

1 Running Head: TRANSMISSION OF OVERCONFIDENCE

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9 **The Social Transmission of Overconfidence**

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46 [https://figshare.com/articles/Social\\_Transmission\\_of\\_Overconfidence/6663200/1](https://figshare.com/articles/Social_Transmission_of_Overconfidence/6663200/1)

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## Abstract

We propose and test the *overconfidence transmission hypothesis*, which predicts that individuals calibrate their self-assessments in response to the confidence others display in their social group. Six studies that deploy a mix of correlational and experimental methods support this hypothesis. Evidence indicates that individuals randomly assigned to collaborate in laboratory dyads converged on levels of overconfidence about their own performance rankings. In a controlled experimental context, observing overconfident peers causally increased an individual's degree of bias. The transmission effect persisted over time and across task domains, elevating overconfidence even days after initial exposure. In addition, overconfidence spread across indirect social ties (person to person to person), and transmission operated outside of reported awareness. However, individuals showed a selective in-group bias; overconfidence was acquired only when displayed by a member of one's in-group (and not out-group), consistent with theoretical notions of selective learning bias. Combined, these results advance understanding of the social factors that underlie inter-individual differences in overconfidence, and suggest that social transmission processes may be in part responsible for why local confidence norms emerge in groups, teams, and organizations.

**Word Count:** 180

**Keywords:** overconfidence, cognitive bias, positive illusions, social transmission, cultural learning

## 67 **The Social Transmission of Overconfidence**

68 Expressions of humility and self-deprecation are plentiful among traditional !Kung  
69 hunter-gatherers of the Kalahari Desert in Southern Africa. For example, members of the society  
70 often minimize the size of their kills, downplay the value of their gifts, speak critically of their  
71 own efforts, and compete in sharing elaborate tales of their own misfortune, whether they  
72 involve hunting failures, pain, thirst, or other hardships (Lee, 1979). Everyone is considered to  
73 be, and considers themselves to be, equals, and a deep-seated sense of modesty is a central  
74 defining feature of life.

75 Thousands of miles away across the Atlantic Ocean, many American corporations are  
76 described in opposite terms. For example, the now infamous energy company Enron was said to  
77 embrace a “culture of arrogance” (Salter, 2008). “There’s no question,” said a former employee,  
78 “that Enron people arrogantly thought they were smarter than everybody else” (Bryce, 2002).  
79 The resulting culture of bravado and overconfidence led Enron to take on increasing risks and  
80 break many laws under the illusion of invincibility, ultimately driving what was the 7<sup>th</sup> largest  
81 company in the United States to collapse.

82 As these examples illustrate, people within groups often show similar levels of  
83 confidence, while different groups can exhibit striking differences. How do these group effects  
84 emerge? Why would people in the same group come to view their own individual skills and  
85 abilities in similar ways? Indeed, empirical evidence similarly points to between-group  
86 differences in normative levels of confidence (Stankov & Lee, 2014; Whitcomb et al., 1995).

87 Little is known about the processes that produce social clustering of overconfidence.  
88 Although a complex set of factors is likely responsible, here we examine one possible  
89 mechanism: social transmission. Social transmission is defined as the process by which attitudes,

90 values, beliefs, and behavioral scripts are passed onto and acquired by individuals and groups  
91 (Cavalli-Sforza & Feldman, 1981; Richerson & Boyd, 2005). We test whether exposure to  
92 others' expressions of confidence (even when it is unwarranted) increases one's own propensity  
93 towards inflated self-assessments. If so, transmission processes that operate on an interpersonal  
94 and micro level might help explain within-group similarities and between-group variation in self-  
95 assessments that appear on a broader macro level. Such a process could explain, in part, how  
96 cultures of overconfidence emerge and persist within social groups and collective entities, as  
97 they did among employees of the former Enron corporation, and not among the !Kung people.

### 98 **Overconfidence: A Prevalent but Also Highly Variable Cognitive Bias**

99 In his landmark work, *The Wealth of Nations* (1776), Adam Smith described the  
100 pervasiveness and havoc of overconfidence, noting that “the over-weening conceit which the  
101 greater part of men have of their own abilities, is an ancient evil remarked by the philosophers  
102 and moralists of all ages” (p. 109). More than two centuries later, this observation has  
103 accumulated extensive support. Many of us are prone to exaggerating the degree to which our  
104 talents and capabilities are superior to those of others (Dunning et al., 2004; Langer, 1975;  
105 Murray et al., 2017; Weinstein, 1980). Such miscalculations can, of course, lead to disaster.  
106 Overconfidence contributes to a vast range of problems, from global disasters such as world wars  
107 and global financial crises, to corporate collapses, investment failures, and costly legal battles.  
108 All these phenomena are rooted in faulty decisions brought on by an exaggerated placement of  
109 oneself above others (Berner & Graber, 2008; Grinblatt & Keloharju, 2009; Meikle et al., 2016;  
110 Moore et al., 2015; Ortoleva & Snowberg, 2015; Zacharakis & Shepherd, 2001). This has led  
111 modern thinkers to echo similar sentiments about Smith's “ancient evil.” Nobel Laureate Daniel

112 Kahneman famously remarked that if he had a magic wand that could change just one thing  
113 about human psychology, he would eliminate overconfidence (Shariatmadari, 2015).

114         Yet, despite the apparent pervasiveness of overconfidence, comparative evidence  
115 indicates variation across groups and societies in the degree of overconfidence bias. Whereas  
116 some communities appear to have a general tendency of false and exaggerated beliefs across a  
117 broad range of domains, others appear to lean towards accurate or even underconfident beliefs  
118 (Heine et al., 1999; Heine & Hamamura, 2007; Johnson, 2004; Muthukrishna et al., 2018; Schulz  
119 & Thöni, 2016; Sedikides et al., 2015; Whitcomb et al., 1995, 1995; Yates, 2010). Within  
120 societies, subgroups and organizations also vary systematically in overconfidence. In a study that  
121 compared the self-assessments of current employees in the banking and trading sectors against  
122 that of a sample of students on track to gaining employment in those same sectors, although both  
123 groups were overconfident about their knowledge of finance, current employees were relatively  
124 more biased (Glaser et al., 2005). Crucial to their design is the comparison of current employees  
125 with students specializing in the same sectors, as this provides a control for personality or trait-  
126 based self-selection into career tracks (Schulz & Thöni, 2016). Similar patterns of cross-group  
127 variability have long been shown in organizations and work teams, revealing examples of  
128 organization- and firm-specific cultural climate, norms, and values (Deshpande & Webster,  
129 1989; Kanter, 2004; Schein, 1990). These lines of evidence converge to indicate that social  
130 entities can vary in their propensity towards overconfidence—from small local clubs and teams,  
131 to broad economic and professional sectors and communities, to large-scale nations and  
132 populations.

