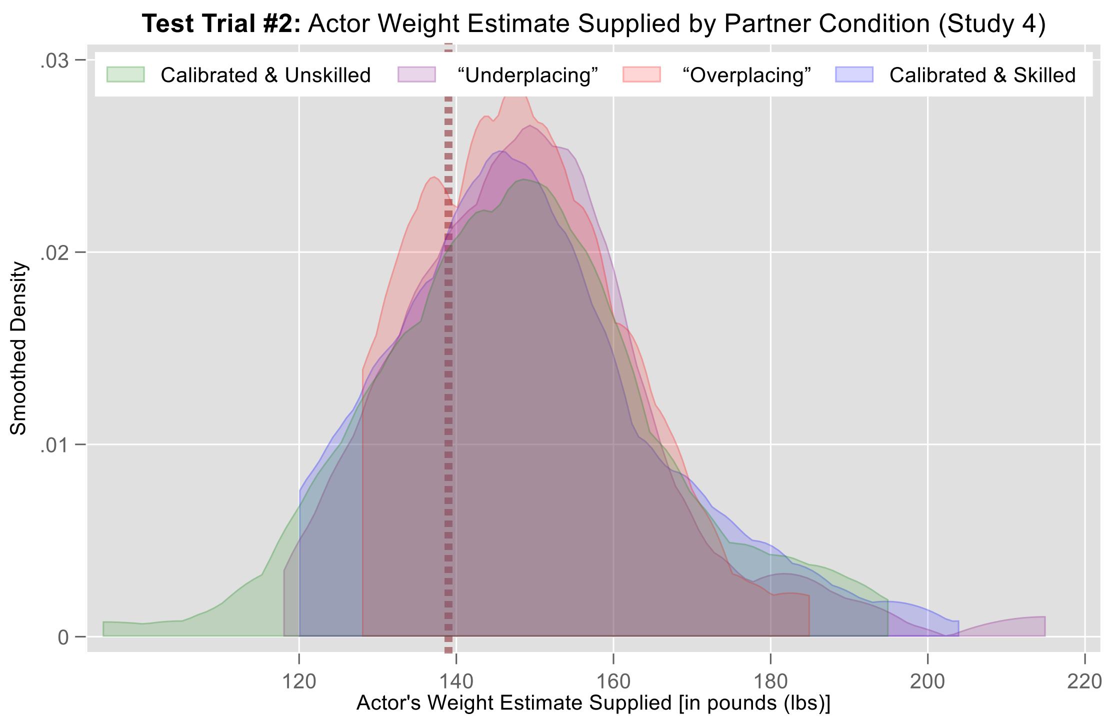
Overall, then, for these reasons noted above, within-person analyses are unable to provide meaningful substantive results and at the same time risk incorrect inferences, and were avoided in our analyses. Instead, our primary results reported in the main text focus on between-person analyses that are unaffected by these issues.

SUPPLEMENTAL RESULTS

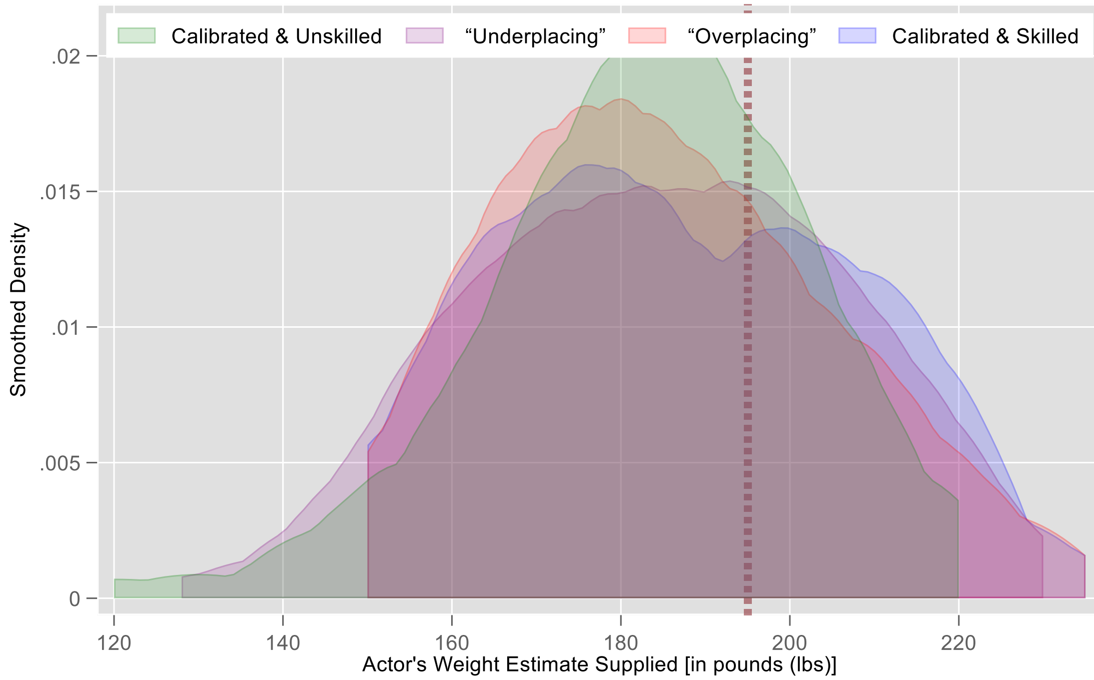
WEIGHT ESTIMATE RESPONSE

Below, density plots for the weight estimate supplied by actors (i.e., their answer to each trial of the weight-guessing game). These plots show the absence of difference across experimental conditions, suggesting that, while partners shift actors' confidence beliefs, they have little influence over actors' in-game responses and (by implication) performance. The dotted line marks the correct answer (i.e., the actual weight of the individual in the photo).

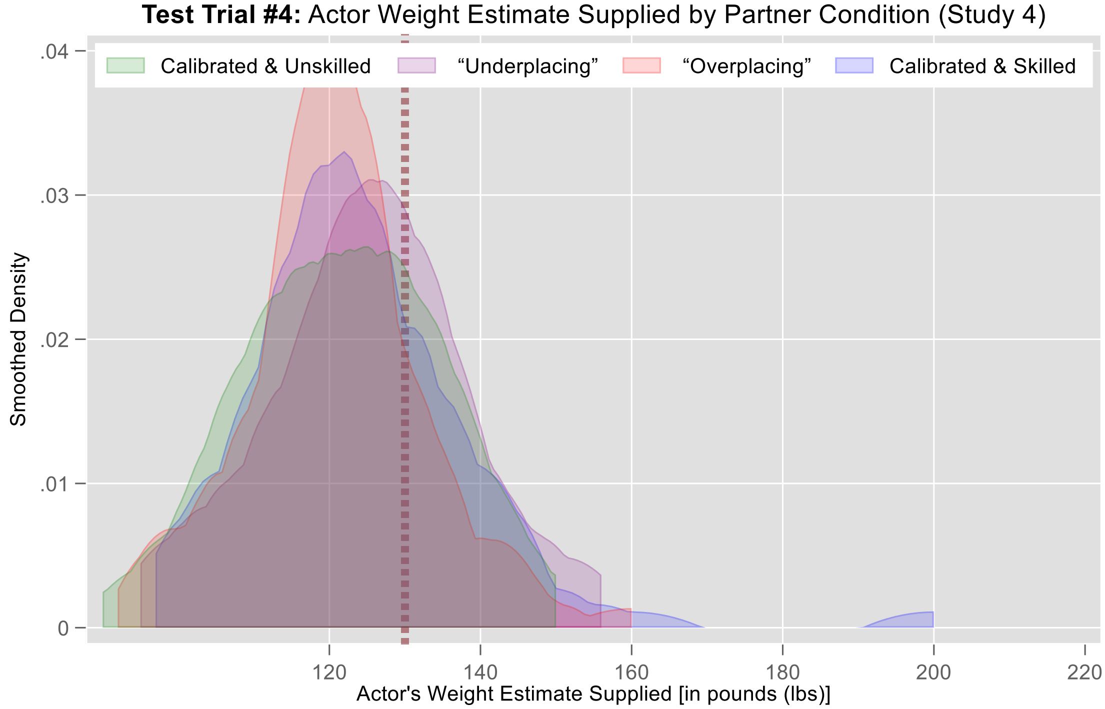

Test Trial #1: Actor Weight Estimate Supplied by Partner Condition (Study 4)
Calibrated & Unskilled
"Underplacing"
"Overplacing"
Calibrated & Skilled
Smoothed Density
.04
.03
.02
.01
0
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

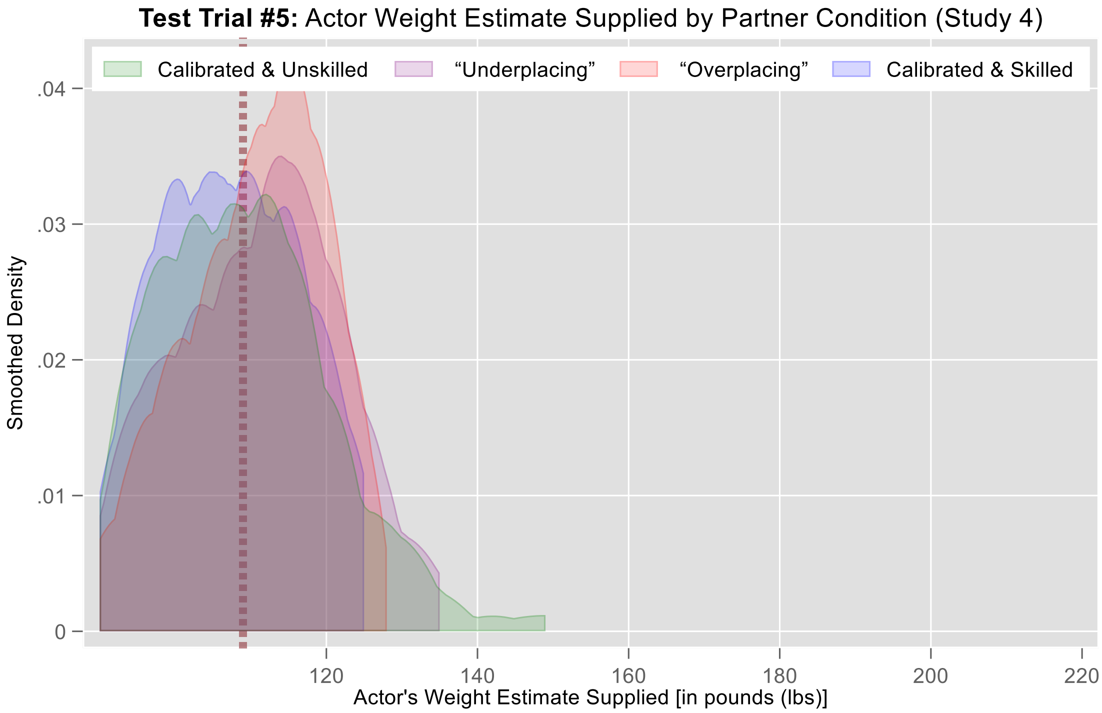

Test Trial #2: Actor Weight Estimate Supplied by Partner Condition (Study 4)
Calibrated & Unskilled
"Underplacing"
"Overplacing"
Calibrated & Skilled
Smoothed Density
.03
.02
.01
0
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

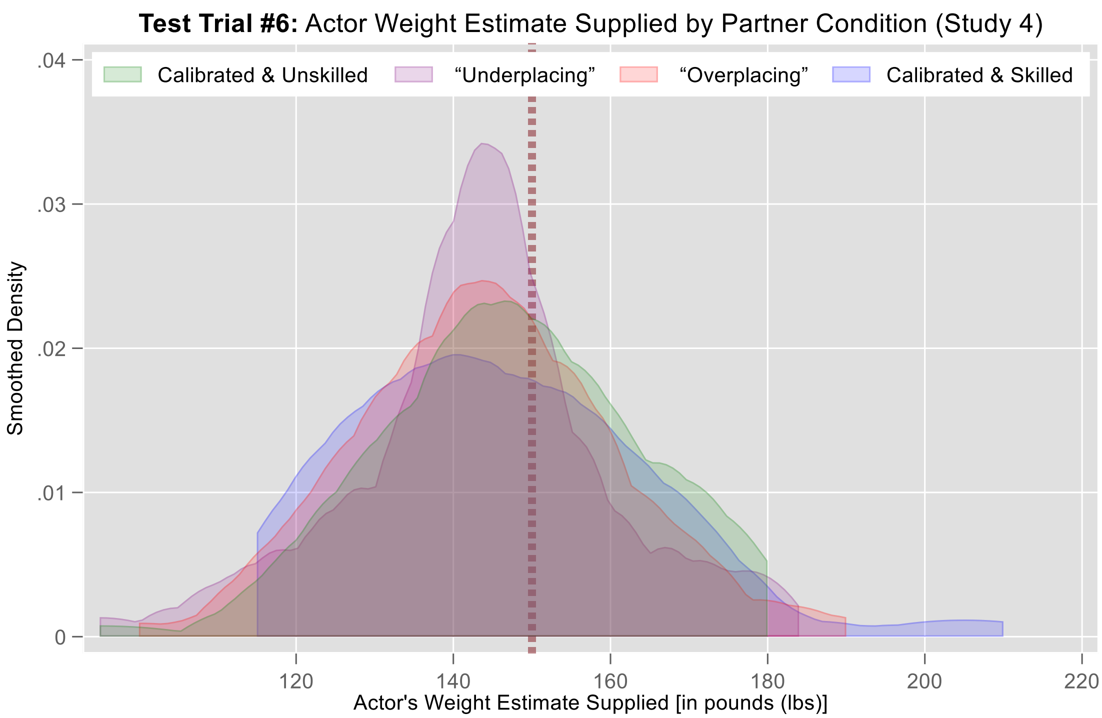
Test Trial #3: Actor Weight Estimate Supplied by Partner Condition (Study 4)



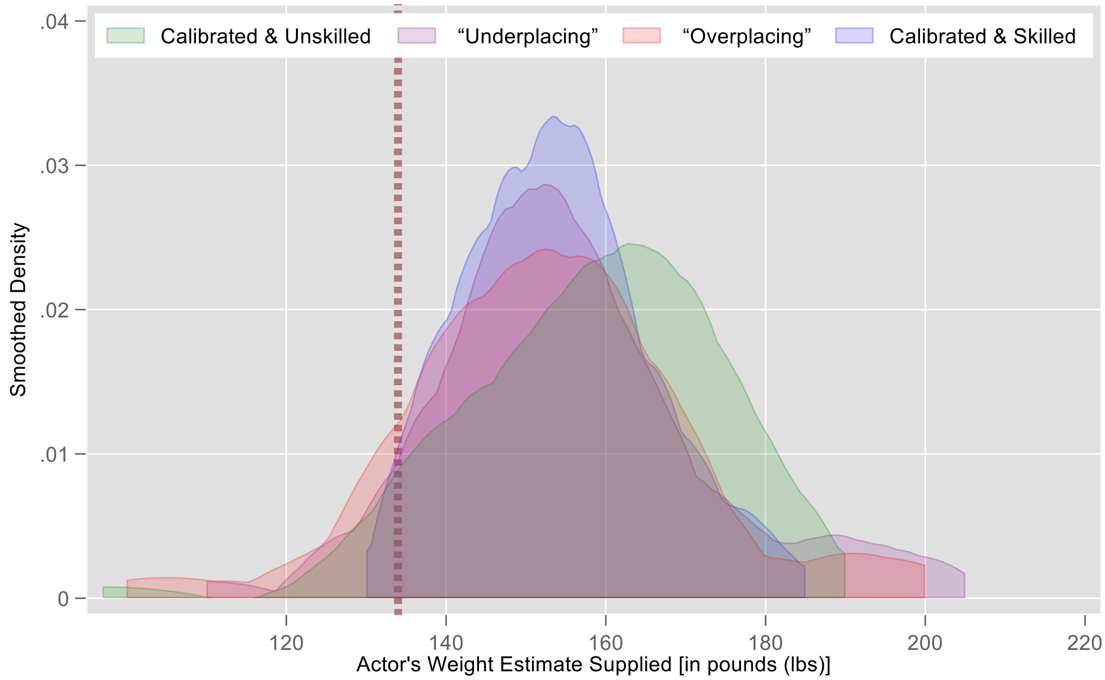
Test Trial #4: Actor Weight Estimate Supplied by Partner Condition (Study 4)



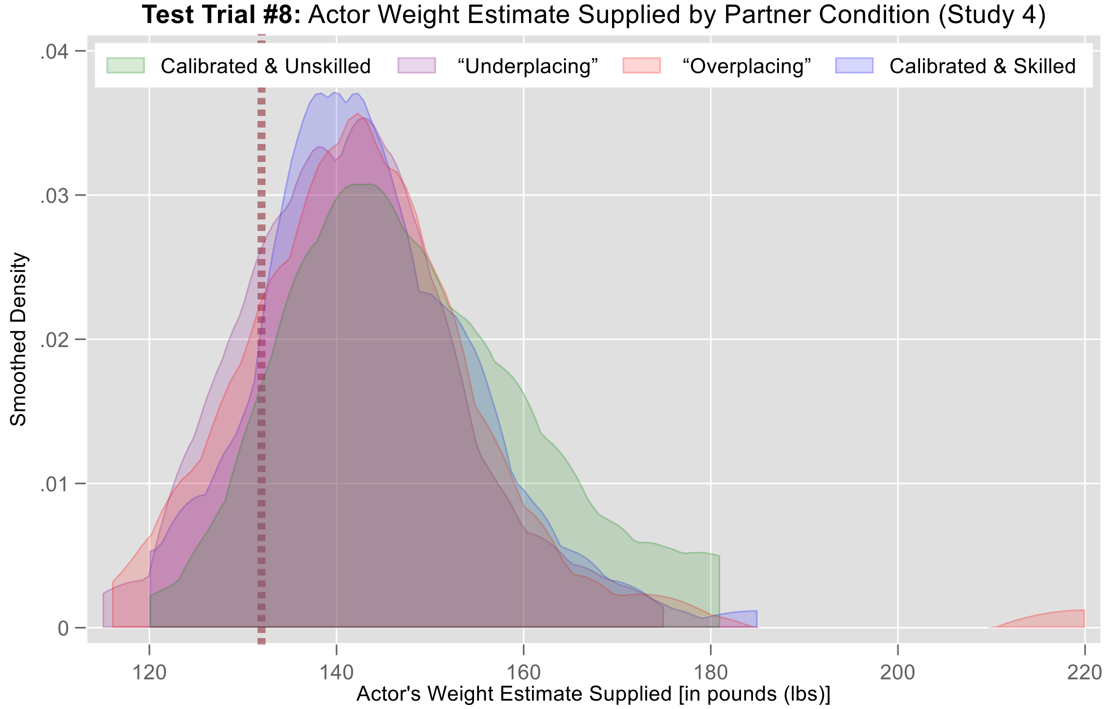

Test Trial #5: Actor Weight Estimate Supplied by Partner Condition (Study 4)
Calibrated & Unskilled
"Underplacing"
"Overplacing"
Calibrated & Skilled
Smoothed Density
0
.01
.02
.03
.04
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