### 133 **How Do Group Effects in Overconfidence Emerge?**

134 How do these within-group similarities in overconfidence emerge and persist over time?

135 Multiple mechanisms are likely at play. In part, group effects might emerge in response to  
136 different ecological circumstances that differentially reward (or penalize) competitive behavior  
137 fomented by overconfidence (an issue we return to in the discussion; K. Hill & Hurtado, 1996;  
138 Talhelm et al., 2014; Tooby & Cosmides, 1992; Triandis, 1994). Overconfidence may increase  
139 (that is, be “evoked”) in environments in which inflated assessments may confer net advantages  
140 (Haselton et al., 2015; Johnson et al., 2013; Johnson & Fowler, 2011; Schwardmann & Weele,  
141 2019; Sharot, 2011, 2012). For instance, in American corporations, the rewards from an  
142 overconfident strategy might outweigh the costs of its risks (Harner, 2010).

143 However, it has long been recognized that such explanations of cultural variation that  
144 emphasize “evoked culture” alone are insufficient for explaining the full variation in our  
145 psychological and behavioral repertoires. Our species’ unique ability to learn from others is also  
146 a powerful driving force of cultural variation (Boyd et al., 2011; Boyd & Richerson, 1985;  
147 Henrich, 2016; Mesoudi, 2009; Richerson & Boyd, 2005). Humans learn everything from  
148 walking and language to affective responses and decision preferences from the people around us.  
149 The immense body of research on cultural transmission focuses on how the propensity to learn  
150 from and to imitate conspecifics enables humans to learn a range of behaviors, beliefs, values,  
151 preferences, and mental representations from others (Pinker, 1997). These abilities enable  
152 complex institutions and technologies from bows and arrows, fire-making tools and  
153 paraphernalia, to religion and normative monogamy (Nielsen & Tomaselli, 2010). This immense  
154 reliance that humans place on social learning, when coupled with specialized transmission biases  
155 (e.g., preferentially learning from in-group members, adopting traits that are most common),

156 explains the emergence and persistence of both similarities within, and differences between,  
157 groups and cultures.

### 158 **The Social Transmission of Overconfidence**

159 To more fully understand why and how similarities in overconfidence can arise among  
160 people within groups, we draw from work on cultural transmission. We propose that, similar to a  
161 wide array of cultural traits, overconfidence transmits socially; observing an expression of  
162 confidence (whether it reflects a case of justified confidence or a case of overconfidence)  
163 increases an individual's own confidence, and thus results in a greater tendency toward  
164 overconfidence. Through social transmission, then, members within a group may acquire an  
165 increased (or decreased) propensity for confidence from others. In turn, convergence develops  
166 among actors within groups in the degree to which they form inflated self-assessments. If  
167 overconfidence transmits from one person to another, this process may operate across a large  
168 number of individuals and generate group-wide overconfidence by allowing the bias to cascade  
169 broadly. Such a process would be consistent with evidence that a small subset of particularly  
170 influential or visible members (such as leaders and high-status individuals) can shift their  
171 broader community's behavioral climate through social transmission (Paluck et al., 2016; Paluck  
172 & Shepherd, 2012).

173 At the core of this hypothesis of overconfidence transmission is the notion of phenotypic  
174 transmission: the degree of inflated beliefs in any given individual is influenced by the  
175 overconfidence of one's social partners (peers). As an initial foray into this question, here we  
176 seek to first document evidence of such a pattern of phenotypic transmission (overconfidence  
177 *can* spread), without attempting to pinpoint the specific proximate mechanisms that might  
178 generate this transmission (*how* this transmission occurs), owing in part to the well-known

179 difficulty of empirically distinguishing between the mechanisms responsible for social  
180 transmission processes (Quispe-Torreblanca & Stewart, 2019).<sup>1</sup>

181         Establishing whether overconfidence can transmit socially between interactants is  
182 important on both theoretical and practical grounds. Theoretically, social transmission may be  
183 particularly important for explaining cases in which evoked cultural explanations fall short. For  
184 example, why do groups that inhabit quite similar regions or social environments sometimes  
185 show striking differences (Mesoudi et al., 2006) see also (Andersen et al., 2013; Apicella et al.,  
186 2014; Henrich & Boyd, 1998; Mesoudi et al., 2006)? We suggest that people’s propensity to  
187 align their values and beliefs with other members of the group can in part explain how these and  
188 other within-group similarities and between-group differences in confidence norms emerge and  
189 are maintained.

190         On a practical level, if overconfidence spreads and can scale up to create group-wide  
191 overconfidence, a key implication is that this produces groups with rampant overconfidence that  
192 may then be especially vulnerable to risky decision-making. In these groups, there is a shortage  
193 of individuals with unbiased (or underconfident) beliefs who can counterbalance extremely  
194 inflated views and “put the brakes” on risky and hazardous decisions. Moreover, individual  
195 errors in judgment, which in many cases may be inconsequential on their own, can aggregate or  
196 interact with errors committed by others to create potentially disastrous consequences (Sharot,  
197 2011; Smaldino, 2014). Examples of large-scale faulty decision-making in groups imbued  
198 broadly with a “culture of overconfidence” abound in history, from the risky decisions made by

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<sup>1</sup> We speculate that—as in many other psychological mannerisms shown to be malleable to social influence—overconfidence transmission in the real-world is likely to involve some combination of conformity (i.e., adopting the local social norm, by copying a prevalent mannerism; Henrich & Boyd, 1998) or unbiased (random) imitation (i.e., adopting a mannerism regardless of its observed frequency; Boyd & Richerson, 1995), social pressure (i.e., fear of potential sanctioning for deviant, norm-violating behavior; Rakoczy, Warneken, & Tomasello, 2008), and prestige-biased learning (i.e., adopting the mannerisms shown by a presumably prestigious person, such as someone who appears confident; C. Anderson, Brion, Moore, & Kennedy, 2012; Chudek, Heller, Birch, & Henrich, 2012).



199 many financial firms leading to the 2008 financial collapse, to the political decisions of a  
200 country's top leaders and their states that precipitate entry into a disastrous war. Thus, given its  
201 effects on catalyzing group-wide overconfidence and risky decision-making, empirical tests of  
202 whether social transmission can spark or exacerbate biased assessments are worthwhile.

203         Finally, an empirical test of whether overconfidence may transmit socially is important  
204 because, although a variety of traits, behaviors, and mannerisms can transmit between  
205 individuals, not everything does. Furthermore, it is even possible that exposure to others'  
206 (over)confidence may actually suppress (rather than increase) confidence. This possibility, which  
207 is antithetical to the overconfidence transmission hypothesis, derives from the concept of  
208 dominance complementarity (for a review, see Horowitz et al., 2006). This complementarity  
209 principle proposes that displays of assertiveness and dominance, to which confident assessments  
210 are linked (Gough et al., 1951; Wiggins, 1979), evoke an opposite, reciprocal behavioral pattern  
211 characterized by submissiveness and deference. These complementarity effects, which have been  
212 empirically documented across a wide range of contexts and domains (Markey et al., 2003;  
213 Thomsen et al., 2011; Tiedens et al., 2007; Tiedens & Fragale, 2003; Zitek & Tiedens, 2012),  
214 may provide coordination benefits by reducing costly conflict over relative dominance ranking  
215 (Tiedens & Fragale, 2003). Accordingly, this pattern raises the possibility that not only  
216 (over)confident beliefs resist transmission, but that observing expressions of confidence may  
217 give rise to *less* confidence, and encourage associated cognitive states such as modesty and  
218 submissiveness (Tiedens & Jimenez, 2003). Given this logically plausible alternative account, in  
219 the present research we aim to consider both possibilities and test whether overconfidence  
220 foments social transmission or complementarity (for an expanded discussion on dominance  
221 complementarity, see Supplemental Materials).