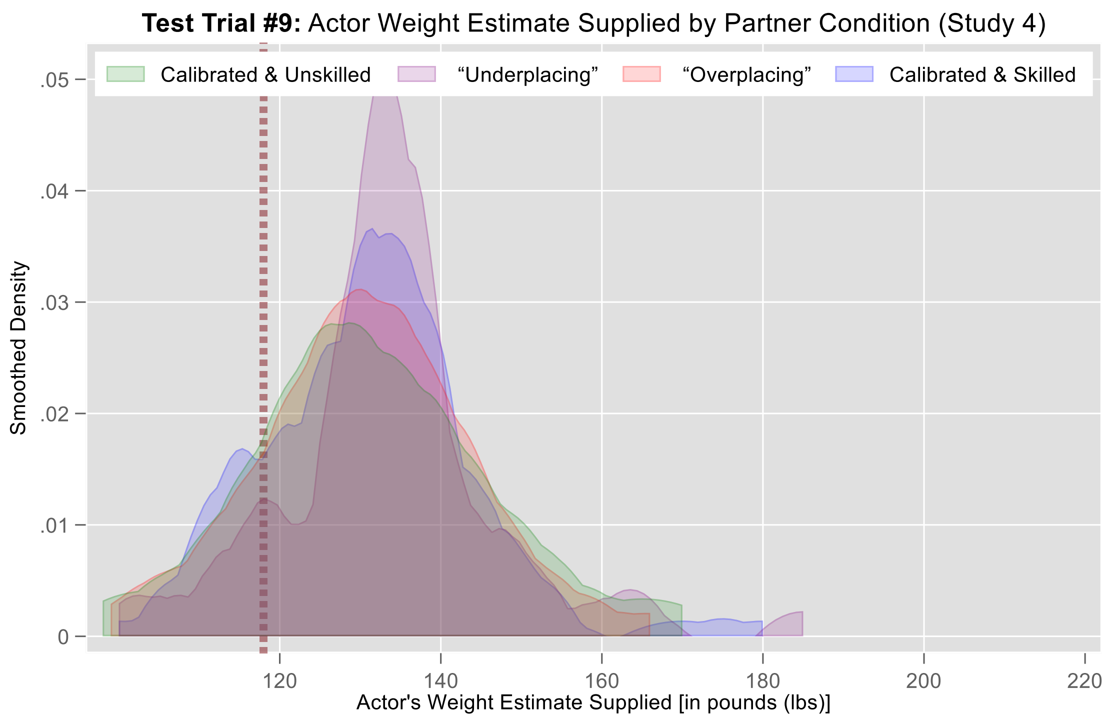

Test Trial #6: Actor Weight Estimate Supplied by Partner Condition (Study 4)
Calibrated & Unskilled
"Underplacing"
"Overplacing"
Calibrated & Skilled
Smoothed Density
0
.01
.02
.03
.04
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

Test Trial #7: Actor Weight Estimate Supplied by Partner Condition (Study 4)



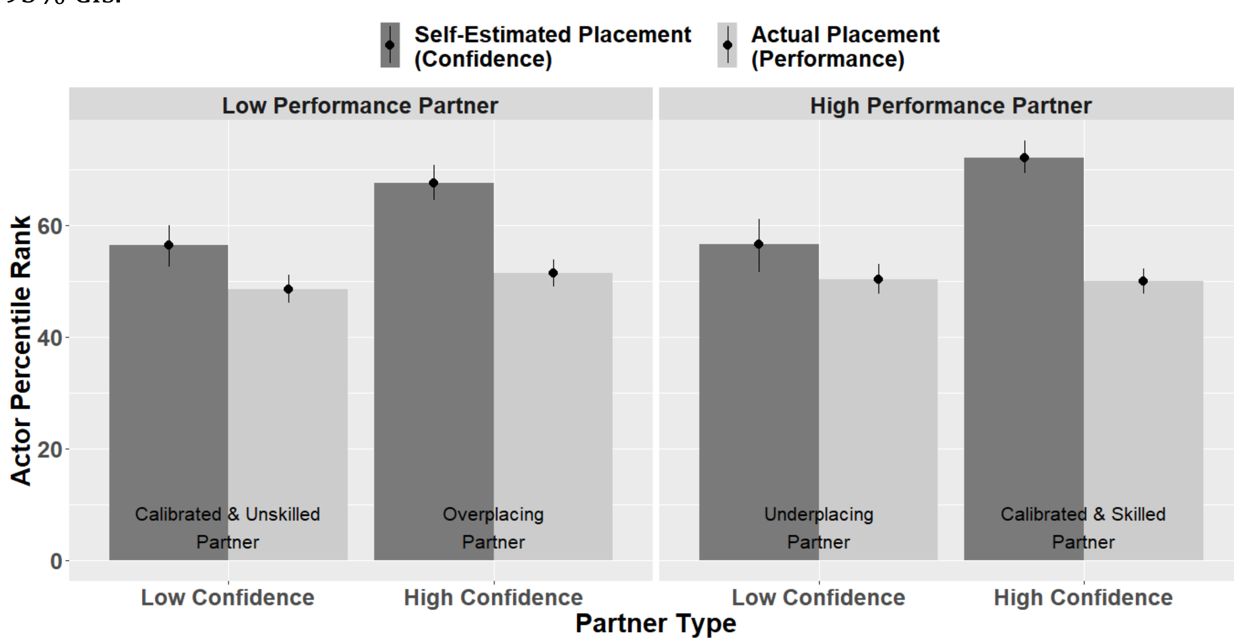
Test Trial #8: Actor Weight Estimate Supplied by Partner Condition (Study 4)





SELF-ESTIMATED AND ACTUAL PERFORMANCE DESCRIPTIVES

Bar graphs showing that individuals exposed to a confident, high self-placing partner—regardless of whether performs well (and thus is calibrated) or poorly (and thus is overplacing)—are more likely to themselves subsequently overplace. Error bars provide 95% CIs.



DOES OVERCONFIDENCE PERSIST OR DETERIORATE ONCE EXPOSURE TO PARTNER CEASES (OVERCONFIDENCE IN THE POST-PARTNER PHASE)?

Ancillary analyses were conducted to explore whether the effects of partners persist, influencing actors even after reminders about the partner's cognitive style ceases (that is, strictly in the post-partner-information phase). Paralleling our efforts in the main text for mean overconfidence reported across the entire test phase, here we regressed only mean overconfidence expressed in trials that lacked any partner responses on the main effects and interaction of partner confidence and performance condition. The results from this regression model, displayed in Table 3 in the main text (last column), indicate that in this post-partner phase, the pattern of results replicate those based on the entire test phase. Consistent with a significant main effect of partner confidence, actors showed approximately a 12.23 percentile point stronger overconfidence bias when exposed to a highly confident partner (M = 18.46; SD = 21.87) several trials back than when exposed to a partner with low confidence (M = 4.46; SD = 23.78; $t(244)$ = 4.76, p < .001, d = .61, CI of mean difference = [8.13, 19.61]). As found for the entire test phase, actor overconfidence remained elevated as long as confidence was high—regardless of whether partner was overconfident (that is, had low performance; M = 16.53; SD = 22.23) or justifiably confident (that is, had high performance; M = 20.14.10; SD = 21.57), which again did not differ in magnitude {$t(244)$ = .89, p = .372, d = .16, CI of mean difference = [-4.34, 11.56]}.

Notably, the effect sizes associated with these comparisons, which contrast levels of overconfidence across partner conditions, are only slightly weaker than those obtained above based on the entire test phase (i.e., ds = .65 and .57, respectively), suggesting that the transmission of overconfidence only showed very slight decay when reminders about the partner's beliefs had ceased. Again, this is consistent with the notion that interacting with confident others—irrespective of the degree to which this confidence is actually justified—enhances beliefs in one's own capabilities, which, in many cases, further increases the pre-existing tendency to hold overly inflated self-assessments. Together, these results, similar to what we later show in Study 5, suggest that the effects of overconfident others on observers may endure and continue to influence cognition and behaviors beyond initial exposure.

ARE INDIVIDUALS (CONSCIOUSLY) AWARE OF THE INFLUENCE OF OVERCONFIDENT PEERS OVER THEIR OWN BIASES?

We also extend our investigation by exploring whether individuals are cognizant of the influence of overconfident models on their own beliefs. This is an important domain of inquiry in light of evidence showing that individuals are largely unaware of and unable to accurately identify the factors that influence their behavior, including the social influence of others (Nisbett & Wilson, 1977; Vartanian et al., 2008). We examined this question here in Study 4 by assessing perceptions of the partner's influence on their own responses, and then contrasting these perceptions across partner conditions. Much prior work on human irrationality raises the possibility that people—who are strongly motivated to see themselves as rational, utility-maximizing agents, who would for example select advisors with true (rather than self-proclaimed) skill and knowledge—may underestimate the social influence of overconfident partners, while failing to observe and identify the true determinants of their cognitive processes (e.g., Hansen, Gerbasi, Todorov, Kruse, & Pronin, 2014; Pronin, 2007; Pronin, Lin, & Ross, 2002; Tavris & Aronson, 2008).

To address this, after completing all task trials, actors rated their subjective perception of their partner's social influence on two items: "I gave a lot of weight to my partner's answers" (M = 3.13, SD = 1.26), and "I took my partner's responses as advice" (M = 3.22, SD = 1.17). Responses to these two items, supplied on a 5-point scale (1 = *Disagree strongly*, 5 = *Agree strongly*), were highly correlated (r = .59), and thus averaged to index overall perceived partner influence (M = 3.18, SD = 1.08; α = .74).