## 222 **The Present Research**

223 The goal of the present research is to provide the first systematic test of the social  
224 transmission account of overconfidence outlined above. Here we test the *overconfidence*  
225 *transmission hypothesis*, or the idea that witnessing confidence in others (even when these  
226 assessments are overly positive) increases in the observer a propensity towards overconfidence.  
227 We posit that individuals readily acquire the confidence level expressed by others. Observing  
228 highly confident models can elevate observer confidence and, along with it, the likelihood of  
229 overconfidence.<sup>2</sup>

230 Theorists distinguish three varieties of overconfidence (Moore & Healy, 2008): (a)  
231 *overestimation* is the belief that you are better than you actually are (e.g., thinking that you  
232 answered 8 of 10 questions correctly when you in fact only got 3); (b) *overprecision* is excessive  
233 faith in the accuracy of your beliefs (e.g., being 100% convinced that you got 8 questions right,  
234 when you didn't); and (c) *overplacement* is the exaggerated belief that you are better than others  
235 (e.g., believing that your score on the test ranks top in the class when in fact you scored second  
236 last). The present research studies *overplacement*, both because it has been the focus of much of  
237 the literature in social psychology and economics and because beliefs about relative placement  
238 are so consequential, from starting a business to applying for a job. For example, evidence  
239 indicates that the decision to start a business is driven by the often biased belief in the likelihood  
240 of coming out ahead of the competition (that is, entrepreneurs falsely believing that they will  
241 outperform their competitors; Astebro, Herz, Nanda, & Weber, 2014; Camerer & Lovallo, 1999).

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<sup>2</sup> Importantly, we emphasize that this theoretical account also applies to *underconfidence*. It is predicted that, in a similar process, a model who expresses little confidence may be emulated, thus lowering confidence and increasing the chances of *underconfidence* on the part of the learner. However, likely because overconfidence increases the risk of costly decisions, its existence has generated greater scientific interest; in the current research we thus chose to focus on explaining overly positive, rather than overly negative, beliefs. We return to this issue in the General Discussion.

242 We avoid single-item confidence judgments that ask participants to estimate the probability of  
243 getting one item correct. Although they are employed frequently in the decision-making  
244 literature, they tend to confound overestimation with overprecision, limiting their usefulness for  
245 our purposes (Moore & Healy, 2008).

246 Little work has examined the social transmission of (over)confidence, despite interest in  
247 this theoretical possibility (Johnson & Fowler, 2011). In the only relevant study we know of,  
248 Paese and Kinnaly (1993) asked participants to complete a knowledge test and indicate their  
249 certainty in the accuracy of each answer. Participants then received a (fictitious) peer's test  
250 responses, which included the peer's answers and certainty of being correct for each answer. In  
251 actuality, the peer's response accuracy and certainty were independently manipulated. While  
252 able to view the peer's answers, participants then completed the exact same knowledge test and  
253 again indicated their certainty for each answer. The authors found that participants who observed  
254 an overconfident peer (i.e., a peer with high confidence but low accuracy) became more  
255 overconfident (that is, more positively biased) on the repeated test, compared to if they viewed  
256 other types of peers.

257 We note two shortcomings of this study. First, by soliciting confidence in accuracy at the  
258 item level, their measure of overconfidence confounds overestimation and overprecision (Moore  
259 & Healy, 2008). Second, in the repeated test, participants actually showed a tendency to rely on  
260 peer input, readily revising their own answers by copying the peer's answers on the knowledge  
261 test. Given that the self-assessments elicited on the second test captured their confidence in the  
262 peer's answers, these assessments in principle conflated confidence in one's own answers with  
263 confidence in the peer's. It is therefore unclear whether participants' changes in beliefs reflected  
264 increased overconfidence in their own abilities or simply greater confidence in the *peer's*

265 answers. Consistent with this possibility, in an exit survey completed at the end of the  
266 experiment, participants in the overconfident-peer condition rated their partner as *more*  
267 knowledgeable, suggesting that they indeed placed greater confidence in this overconfident peer.  
268 Given the conceptual ambiguity, these results are inconclusive as to whether and what kind of  
269 overconfidence spreads socially. The current research, by proposing and testing a framework for  
270 understanding the clustering of overconfidence—by isolating and focusing on overplacement in  
271 particular—aims to fill this gap.

## 272 **Overview of Studies**

273         We report six studies designed to test the overconfidence transmission hypothesis as it  
274 applies to the case of overplacement. If overconfidence spreads interpersonally, we expect that  
275 individuals who witness or interact with others who overplace will subsequently demonstrate  
276 greater overplacement. Study 1 utilized a correlational design to test whether two previously  
277 unacquainted individuals who are randomly assigned to collaborate on a laboratory task  
278 converge in overplacement. Studies 2 through 6 employed experimental methods to further probe  
279 the causal process by which overplacement transmits. Drawing on prior experimental work  
280 designed to examine how “information cascades” from one person to another in the laboratory  
281 via social learning (L. R. Anderson & Holt, 1997; Fowler & Christakis, 2010; McElreath et al.,  
282 2005), our approach in these subsequent studies involves presenting individuals with information  
283 about other participants’ self-assessed rank and actual rank. Then, we examine how this  
284 information alters peoples’ beliefs about their own rank (a form of peer-to-peer transmission).

285         Analytically, to calculate the discrepancy between self-estimated placement and actual  
286 placement, we simply subtracted actual from estimated placement for all tests of mean  
287 differences. For tests of covariation involving overplacement, we used the residuals when

288 regressing self-estimated placement on actual placement, which capture aspects of beliefs that  
289 cannot be explained by true performance, consistent with existing approaches (C. Anderson et  
290 al., 2012; Cronbach & Furby, 1970; Dubois, 1957; John & Robins, 1994a; see Supplemental  
291 Materials for expanded discussion on calculating discrepancy).

292         The reasoning outlined above predicts that observing an overplacing peer should  
293 increase individuals' own overplacement, even on a novel set of judgments (beyond the same set  
294 of judgments made by the peer; (cf. Paese & Kinnaly, 1993). We hypothesize that this  
295 transmission process stems from a general tendency to align one's level of confidence to that  
296 witnessed in others, both when these self-assessments are warranted and unwarranted (and thus  
297 overplacement; our Study 4). We explore several key aspects of the transmission process that  
298 facilitate its spread. This includes examining whether overplacement transmits (a) across indirect  
299 social ties—that is, from person to person to person—to create a cascade effect (Study 3); (b)  
300 across time and domains, such that the effect of overplacing models persists several days after  
301 initial exposure, and “spills over” to influence self-assessments in a novel, unrelated task (Study  
302 4); and (c) selectively within coalitional groups, such that overplacing models influence  
303 exaggerated self-assessed rank only when expressed by in-group but not out-group members,  
304 consistent with selective learning that allows individuals to acquire the most self-relevant  
305 behaviors and practices (Henrich & Broesch, 2011; Henrich & Henrich, 2007). Our studies, with  
306 their diverse approaches and research questions, provide a systematic investigation of the  
307 existence and nature of overconfidence transmission.

308         The data for all present studies are archived and available at  
309 [https://figshare.com/articles/Social\\_Transmission\\_of\\_Overconfidence/6663200/1](https://figshare.com/articles/Social_Transmission_of_Overconfidence/6663200/1). The  
310 procedures for data collection in these studies were approved by the Institutional Review Board

311 (IRB) for the Protection of Human Subjects at the University of California, Berkeley, the  
312 University of British of Columbia, and the University of Illinois at Urbana-Champaign.

### 313 **Study 1: Overconfidence Spreads in Assigned Dyads in the Lab**

314 Study 1 sought to test whether overplacement spreads between randomly paired  
315 individuals in the laboratory. To distinguish *overplacement*—falsely inflated self-assessed rank  
316 that exceeds what is warranted by actual rank—from true placement that is deservedly rooted in  
317 superior relative performance (Heck & Krueger, 2015; Humberg et al., 2018; Moore & Healy,  
318 2008), here and in all our studies below, we deployed tasks that yield objective performance  
319 indices. Analytically, we operationalize overplacement as the degree to which self-estimated  
320 placement exceeds actual placement.

321 Participants attended a laboratory session and individually completed a task in which they  
322 guessed the personality traits of target individuals from photographs and then estimated their  
323 own individual placement rank (i.e., relative performance) on the task (C. Anderson et al., 2012).  
324 Participants were then randomly paired with another person with whom they had no prior history  
325 to collaborate on a variation of the same task. Finally, participants revisited their initial  
326 performance judgment and estimated their individual rank again. Overplacement on these two  
327 occasions was measured by computing the discrepancy between estimates of own relative  
328 performance and actual scored relative performance in the task. We expected members of a dyad  
329 to show greater convergence in their overplacement after the collaboration, compared to before.  
330 Because random assignment precludes the possibility of homophily often observed in the real  
331 world (that is, individuals preferentially connecting with more similar others; McPherson, Smith-  
332 Lovin, & Cook, 2001), a positive association between members' overplacement post-  
333 collaboration would indicate that members influence each other over the course of the

334 collaboration to create a convergence in their overplacing tendency. That is, individuals within  
335 the same dyad will become more similar to each other than to individuals in other dyads.

## 336 **Method**

### 337 **Participants.**

338 One hundred and four undergraduate students (59% women; 8 participants did not report  
339 gender) at a large public university in the U.S. participated. We sought to recruit at a minimum  
340 of one hundred participants, consistent with prior work on overconfidence in dyads (C. Anderson  
341 et al., 2012). A power analysis that assumes an effect size of  $r = .40$  (to capture convergence  
342 between members of a dyad), with an alpha level of .05 and power of .80 suggests sampling 94  
343 participants (or 47 dyads). We terminated data collection at the end of the academic semester in  
344 which this target sample size was reached. Participants' ages ranged from 19 to 39 ( $M = 21.94$ ,  
345  $SD = 2.82$ ; 12 participants did not report age). All participants received partial course credit for  
346 their participation.

### 347 **Material and procedure.**

348 Sessions included 4-8 participants, paired randomly into 52 dyads of variable gender  
349 composition. After arriving to the laboratory, participants sat at individual computer stations and  
350 learned that the study consisted of two parts: an individual component and a dyadic component.  
351 In both components, they would guess the personality of target individuals from photographs  
352 shown on the computer screen. Each target would be rated on ten traits from 1 (*Does not*  
353 *describe this person at all*) to 7 (*Describes this person very well*). Participants were informed  
354 that a rating was considered correct if it was within .50 points above or below the target's "true"  
355 personality, which was operationalized as the actual average rating made by the target and eight

356 knowledgeable informants who were friends or coworkers.<sup>3</sup> To incentivize attention and task  
357 engagement, the dyad with the highest number of correct answers on the dyadic component  
358 received a \$200 cash prize.

359         In the individual phase, participants independently judged photos of ten targets. They  
360 then reported their confidence, in the form of a numeric value between 1<sup>st</sup> and 99<sup>th</sup> percentile to  
361 capture their self-estimated placement (*relative performance*), compared to other students at the  
362 university. This variable indexes estimated placement exhibited before the dyadic component.

363         Participants then proceeded to the dyadic phase. Each participant was randomly paired  
364 with another who we verified was an unacquainted stranger. Seated together at an assigned  
365 computer workstation, dyads worked together for 15 minutes to guess the personalities of five  
366 new targets. After the dyadic task, participants returned to their individual workstations and  
367 provided a second, retrospective estimate of their own independent performance in the individual  
368 component. They completed the same self-estimated placement measure, though with slightly  
369 adapted instructions (e.g., “Now that you have completed the entire task, compared to the  
370 average undergraduate at this university, where do you think your original judgments that you  
371 made alone rank in terms of accuracy?”).<sup>4</sup> This serves as a measure of estimated placement after  
372 the dyadic collaboration.

373 **Key variables: Overplacement pre- and post-collaboration.**

374         Participants’ overplacement before and after the collaboration were determined as  
375 follows. We began by scoring whether their answers were correct in the manner described to

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<sup>3</sup> The ten target photos used in this personality guessing game were taken from a larger pool of stimuli materials obtained from Daniel Ames, and were used in Anderson, Brion, Moore, and Kennedy (2012).

<sup>4</sup> Prior work indicates that overconfidence in one’s own performance is both conceptually and empirically distinct from overconfidence in the performance of one’s group (Healy & Pate, 2011; Klar & Giladi, 1997). Guided by these studies, we assessed participants’ post-collaboration overplacement using confidence in their own placement (rather than their group’s placement). This allowed us to directly compare convergence pre- and post-collaboration.