To analyze these data, we regressed actors' ratings of partner influence on the main effects and interaction of partner confidence and performance. This analysis yielded a significant interaction indicating that forecasts of a confident partner's influence depends on her skill level {b = .65, t(244) = 2.70, p = .007, 95% CI = [.177, 1.128]}. Consistent with the prediction emerging from overly optimistic motivated reasoning, overconfident partners were subjectively evaluated as the *least* influential (M = 2.41; SD = .92; compared against the 3 other conditions, ts = 3.15 to 7.87, ps < .002), despite their substantial sway on actors' behaviors. As expected, deemed most influential were calibrated and high-performing partners (M = 3.72; SD = .91) and underconfident (low confidence, high performance) partners (M = 3.61; SD = .1.07; each compared against the 2 other conditions, ts = 3.81 to 7.87, ps < .001).

Moreover, as another look at this question, we explored whether actors' judgments of their partner's influence tracked the changes in their own confidence levels, which were themselves influenced by the partner. That is, did actors whose confidence levels show the greatest increase from pre- to post-exposure to the partner (accurately) deem their partner as more influential? Change in confidence was computed by subtracting the actor's mean confidence in the baseline phase from her mean confidence in the entire test phase. This analysis shows, interestingly, no significant association between actual increases in confidence and ratings of partner influence in each condition (ps > .105), except for actors in the overconfident partner condition, for whom increases in confidence was *negatively* associated with perceived partner influence (r = -.30, p = .021). Taken together, these results show that, despite a strong propensity to copy the cognitive pattern of overconfident others, actors subjectively perceived overconfident partners as the least influential, and instead believed that they had rationally prioritized actual performance over confidence in their choice of whom to model. In fact, contrary to the motivated perception that one can accurately identify the sources that influence one's behavior, the more potent the influence of the overconfident partner in actually boosting the actor's confidence, the less did the actor see this influential partner as playing a salient role in their decision-making.

STUDY 5: The TRANSMISSION OF OVERPLACEMENT ACROSS TIME AND TASK DOMAINS

SUPPLEMENTAL METHODS

MANIPULATION CHECK

To verify the effectiveness of our experimental manipulation, we checked whether ratings of partner confidence supplied at both time points were higher in the overconfident partner condition than in the calibrated partner condition. Confirming this, at Time 1, ratings of partner confidence were higher for actors who observed an overconfident partner (M = 4.32, SD = .78) than for actors who observed a calibrated partner {M = 3.19, SD = 1.04; $t(402)$ = -12.27, p < .0001, d = 1.22, CI of mean difference = [.94, 1.30]}. At Time 2, the same pattern emerged {M = 4.05, SD = .77 vs. M = 3.45, SD = .91; $t(198)$ = 5.00, p < .0001, d = .71, CI of mean difference = [.36, 84]}, indicating that despite the time that has elapsed, participants were able to recall the observed partner's characteristic and behaviors at Time 2. The diminished effect size of these comparisons at Time 2 compared to Time 1 indicates that, as would be expected given some degree of memory decay, our manipulations were more potent at Time 1 than Time 2 but remained effective in creating the intended perceived partner profiles at both time points.

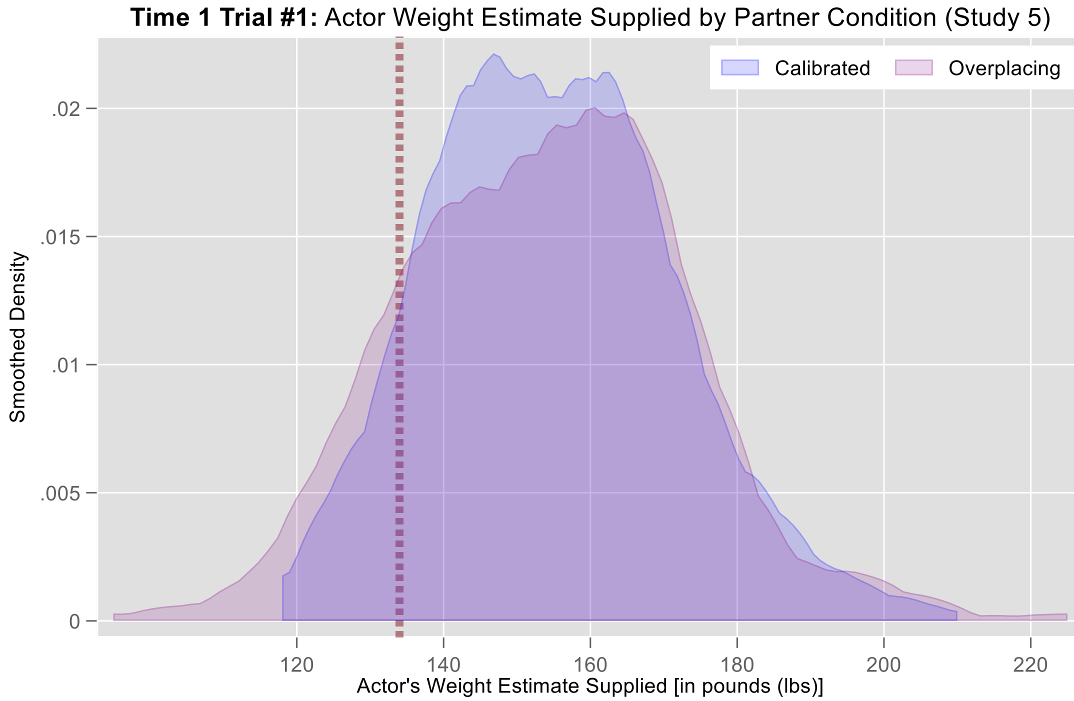
CONTROL VARIABLE: MEMORY OF TIME 1 SURVEY

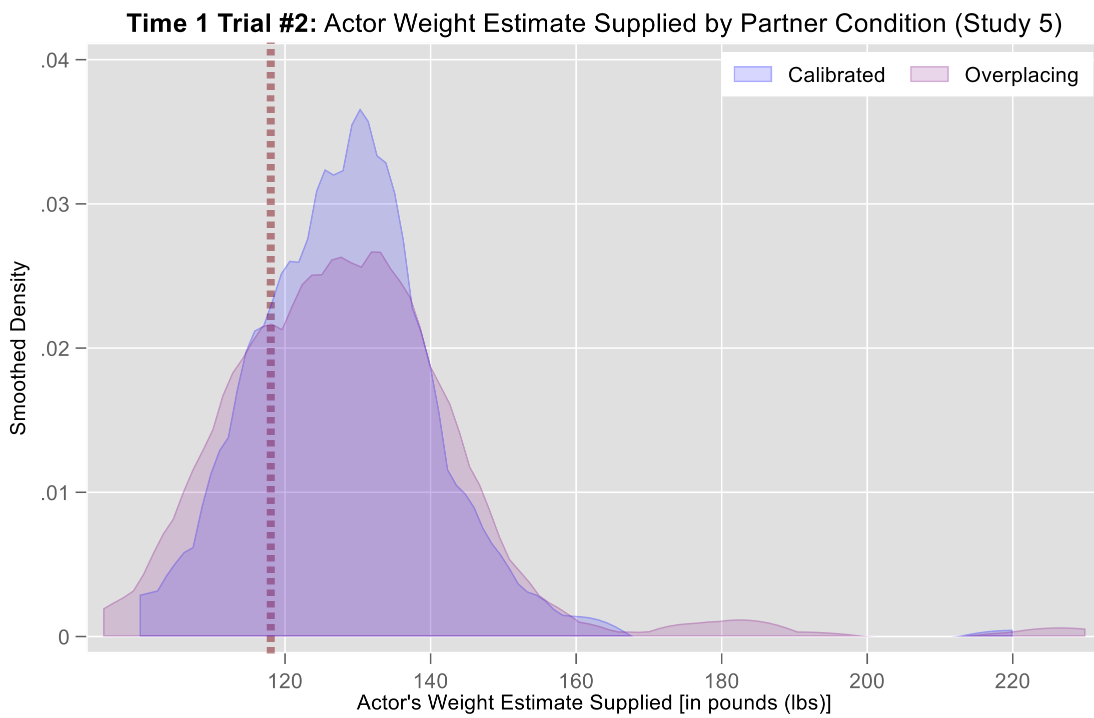
We were concerned that actors' differential memory of the Time 1 survey (i.e., the first part of the study) may contribute to variation in overconfidence shown on the Time 2 survey. To address this, at Time 2 after the completion of all tasks, we probed actors for their memory of the Time 1 survey to use as controls in our models. Actors were asked to rate how well they remembered playing the game earlier (1 = *No memory of it at all*, 4 = *Remember doing it but not the specifics*, 7 = *Remember it extremely well*). Our regression models (reported in Table 4) indicate that this memory of game measure did not predict any of our measures of overconfidence, nor did it change our key results highlighting the effect of partners on actor overconfidence.