376 them, using the “true” personality of the target as the criterion. The total number of correct  
377 personality judgments made by each participant (out of all 100 judgment items across all 10  
378 targets;  $M = 16.89$ ,  $SD = 5.61$ ) was taken as their actual performance. We then computed each  
379 person’s actual placement (*relative* performance) among all participants by transforming the  
380 number of correct items into relative percentile rankings (with ties allowed), such that those who  
381 answered more questions correctly had higher percentile rankings.<sup>5</sup>

382 Finally, Study 1 operationalized overconfidence as the degree to which self-estimated  
383 placement exceeds actual placement. Conceptually, this measure captures the exaggerated belief  
384 that one is better than others, beyond what is justified by true performance. Because we assessed  
385 beliefs in *relative* (i.e., estimated rank relative to others) rather than in *absolute* terms (i.e.,  
386 estimated score), this measure assesses the biased belief that one is better than others. For  
387 example, a student might think she ranks top of class if the rest of the class is seen as weak, but  
388 she may still think she ranks at the top even if she finds the other students collectively strong  
389 (and all of these students can be ranked relative to each other, starting at the second place). Put  
390 differently, in a class of 100 students, the student with the 50<sup>th</sup> rank always has the median  
391 performance, regardless of whether the class as a whole is weak or strong. Moreover, by holding  
392 constant across conditions the partner’s actual placement (their performance), we are able to  
393 ascertain that any differences in actor overplacement across conditions results from our actors’  
394 inflated self-assessments rather than underplacing others’ ability (from inferring that others  
395 perform poorly). Our measure of overplacement is commonly used in research on  
396 overconfidence (e.g., C. Anderson et al., 2012; Belmi, Neale, Reiff, & Ulfe, 2019; Ehrlinger,

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<sup>5</sup> Interestingly, individuals had little insight into their actual placement on this task; self-estimated placement was not associated with actual placement either before the collaboration ( $r = .02$ ,  $p = .869$ ) or after the collaboration ( $r = -.09$ ,  $p = .387$ ), consistent with the weak or null association often observed between ability and confidence in many domains (Alba & Hutchinson, 2000; Pallier et al., 2002).

397 Mitchum, & Dweck, 2016; Emich, 2014; Friehe & Pannenberg, 2019; Muthukrishna et al.,  
398 2018).

### 399 **Results and Discussion**

400 To examine whether overplacement converges between individuals in a social  
401 interaction, we first examined the association between the two partners' overplacement, both  
402 before and after the dyadic component, across dyads. Results indicate that, before the dyadic  
403 task, correlation between the dyad partners' overplacement levels, though negative, did not reach  
404 statistical significance ( $r = -.12$ , 95% CI [-0.379, 0.160]),  $p = .404$ ,  $n = 52$  dyads). However, after  
405 the 15-minute dyadic interaction, dyad partners' overplacement levels became positively and  
406 significantly correlated, ( $r = .32$ , 95% CI [0.048, 0.547],  $p = .022$ ,  $n = 51$  dyads).<sup>6</sup> These pre- and  
407 post-collaboration dyad-level overplacement correlations differ significantly from each other ( $Z$   
408  $= 2.22$ ,  $p = .027$ ). Our follow-up analyses show that these results are robust to controls for  
409 participant gender and the dyad's joint performance, which indicate that the convergence  
410 observed between dyad members' overconfidence is not dependent on (i.e., moderated by)  
411 whether they performed well or poorly (which might have altered both partners' self-estimated  
412 placement, creating convergence; see Supplemental Materials).

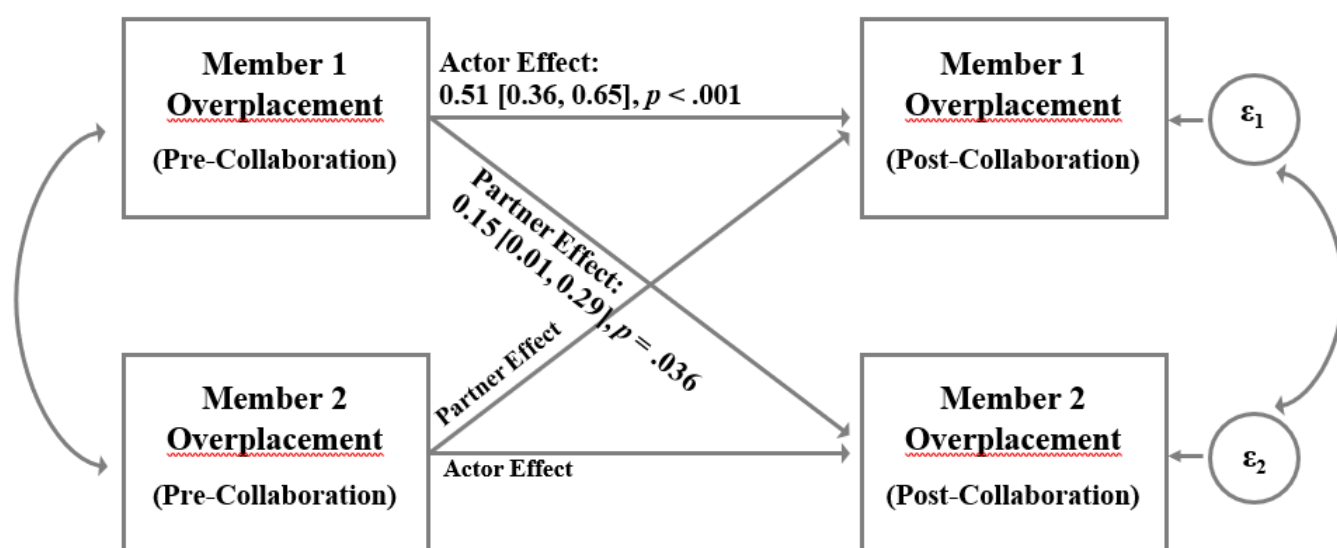
413 What then explains the similarity between dyad members' overplacement? To directly  
414 examine whether this within-group similarity results from social transmission, we adopt the  
415 actor-partner interdependence model (APIM; Kenny & Kashy, 2014; Kenny, Kashy, & Cook,  
416 2006) to tease apart the temporal processes underlying these dyadic data. Using this model we  
417 explore whether members' post-collaboration overplacement is predicted by their partner's pre-

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<sup>6</sup> One participant provided incomplete data in the post-collaboration phase and was thus dropped. This also necessitated dropping the corresponding partner in the dyad unit. This leaves 102 participants from across 51 dyads.

418 collaboration overplacement (a peer effect), controlling for their own pre-collaboration  
 419 overplacement tendency (within-person stability). This model accounts for statistical dependency  
 420 between dyad members' post-collaboration overplacement outcomes, which allows us to avoid  
 421 violating the assumption of independence in standard regression models. Figure 1 depicts the  
 422 APIM, and the corresponding multilevel model results estimated using the 'nlme' package  
 423 (Pineiro et al., 2019) in R.

424 **Figure 1. Overplacement post-collaboration as explained by the actor-partner**  
 425 **interdependence model (Study 1).**  
 426



427  
 428 *Figure 1.* Overplacement post-collaboration explained by the actor-partner interdependence  
 429 model (APIM; Kenny & Kashy, 2014; Kenny, Kashy, & Cook, 2006) for indistinguishable  
 430 dyads. The predictor variables are overplacement pre-collaboration for member 1 and member 2,  
 431 the outcome variables are overplacement post-collaboration for both members, and residual  
 432 variances (error terms) are modeled. The effect of a member's pre-collaboration overplacement  
 433 on her own overplacement post-collaboration is an actor effect. The effect of a member's pre-  
 434 collaboration overplacement on the partner's post-collaboration overplacement is a partner  
 435 effect. Dyad members are treated as indistinguishable, given a lack of systematic or meaningful  
 436 difference for designating who is member 1 and who is member 2 (the numbering is randomly  
 437 assigned); thus, actor and partner effects are constrained to be equal across members, such that in  
 438 the model only one actor effect and one partner effect are estimated. The statistically significant  
 439 partner effect in this model is consistent with social transmission of overplacement from one  
 440 member of a dyad to another.

441 Results of this APIM analysis support the social transmission hypothesis, revealing that  
442 members' post-collaboration overplacement is jointly predicted by their own initial  
443 overplacement and their partner's baseline overplacement beliefs, as measured prior to  
444 collaboration. Indicating intra-person consistency (an actor effect), a member's initial degree of  
445 overplacement pre-collaboration positively predicts his own subsequent, post-collaboration  
446 overplacement ( $b = .51$ , 95% CI [.360, .649],  $\beta = .58$ ,  $p < .001$ ). Beyond this, however, partners  
447 also exert a unique effect on actor beliefs over and above this temporal consistency in people's  
448 biased beliefs. Consistent with evidence of cross-person social transmission (a partner effect),  
449 partner overplacement at baseline predicts actor post-collaboration overplacement ( $b = .15$ , 95%  
450 CI [.012, .293],  $\beta = .18$ ,  $p = .036$ ). By controlling for the stability of an actor's tendency to hold  
451 biased beliefs, we are able to isolate the unique effect of partner beliefs and infer that social  
452 transmission explains the focal actor's change in overplacement (from pre- to post-interaction)  
453 above and beyond the temporal stability of these beliefs. Together, these results show that having  
454 a more overplacing partner predicts an increase in one's own level of bias.

455 **Summary.** Results from Study 1 suggest that individuals demonstrate an increased  
456 tendency towards overplacement when their partner overplaces, consistent with the  
457 overconfidence transmission hypothesis. While we are unable to make strong inferences of  
458 causality from these correlational data, we find evidence that after working together, initially  
459 non-similar strangers became more similar to each other in overplacement, suggesting the  
460 convergence of overconfidence. Importantly, the use of random assignment of partners in a  
461 controlled laboratory rules out the possibility that the observed convergence results from the  
462 tendency to affiliate with similar others, or from shared exposure to contextual factors prior to  
463 participating that shaped both individuals' psychology, both of which are processes that

464 commonly operate in the real-world and thus are difficult to rule out otherwise. Nevertheless, our  
465 subsequent studies adopt an experimental approach by testing whether individuals align their  
466 self-estimated placement with those seeded in a social partner, and by doing so will provide an  
467 effective means of testing whether overplacement transmits under more controlled experimental  
468 conditions.

### 469 **Study 2: Overplacement Spreads from Person to Person**

470 Although Study 1 established the convergence in overplacement among interacting  
471 individuals, observational studies such as these make strong causal inferences about peer  
472 influence effects difficult (Aral et al., 2009; Bond et al., 2012). For instance, although Study 1  
473 randomly assigned dyads and thus precludes the possibility of inherent similarities between  
474 partners creating correlated overplacement patterns, shared exposure to local experiences over  
475 the course of collaboration (e.g., a pleasant, collaborative working climate within the dyad;  
476 McPherson et al., 2001) may nevertheless cause the two members to make correlated  
477 assessments, creating convergence in overplacement. Study 2 thus used an experimental design  
478 to gain greater internal control over the content of transmissible information, restricting  
479 information to only the partner's self-assessments, in order to allow for clearer causal inference.  
480 Random assignment to partners who vary in self-assessments means that any relationship  
481 between the type of partner observed and the observer's self-estimated placement is due to  
482 neither inherent similarities in their characteristics nor to shared experiences during the social  
483 interaction, both of which are uncorrelated with the experimental treatment. To directly measure  
484 peer influence effects, we compared the overplacement of participants exposed to a partner who  
485 expressed substantial overplacement against that of participants exposed to a partner who  
486 demonstrated little to no overplacement.

487           Three features of this study are noteworthy. First, participants learned the extent of their  
488 partner's overplacement via clear and explicit information about the partner's self-estimated  
489 placement and actual placement. Second, we deployed incentives that encouraged calibration and  
490 discouraged over- and under-placement, so as to parallel the many (though admittedly not all)  
491 occasions in life in which unbiased decisions confer an advantage (Cain et al., 2015; Neale &  
492 Bazerman, 1985; Tenney et al., 2007). Together, these two features create a tougher test of the  
493 overconfidence transmission hypothesis. If individuals indeed acquire biased beliefs from merely  
494 being exposed to overplacing partners—despite clear information that the partner has overplaced  
495 and despite incentives that favor accurate placement—it would suggest that overplacement can  
496 spread even from a social partner who is known to hold biased beliefs. Third, we assessed  
497 participants' estimated placement in each of their guesses and determined their mean  
498 overplacement bias by aggregating across the level of overplacement displayed in all trials. Thus,  
499 we relied on multiple reports of estimated placement and overplacement, rather than a single  
500 post-task retrospective report.

## 501 **Method**

### 502 **Participants.**

503           Through a campus-wide solicitation at a large public university in Canada, we recruited  
504 425 participants (65.25% women) for an in-person computerized study on judgment and  
505 decision-making. This sample size was determined based on a power analysis in which we  
506 assumed an effect size of  $d = .35$  (equivalent to  $r = .17$ ), using an alpha level of .05 and power  
507 of .80, which suggests sampling 130 participants in each of three conditions (targeted  $N = 390$   
508 combined). Data collection terminated at the end of the week in which we attained the target  
509 sample size. Participants' ages ranged from 16 to 56 ( $M = 21.27$ ,  $SD = 3.59$ ). We informed

510 participants that their responses may be presented to future participants (for the purposes of  
511 Study 3; see below), but that their identities and other demographic information would remain  
512 confidential. Analyses below include data from all participants.

513 **Experimental procedure.**

514 After giving consent, participants read on-screen instructions that they would guess the  
515 weight of a number of target individuals from photographs shown on the computer screen, by  
516 entering a numerical value in pounds. They also read that, after each guess, they would indicate  
517 their estimated placement (relative rank) in the accuracy of that guess. Participants who preferred  
518 thinking in kilograms received a table that converted kilograms to pounds and vice-versa. To  
519 incentivize calibrated (rather than overconfident) self-assessments, the top five scorers in the  
520 task—whose weight and estimated placement were the most accurate—were entered into a \$30  
521 raffle. Thus, participants maximized their potential earnings by guessing the correct weight *and*  
522 avoiding both over- or under-placing their performance.