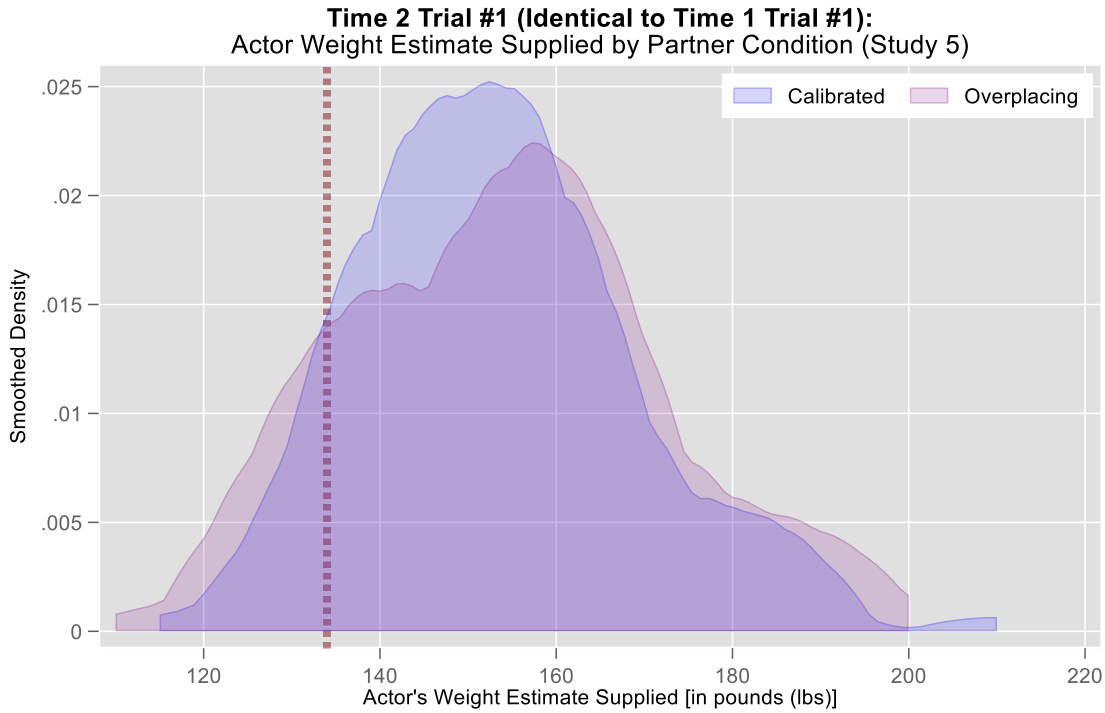
SUPPLEMENTAL RESULTS

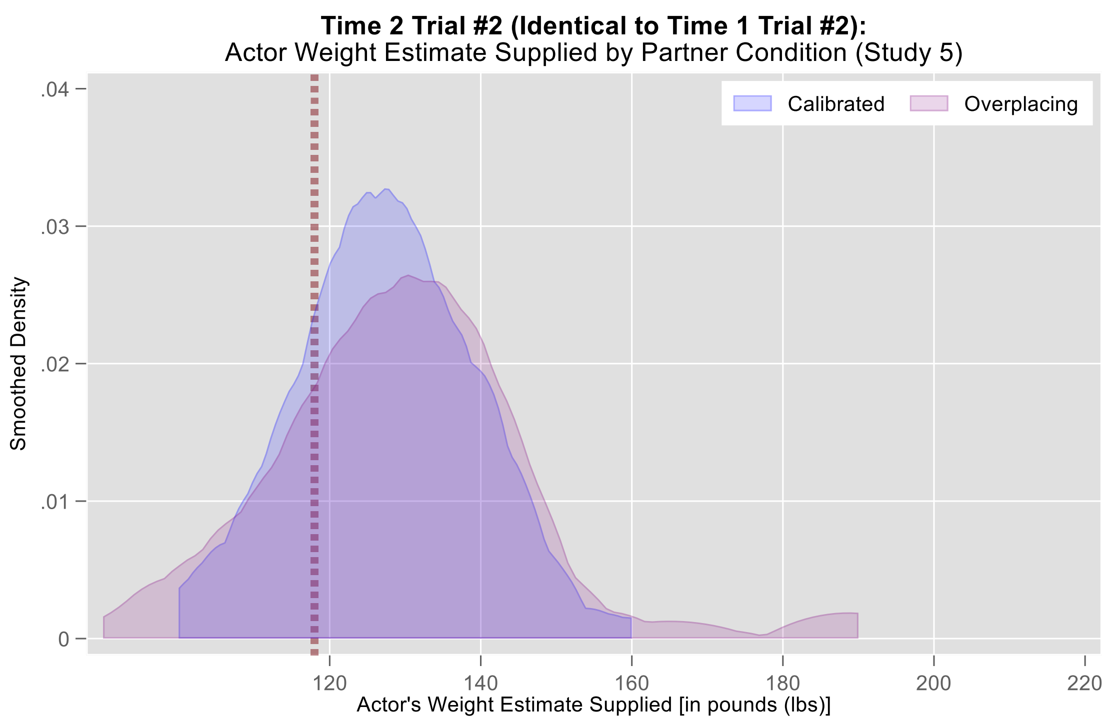
WEIGHT ESTIMATE RESPONSE

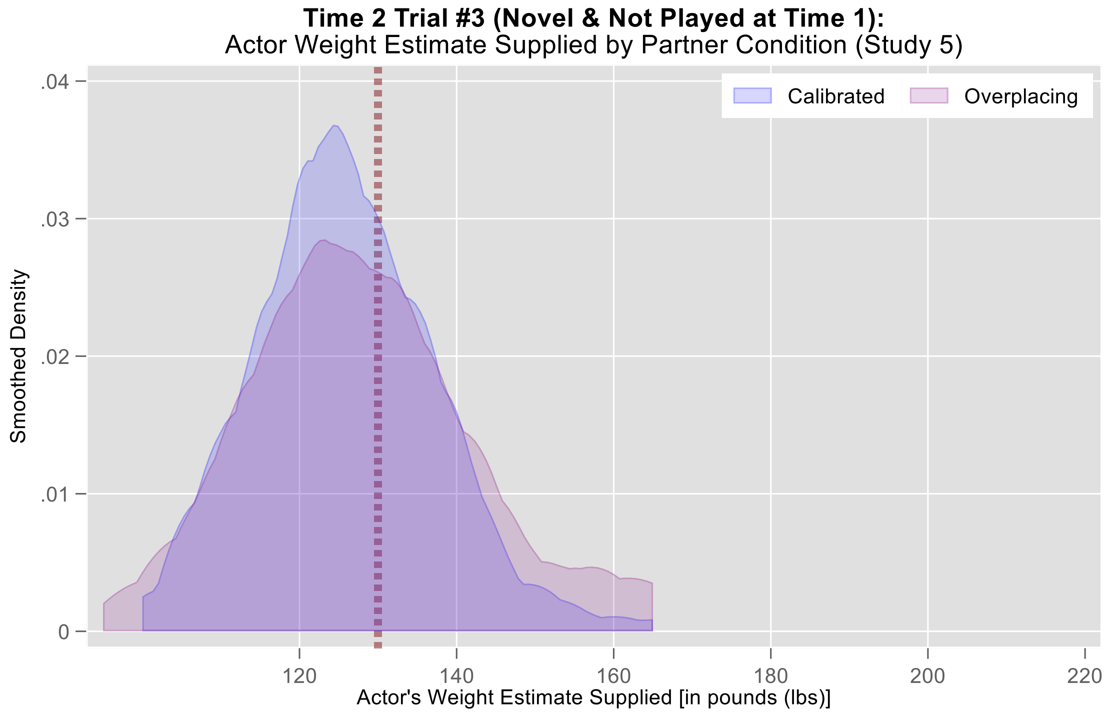
Below, density plots for the weight estimate supplied by actors (i.e., their answer to each trial of the weight-guessing game). These plots show the absence of difference across experimental conditions, suggesting that, while partners shift actors' confidence beliefs, they have little influence over actors' in-game responses and (by implication) performance. The dotted line marks the correct answer (i.e., the actual weight of the individual in the photo).

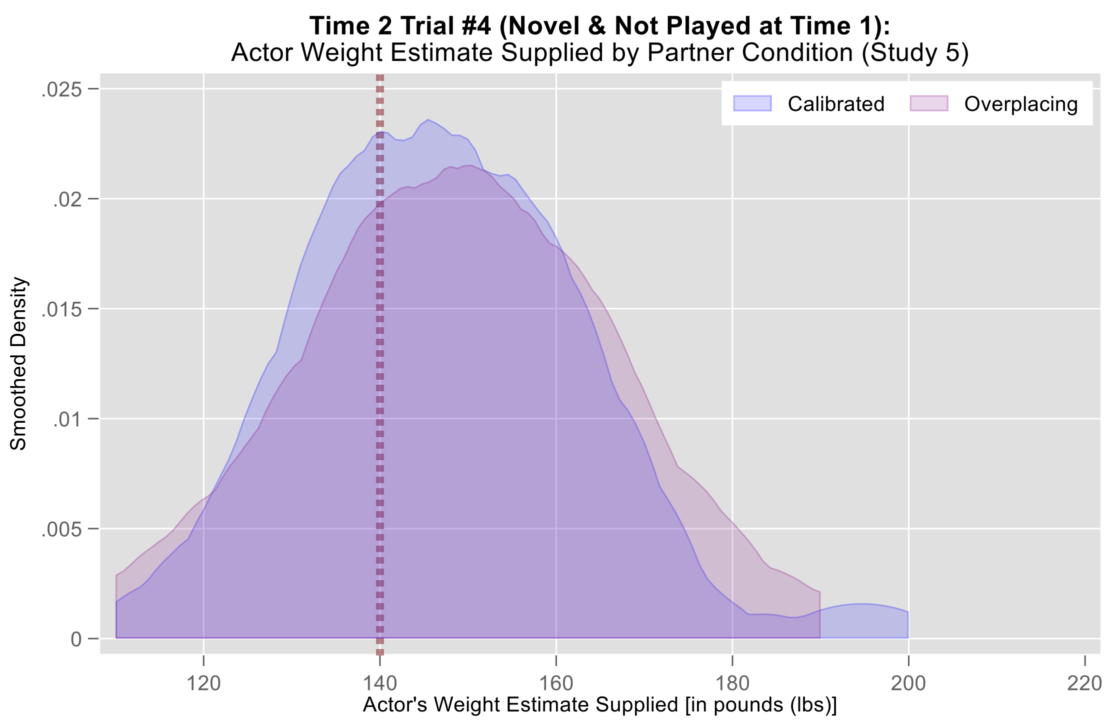

Time 1 Trial #1: Actor Weight Estimate Supplied by Partner Condition (Study 5)
Calibrated
Overplacing
Smoothed Density
0
.005
.01
.015
.02
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]


Time 1 Trial #2: Actor Weight Estimate Supplied by Partner Condition (Study 5)
Calibrated
Overplacing
Smoothed Density
0
.01
.02
.03
.04
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]


Time 2 Trial #1 (Identical to Time 1 Trial #1):
Actor Weight Estimate Supplied by Partner Condition (Study 5)
Calibrated
Overplacing
Smoothed Density
0
.005
.01
.015
.02
.025
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

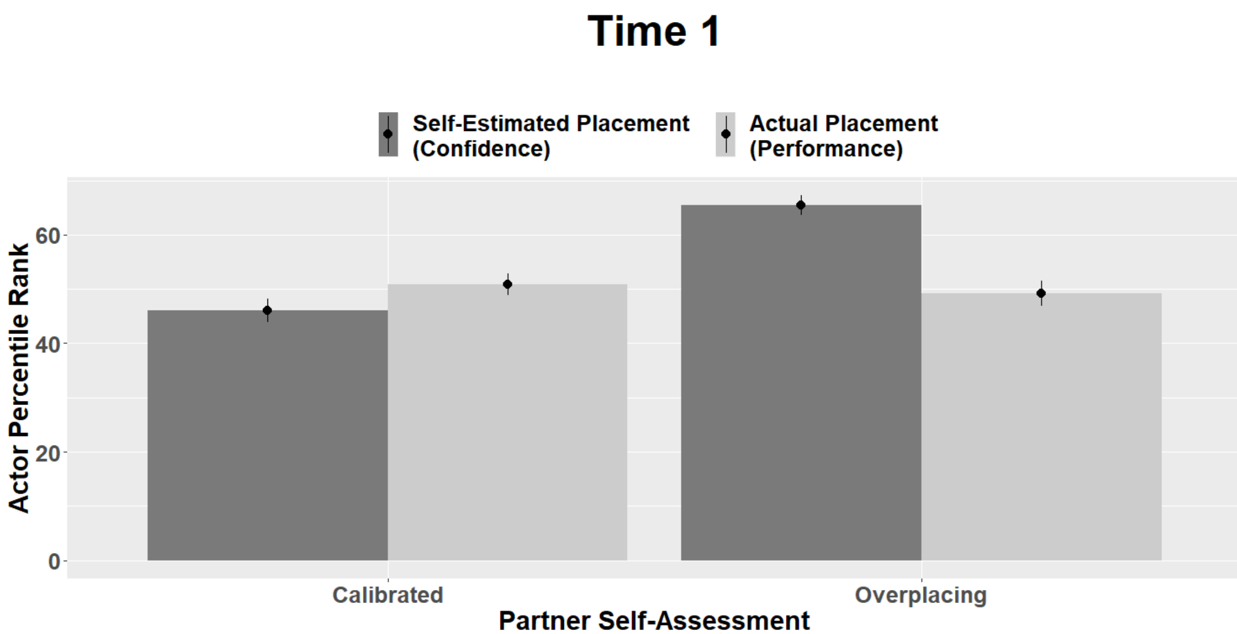

Time 2 Trial #2 (Identical to Time 1 Trial #2):
Actor Weight Estimate Supplied by Partner Condition (Study 5)
Calibrated
Overplacing
Smoothed Density
0
.01
.02
.03
.04
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

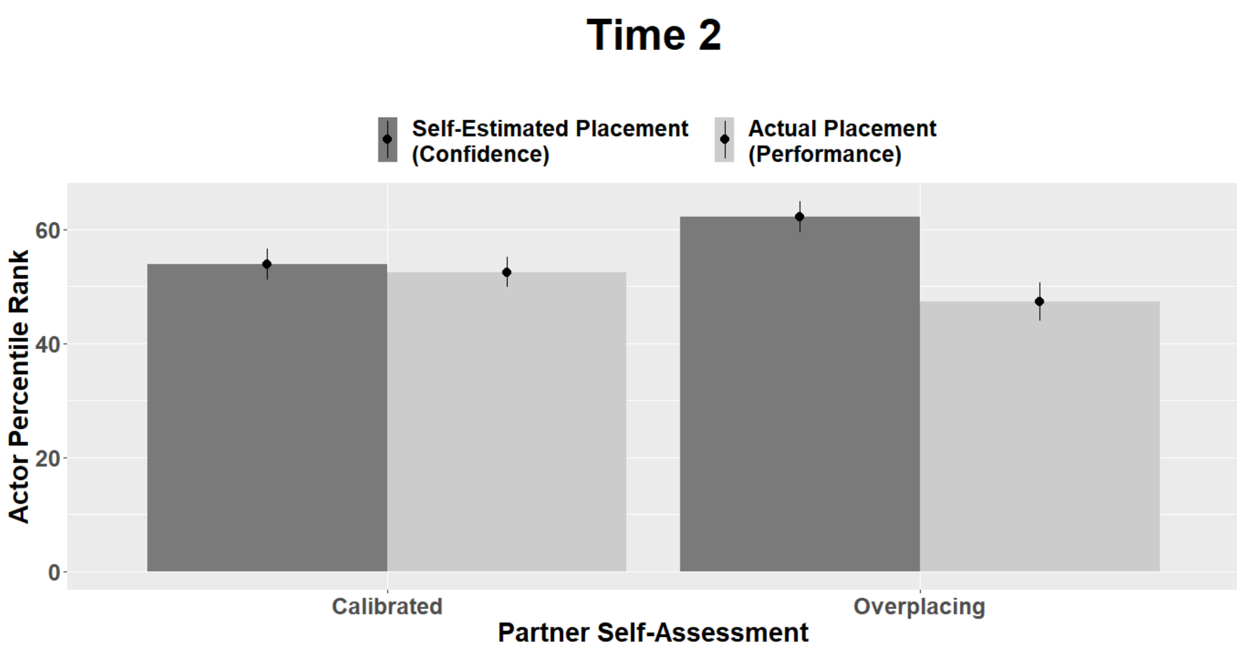

Time 2 Trial #3 (Novel & Not Played at Time 1):
Actor Weight Estimate Supplied by Partner Condition (Study 5)
Calibrated
Overplacing
Smoothed Density
.04
.03
.02
.01
0
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]


Time 2 Trial #4 (Novel & Not Played at Time 1):
Actor Weight Estimate Supplied by Partner Condition (Study 5)
Calibrated
Overplacing
Smoothed Density
.025
.02
.015
.01
.005
0
120
140
160
180
200
220
Actor's Weight Estimate Supplied [in pounds (lbs)]

SELF-ESTIMATED AND ACTUAL PERFORMANCE DESCRIPTIVES

Bar graphs showing that—at both the initial time of partner exposure (Time 1) and days later (Time 2)—individuals exposed to an overplacing partner are more likely to themselves subsequently overplace. Time 2 data are on the weight-guessing repeated trials. Error bars provide 95% CIs.





DID THE TRANSMISSION OF OVERCONFIDENCE EFFECT INFLUENCE SELF-ASSESSMENTS ON NOVEL TRIALS (BEYOND JUST THE IDENTICAL TRIALS) IN THE SAME TASK DOMAIN AT TIME 2, DAYS AFTER THE INITIAL EXPOSURE TO OVERCONFIDENT OTHERS?

Recall that, of the 4 trials of the weight-guessing game played at Time 2, 2 trials were repeated from Time 1 (identical photos were played in Time 1 trials) and 2 other trials were completely novel (photos not previously displayed). To examine whether overconfidence at Time 2 was elevated only for repeated trials, we can compare results from the regression models that analyze the repeated and novel trials separately (Table 4, columns 5-8). These results indicate that the boost in overconfidence resulting from overconfidence exposure was only slightly (and non-significantly) stronger on the repeated trials than the novel trials. When the partner was overconfident, actors increased their overconfidence by 13.45 percentile points in the repeated trials {$t(198) = 3.22$, $p = .002$, $d = .46$, CI of mean difference = [5.20, 21.71]} and 11.77 percentile points in the novel trials {$t(198) = 3.01$, $p = .003$, $d = .43$, CI of mean difference = [4.05, 19.48]}. These results indicate that overconfident partners increased actors' Time 2 overconfidence even on novel stimuli within the same task domain.

TO WHAT EXTENT DID THE EFFECT OF EXPOSURE TO OVERCONFIDENT OTHERS DETERIORATE (IF AT ALL) ACROSS TIME, FROM TIME 1 TO TIME 2, WITHIN THE SAME TASK DOMAIN (AND ON IDENTICAL TRIALS)? A WITHIN-PERSON ANALYSIS USING MULTILEVEL MODELING

Although the effects of exposure to overconfidence at Time 1 and Time 2 was clearly seen in the results reported in the main text, an interesting question is the extent to which the elevated overconfident mindset generated by overconfident peers changes or deteriorates across the two time points, within-person. That is, how did the transmission of overconfidence operate across the sampled times? To address this, we examined how actor overconfidence changed within-person on the two identical weight-guessing game trials at Time 1 and Time 2 using a multilevel model that predicts actor overconfidence (expressed on these two exact trials) from time point (0 = Time 1; 1 = Time 2), partner self-assessment condition (0 = calibrated partner; 1 = overconfident partner), and their interaction. This interaction term tests whether exposure to an overconfident partner moderates the within-person trajectory (that is, whether it predicts how overconfidence changes from Time 1 to Time 2). Note that, of all the game trials assessed, this set of repeated trials in the weight-guessing game is the most appropriate for fitting within-person change trajectories because, by virtue of being identical trials (the same photos), the perceived difficulty of the task is held constant and thus renders overconfidence expressed on them comparable.