523 After receiving these instructions, participants (hereafter termed “actors”) were presented  
524 with the answers that a “previous, randomly selected respondent” (“partner”) purportedly  
525 provided. Actors learned that the partner’s responses were presented merely as an example and  
526 may or may not be helpful towards their own performance in the task. More specifically, for each  
527 of the two “sample” trials, actors viewed the full-body photograph that the partner had seen,  
528 followed by the partner’s purported: (a) weight estimate (in lbs); (b) self-estimated placement, in  
529 the form of a numeric value between 1<sup>st</sup> and 99<sup>th</sup> percentile to capture her self-perceived  
530 performance rank for that guess, relative to all other participants in the study; (c) actual  
531 placement (also in percentile); and (d) correct answer (the target’s actual weight). In actuality,  
532 however, the responses of the partner were experimentally created and pre-determined. In the

533 two partner conditions, the partner always guessed weights for the same two target photos in the  
534 sample trials, and always gave weight estimates of 139 lbs and 195 lbs, which placed her actual  
535 performance rank in the 24<sup>th</sup> and 26<sup>th</sup> percentile, respectively. Critically, despite the partner's  
536 substantially below-average performance, her estimated placement differed across conditions.  
537 The correct answer (i.e., the target's actual weight) on these two respective trials is 134 lbs and  
538 118 lbs.

539         In the *overplacement partner* condition ( $n = 129$ ), the partner's placement far exceeded  
540 her actual rank. Despite her poor rank, she placed herself at the 91<sup>st</sup> and 89<sup>th</sup> percentile for her  
541 two guesses. In the *calibrated partner* condition ( $n = 137$ ), the partner placed herself at the 26<sup>th</sup>  
542 and 28<sup>th</sup> percentile. In other words, her estimated placement was relatively low but well  
543 calibrated to her actual rank. Finally, in the *control condition* ( $n = 159$ ) there was no partner, and  
544 therefore no opportunity for social transmission. Actors were simply instructed to view "two  
545 quick examples [of the task] before getting started," and observed the same two photos as above  
546 and all associated information (excluding any partner self-placement information). The values of  
547 these parameters were identical to the partner conditions. Though this control condition was not  
548 of primary interest, it was included to establish baseline overplacement in the task in the absence  
549 of a partner.

550         Actors then proceeded to complete two trials of the task with new photos. They viewed a  
551 full-body photograph of the target individual, provided a weight estimate, and indicated their  
552 self-estimated placement using the same percentile rank scale ostensibly used by the partner (for  
553 descriptive information, see Supplemental Materials). Upon completing the task, actors  
554 responded to open-ended questions that probed for suspicion about the study—none in the  
555 partner conditions reported suspicion about the authenticity of the partner or partner responses.



556 **Dependent measure: Overplacement.**

557 Overplacement was again operationalized as the degree to which estimated placement  
558 exceeds actual placement. We first computed the absolute difference between participants'  
559 estimate and the correct answer (the true weight of the target). We then transformed these  
560 difference scores into proximity percentile rankings (with ties allowed). To account for any  
561 possible differences in actual performance between conditions (though they were not  
562 anticipated), participants' actual relative performance in each trial was determined in relation to  
563 others in the same condition. As described above, difference scores were used here given our  
564 interest in mean differences in overplacement across experimental conditions. Overplacement in  
565 each trial was computed by subtracting actual placement from self-estimated placement (Rogosa  
566 & Willett, 1983), and the scores on the two trials were then averaged together to form a  
567 composite measure of actor overplacement.

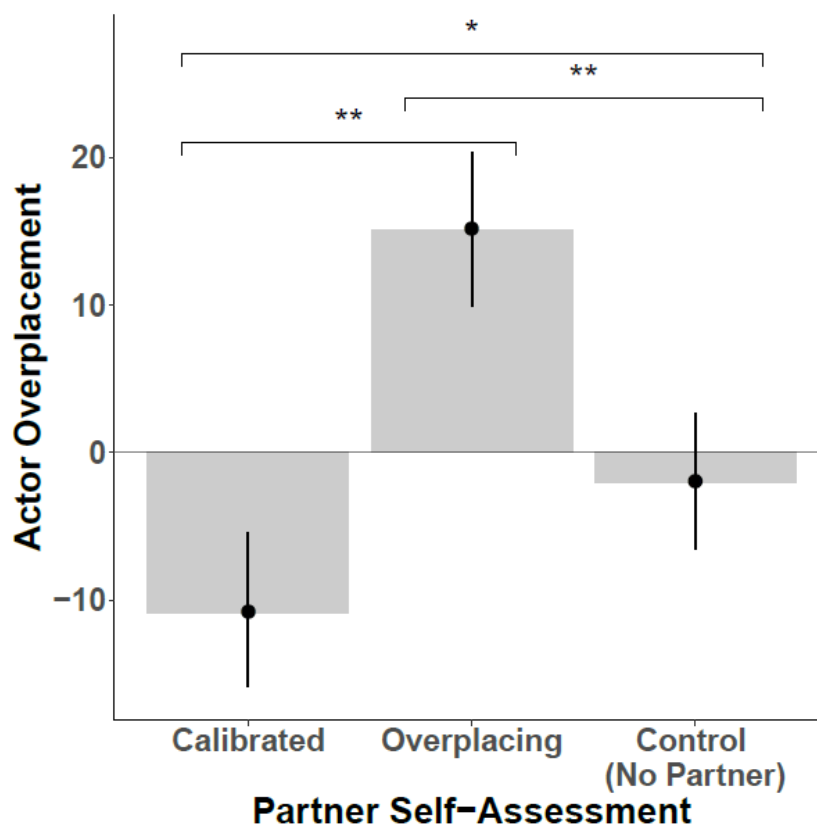
568 **Results and Discussion**

569 The overconfidence transmission hypothesis predicts greater overplacement in actors  
570 who observe the behavior of an overplacing partner, compared to those who observe a calibrated  
571 partner or no partner at all (our control). To compare the effect of different partners, we  
572 regressed actor overplacement on our 3 partner conditions (using 2 dummy variables). Moreover,  
573 to assess the robustness of results, we ran additional specifications that included controls: actor  
574 gender, age, and ethnicity.

575 The raw mean overplacement levels for each partner condition appear in Figure 2. We  
576 found that overplacing partners significantly increased actors' overplacement compared to  
577 calibrated partners or no partner (Table 1). Actor overplacement was 25.95 percentile points  
578 higher on average if the partner overplaced ( $M = 15.12$ ;  $SD = 2.67$ ) than if the partner was

579 calibrated  $\{M = -10.84; SD = 2.59; t(422) = 6.87, p < .001, d = .85, CI \text{ of mean difference} =$   
 580  $[18.64, 33.27]\}$ , and 17.12 percentile points higher than if there was no partner  $\{M = -2.00; SD =$   
 581  $2.41; t(422) = 4.76, p < .001, d = .58, CI \text{ of mean difference} = [10.05, 24.17]\}$ . These effects are  
 582 consistent and large across all additional specifications that include controls. Note the control  
 583 condition revealed that, without any potential for influence from partners, actors' self-estimated  
 584 placement on this task was well calibrated. Descriptively, their weak negative score was not  
 585 distinguishable from zero, the point of perfect calibration  $[t(158) = -.85, p = .399, d = -.07]$ ;  
 586 however, actors who were exposed to an overplacing partner exhibited self-estimated placement  
 587 that was strongly positively biased. Thus, as predicted, observing overplacement led to greater  
 588 overplacement.