Figure 6, in the main text, illustrates the results of this analysis. The most interesting and relevant finding is that the type of partner moderates how overconfidence changes over time points {i.e., a significant time point × partner self-assessment condition interaction; $b = -7.30$, $z = -2.15$, $p = .032$, 95% CI = [-13.96, -.64]}. Complementing and converging with the between-person analyses presented in the main text, actors exposed to a calibrated partner showed, descriptively, slight (but non-significant) underconfidence at Time 1 but became increasingly overconfident over time ($b = 5.73$, $z = 2.43$, $p = .015$, 95% CI = [1.11, 10.35]). Thus, although calibrated partners initially suppressed overconfidence in the actor, the effect "wore off" over time. In contrast, actors exposed to an overconfident partner not only expressed a high degree of overconfidence at both time points, but going from Time 1 to Time 2 their overconfidence remained stable and did not show any significant change over time (b = -1.57, z = -.64, p = .522, 95% CI = [-6.36, 3.23]).[1] This lack of significant decay in the effect of observing confident norms days before is interesting, and, combined with results in the main text, emphasizes the longevity of the overconfidence transmission effect.

WERE PARTICIPANTS EXPLICITLY AWARE OF THE INFLUENCE OF OVERCONFIDENT PARTNERS?

Similar to Study 4, after the completion of all trials, at both Time 1 and Time 2, actors were asked to rate the extent to which they gave a lot of weight to the partner's answers, and whether they took the partner's answers as advice. Both ratings were obtained on a 5-point scale (1 = *Disagree strongly*, 5 = *Agree strongly*).

We found no detectible difference in actors' explicit self-reports of how much weight they gave to the partner when the partner was overconfident vs. calibrated (Time 1: p = .994; Time 2: p = .851), nor were there differences in their reported awareness of whether they took the partner's responses as advice (Time 1: p = .688; Time 2: p = .245). Thus, similar to results from Study 4, actors underestimated the social influence of peers and, in particular, were blindsided by the sway of overconfident peers that is in fact evidenced in their self-assessments.

STUDY 6: THE TRANSMISSION OF OVERPLACEMENT AND COALITIONAL MEMBERSHIP

SUPPLEMENTAL METHODS

MANIPULATING THE PARTNER'S COALITIONAL MEMBERSHIP (IN-GROUP VS. OUT-GROUP)

To manipulate the partner's group membership, the following instructions were presented to actors before they viewed their partner's responses:

[1] To examine the robustness of these results, we ran an additional specification with added covariates. These results indicate that our reported differences in how overconfidence changes over time post-exposure to overconfident (vs. calibrated) peers are robust and not confounded by actors' gender (p = .771), age (centered; b = .57, z = 4.02, p < .001, 95% CI = [-.85, -.29]), or memory of the game from Time 1 (p = .952).

In-Group Partner Condition	Out-Group Partner Condition
"This previous respondent was recruited in the same way as you have been. Like you, he/she also attends University of Illinois. As a side note, as you probably know, our university's football team—the Illinois Fighting Illini—ranks among the top teams nationally, along with our biggest rival—the Ohio State University Buckeyes."	"This previous respondent was recruited in the same way as you have been. Unlike you, he/she attends the Ohio State University, our biggest rival in college football. As a side note, as you probably know, our university's football team—Illinois Fighting Illini—ranks among the top teams nationally, along with our biggest rival—the Ohio State University Buckeyes."

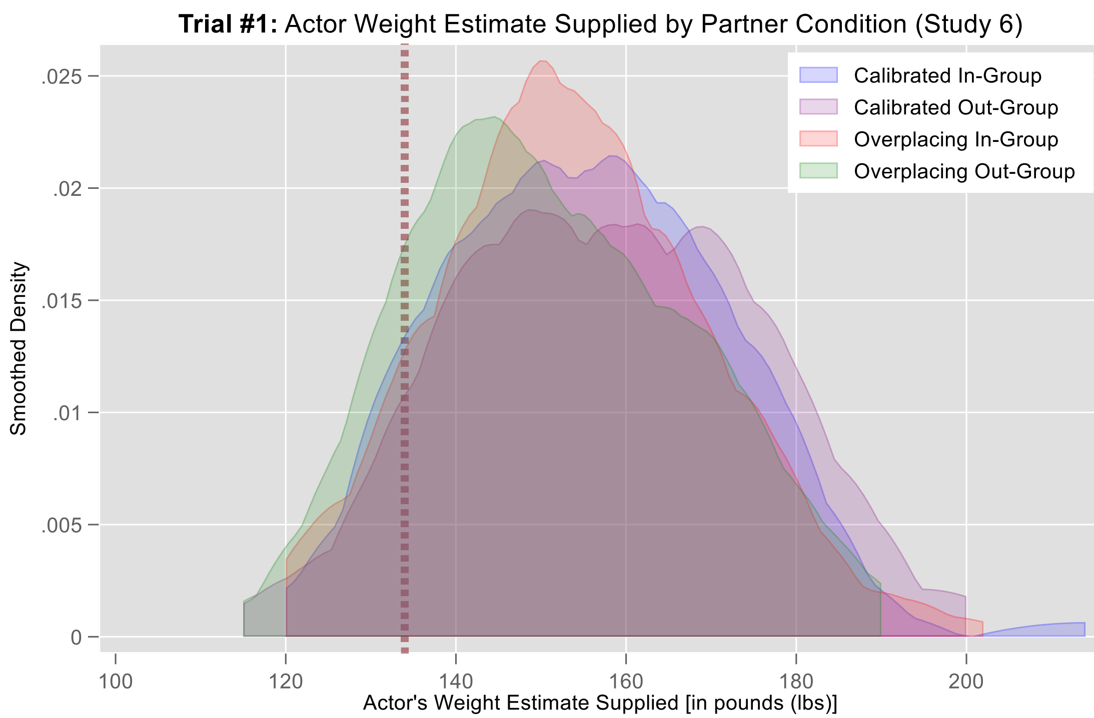
CONTROL VARIABLES: KNOWLEDGE OF FOOTBALL, IDENTIFICATION WITH IN-GROUP

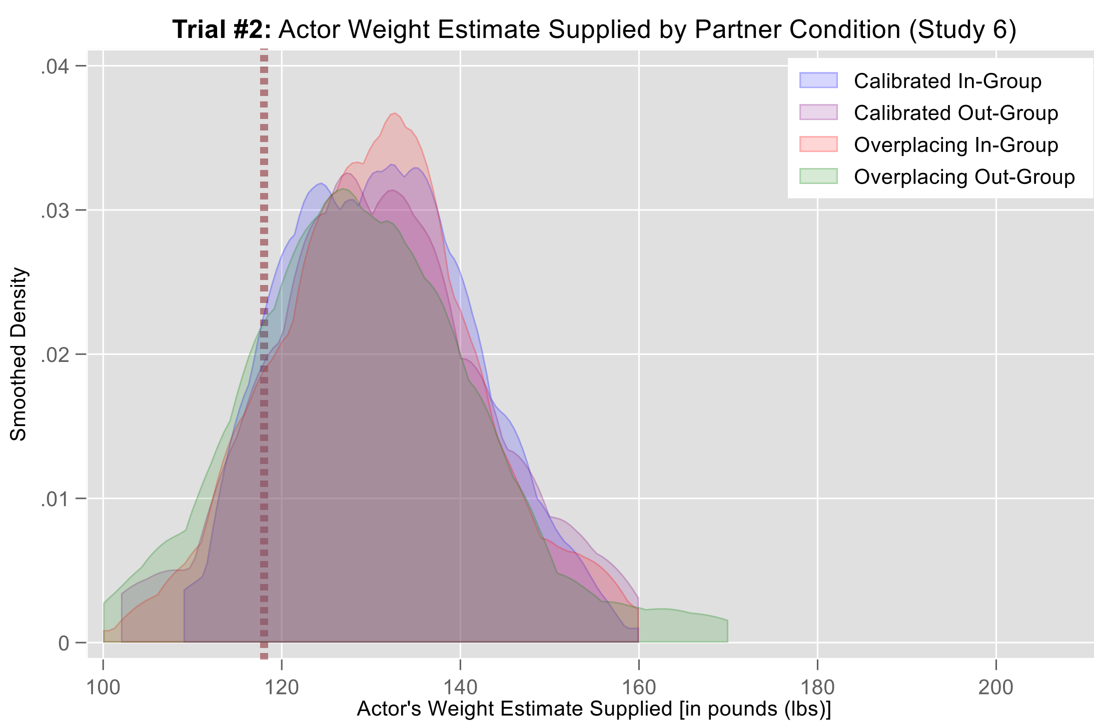
After completing the weight-guessing game, participants reported their demographic details and two individual characteristics that may influence their susceptibility to the coalitional status membership manipulation. First, individuals rated the extent to which they are knowledgeable about football news and events, on a 7-point scale ranging from 1 "Not at all knowledgeable" to 7 "Very knowledgeable". Second, we measured in-group identification using a measure adapted from prior work on identity fusion (Swann, Gomez, Seyle, Morales, & Huici, 2009). Actors were presented with a series of five images that each contain a smaller circle marked "Self" and a larger circle marked "UIUC Student Body". The five images vary in the degree to which the circles overlap. Actors were asked to "select the picture below that best represents the way you perceive your relationship with the general UIUC student body". Responses are scored such that selecting the image depicting the "Self" circle positioned completely outside of the "UIUC Student Body" circle is scored as '1'. In the other extreme, selecting the image depicting the "Self" circle positioned completely within the "UIUC Student Body" circle is scored as '5'. Other selections receive intermediary scores.

SUPPLEMENTAL RESULTS

WEIGHT ESTIMATE RESPONSE

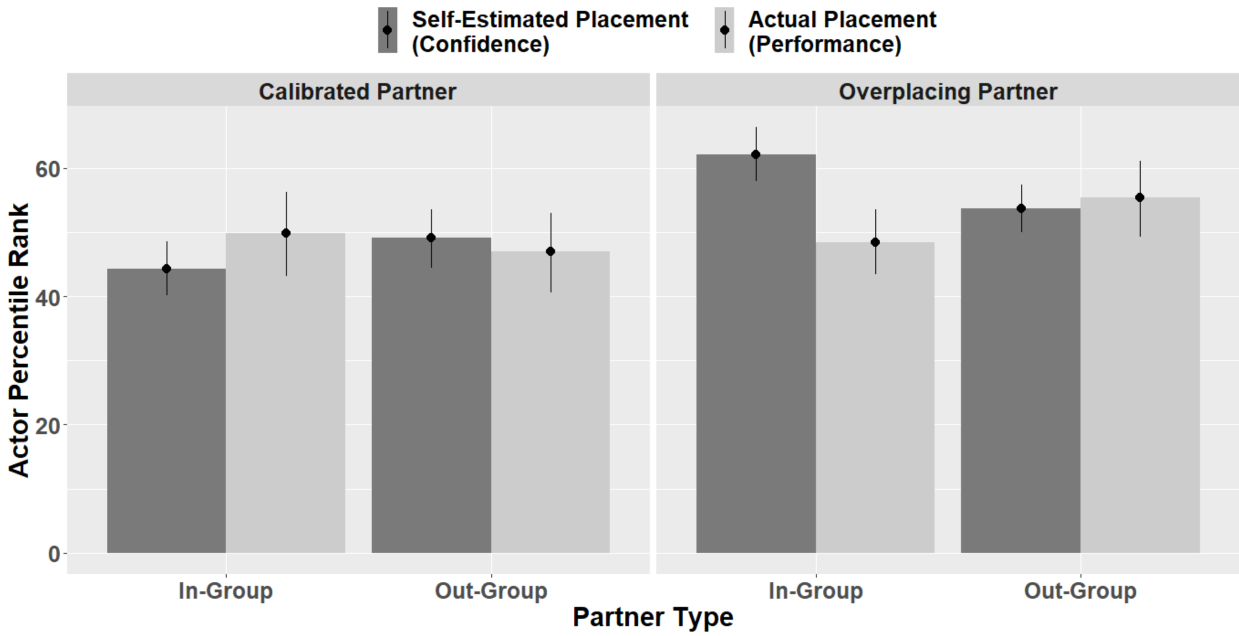
Below, density plots for the weight estimate supplied by actors (i.e., their answer to each trial of the weight-guessing game). These plots show the absence of difference across experimental conditions, suggesting that, while partners shift actors' confidence beliefs, they have little influence over actors' in-game responses and (by implication) performance. The dotted line marks the correct answer (i.e., the actual weight of the individual in the photo).


Trial #1: Actor Weight Estimate Supplied by Partner Condition (Study 6)
Calibrated In-Group
Calibrated Out-Group
Overplacing In-Group
Overplacing Out-Group
Smoothed Density
.025
.02
.015
.01
.005
0
100
120
140
160
180
200
Actor's Weight Estimate Supplied [in pounds (lbs)]


Trial #2: Actor Weight Estimate Supplied by Partner Condition (Study 6)
Calibrated In-Group
Calibrated Out-Group
Overplacing In-Group
Overplacing Out-Group
Smoothed Density
.04
.03
.02
.01
0
100
120
140
160
180
200
Actor's Weight Estimate Supplied [in pounds (lbs)]

SELF-ESTIMATED AND ACTUAL PERFORMANCE DESCRIPTIVES

Bar graphs showing that individuals exposed to an overplacing partner from their in-group are more likely to themselves subsequently overplace. Error bars provide 95% CIs.



INTERNAL META-ANALYSIS

META-ANALYSIS ACROSS STUDIES 2-6: CCMA AND LIKELIHOOD RATIOS

In keeping with emerging best practices for evaluating the replicability of research findings, we conducted two sets of additional meta-analytic analyses aimed at assessing the combined evidential weight of these studies in supporting the overconfidence transmission hypothesis (Asendorpf et al., 2013).[2]

First, we used continuously cumulating meta-analysis (CCMA) to combine the data from Studies 2-6 and compute a meta-analytic index to index the degree of confidence in our conclusion that overconfidence spreads via social transmission. The pooled Cohen's *d* across studies = .6523 (*SE* = .0600), *p* < .00001, 95% CI [.5347, .7699]. On average across studies, individuals exposed to an overplacing peer subsequently overplaced themselves by 17 percentile points higher compared to those exposed to a well calibrated peer, confirming a reliable overconfidence transmission effect across studies.

[2] In these internal meta-analytic analyses, we included all studies (Studies 2-6) with the exception of Study 1. Study 1 deviates substantially from the other experimental studies with its non-experimental design and, relatedly, the suitable analytic approach deployed to test for evidence of overconfidence transmission. That is, in Study 1, we correlated the level of overplacement shown across two participants before vs. after they interact, whereas in Studies 2-6 we compared mean overoverplace across experimental conditions. Such differences limit our ability to make direct comparisons with Study 1.

Second, we used Lakens and Etz' (2017) likelihood ratio test to directly compare how well our research hypothesis (that exposure to overplacing models increases the observer's overplacement) and the null hypothesis (that exposure to overplacing models has no effect on the observer's overplacement) predict the data. This analysis yields a likelihood ratio of 3043168.16. As a benchmark for comparison, likelihood ratios between 1 and 8 indicates weak evidence for the research hypothesis, between 8 and 32 is moderate evidence, and 32 or above is strong evidence. The results we obtained thus supply very strong evidence that the overconfidence transmission research hypothesis is far more likely than the null hypothesis. Specifically, it is over 3 million times more likely to observe a significant result when the transmission effect is true than when there is no true effect.

Overall, these two sets of meta-analytic results converge to indicate that evidence for the overconfidence transmission hypothesis emerging from the current set of studies is highly robust and reliable.

SUPPLEMENTAL DISCUSSION

THE ROLE OF SOCIAL NORMS IN EXPLAINING WITHIN-GROUP SIMILARITIES AND BETWEEN-GROUP DIFFERENCES IN OVERCONFIDENCE

As alluded to in the General Discussion in the main text, beyond transmitted and evoked culture (i.e., responses evoked by environmental conditions), another reason why overconfidence may be particularly transmissible within social groups, and that warrants future research attention, is the existence of norms enforced by social sanctions or punishment. A broad range of findings from across the social sciences indicate that humans possess an evolved willingness to engage in the punishment of individuals who violate social norms (common beliefs, practices, or behavioral standards; (Fehr & Fischbacher, 2004a, 2004b; Henrich et al., 2006; Quervain et al., 2004). This punitive inclination emerges reliably and early in development, and is observed even among preverbal infants and young children (Hamlin, 2013; Hamlin et al., 2011; Mendes et al., 2018). Converging lines of work indicate that the propensity to follow social norms—that is, behavioral standards that are widely shared and enforced by a community—is a robust and innate feature of our species' psychology (Lapinski & Rimal, 2005; Whiten & Ham, 1992). The substantive content and details of norms emerging within a society, however, are culturally constructed (Chudek & Henrich, 2011). Insofar that groups possess culture-specific confidence norms (e.g., social rules about how much or little confidence to express, when and where to express it), which prescribe it in some groups but deplore it in others and are often context- and domain-specific, mechanisms that are known to sustain norms (e.g., reputation, punishment, norm internalization) likely operate to further re-inforce the tendency for social group members to adopt the local group's standards of confidence (even when the norm favors overly optimistic beliefs).

Consistent with this possibility, evidence from field studies and experimental work indicates that violators of local confidence norms—that is, deviation from the standards of the community, both in being overly positive or overly self-critical—accrue bad reputation and are harshly sanctioned (Anderson et al., 2006, 2008; Ridgeway & Berger, 1986). These punishment mechanisms generate selection pressures to attend and conform to local confidence norms (Krueger, 1998), which allow individuals to rapidly and effectively acquire the locally preferred level of confidence, and thus avoid negative consequences from sanctions and reap social benefits of norm-compliance, such as the ability to more effectively coordinate and cooperate with others.

This potent drive to adhere to confidence standards within one's community may combine with our evolved imitative propensity (documented by these studies here) to jointly explain why and how (over)confidence spreads powerfully between interacting individuals and increase phenotypic similarity within communities and social contexts. That is, these incentive mechanisms involving reputational damage and punishment, which have been shown to stabilize a range of behaviors and practices (even when costly for their adopter; DeScioli & Kurzban, 2009; Fehr & Fischbacher, 2004; Henrich & Boyd, 2001), further enforce and strengthen the inherent tendencies to imitate and adopt the local group members' prescribed (over)confidence. Beyond shaping individual-level cognition, this imitative propensity also impacts group-level outcomes. By adopting the same confidence norms as their interaction partners, community members are more likely to resemble one another in their (over)confidence proclivities, thus increasing phenotypic similarity within groups. In sum, imitation, evoked responses, and norm adherence may jointly lead to the spread of overconfidence and sustain the apparent group effects (that is, intragroup similarity and intergroup variability) in biased assessments.

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