589 **Figure 2. Actor overplacement by partner condition (Study 2).**  
 590



593 *Figure 2.* Raw overplacement in percentiles (and 95% confidence intervals) expressed by  
 594 participants directly exposed to different partner self-assessment conditions (calibrated,  
 595 overplacing, or no partner control). Positive percentile values index overplacement, 0 indexes  
 596 perfect calibration, and negative values index underplacement. In terms of absolute levels,  
 597 participants paired with an overplacing partner expressed overplacement, whereas those paired  
 598 with a calibrated partner displayed *underplacement*. Participants in the control condition (who  
 599 were not exposed to a partner) were well calibrated. This pattern of results is consistent with a  
 600 transmission process.

601

602 **Table 1. OLS regression of actor overplacement on partner self-assessment condition**  
 603 **(Study 2). Subsequent models control for actor gender, ethnicity, and age (centered).**  
 604 **Values are unstandardized regression coefficients followed by 95% confidence interval and**  
 605 ***p*-value in parentheses. The key results highlighted in gray indicate that overplacing**  
 606 **partners led to greater actor overplacement.**

607

	Baseline Model	Model with Covariates	Model with Covariates	Model with Covariates
Partner Self-Assessment Condition: Overplacement (0 = Calibrated; 1 = Overplacement)	25.95*** [18.64,33.27] (0.0000)	25.05*** [17.75,32.35] (0.0000)	24.85*** [17.52,32.18] (0.0000)	24.63*** [17.31,31.95] (0.0000)
Partner Self-Assessment Condition: No Partner Control (0 = Calibrated; 1 = No Partner)	8.84* [1.89,15.78] (0.0128)	7.76* [0.81,14.71] (0.0286)	7.78* [0.82,14.73] (0.0285)	7.31* [0.35,14.28] (0.0397)
Gender (1 = Male)		8.87** [2.80,14.94] (0.0043)	8.85** [2.77,14.92] (0.0044)	8.41** [2.32,14.50] (0.0069)
Ethnicity (0 = Caucasian; 1 = Non-Caucasian)			2.07 [-4.23,8.37] (0.5193)	2.58 [-3.74,8.90] (0.4229)
Age (centered)				0.66 [-0.15,1.48] (0.1090)
Constant	-10.84*** [-15.93,-5.74] (0.0000)	-13.31*** [-18.66,-7.96] (0.0000)	-14.68*** [-21.49,-7.88] (0.0000)	-14.64*** [-21.43,-7.85] (0.0000)
<b>R<sup>2</sup></b>	0.106	0.124	0.125	0.130
<b>Adjusted R<sup>2</sup></b>	0.102	0.117	0.116	0.119
<b>AIC</b>	4109.3244	4084.9415	4086.5207	4085.9118
<b>BIC</b>	4121.4806	4101.1310	4106.7575	4110.1960
<b>Observations</b>	425	423	423	423

608

+ p &lt; 0.10, \* p &lt; 0.05, \*\* p &lt; .01, \*\*\* p &lt; .001

609

610           **Summary.** These results support the notion that overplacement spreads from person to  
611 person. Actors were socially influenced by the high placement they observed expressed in an  
612 overplacing partner, and in doing so became more likely to overplace when assessing their own  
613 abilities. This process also lowered estimated placement among those paired with a calibrated  
614 partner who (appropriately) placed themselves more poorly, resulting in underplacement.  
615 Importantly, through the inclusion of a control condition with no partner, we are able to establish  
616 that this social transmission process can both increase and decrease overplacement.

617           These findings, combined with those from Study 1, suggest that overplacement spreads  
618 not only between individuals assigned to work together in person, but also from a brief  
619 observation of another person's biased beliefs. Thus, even ephemeral encounters with  
620 overconfident individuals could potentially have an effect on the likelihood and extent of  
621 adopting the overconfidence bias. Also striking is that actors in the overplacing partner condition  
622 knew their partners were *overplacing* (they falsely believed that they were among the most  
623 skilled), based on the information we supplied. Yet these actors were still influenced by their  
624 overplacing partners. Our findings thus highlight the ease with which overplacement may spread.

### 625                           **Study 3: Overplacement Spreads to Indirect Ties**

626           Our studies thus far have focused on the transmission of overplacement between directly  
627 connected individuals. However, many relationships between group members, especially within  
628 larger groups, are indirect (Christakis & Fowler, 2009; Granovetter, 1977). For example, suppose  
629 that Agnes and Paul work in the same organization but have never worked nor interacted with  
630 each other directly. Both of them, however, work closely with Peter. Is it possible that Agnes'  
631 overconfidence may influence Peter, who in turn influences Paul, even though Paul has never  
632 met Agnes? Such effects, which have been the focus of an extensive empirical literature on

633 social influence (Bond et al., 2012; Christakis & Fowler, 2008; Fowler & Christakis, 2010;  
634 Gruenfeld et al., 2000), would suggest that social transmission may play an important role in the  
635 emergence of group and cultural differences in overconfidence on a broader scale (Mesoudi &  
636 Whiten, 2008; Whiten & Mesoudi, 2008).

637         To test the transmission of overplacement between indirectly connected individuals, we  
638 presented the responses of the participants in Study 2 to a new set of participants in Study 3. This  
639 design, which is similar to an abridged version of the linear transmission chain method employed  
640 in studies of cultural transmission (Bartlett, 1932; Mesoudi, 2007), allows us to examine whether  
641 the overplacement of participants in the present study (“actors” hereafter; C in the chain) was  
642 influenced: (a) directly by their immediate partner who was a real participant from Study 2  
643 (“partner” hereafter; B in the chain); and (b) indirectly by the fictitious partner whom their  
644 partner had observed in Study 2 (“partner’s partner” hereafter; A in the chain), but they  
645 themselves did not directly observe. Consequently, in contrast to Study 2 in which partner  
646 responses were experimentally manipulated and fictitious, in this study actors observed genuine  
647 responses supplied by participants from Study 2. No deception was used.

## 648 **Method**

### 649 **Participants.**

650         Through a campus-wide solicitation at a large public university in Canada, we recruited  
651 255 participants (59.29% women; 3 participants did not disclose gender) for an in-person  
652 computerized study on judgment and decision-making. As in Study 2, we initially targeted 130  
653 participants in each of two conditions (targeted  $N = 260$  combined), as guided by a power  
654 analysis in which we assumed a typical effect size of  $d = .35$ , using an alpha level of .05 and  
655 power of .80. Data collection was terminated immediately after this target sample size was

656 reached. However, data from 5 participants were not recorded due to experimenter error, leaving  
657 a final sample of 255 participants. Participants' ages ranged from 17 to 50 ( $M = 21.37$ ,  $SD =$   
658 4.56). As in Study 2, participants received a candy bar for participating and were entered into a  
659 raffle to win \$30 based on performance and calibration. Data from the 255 individuals who  
660 completed the study were included in our analyses below.

### 661 **Materials and procedure.**

662 The study design was identical to Study 2 with two exceptions. First, actors viewed the  
663 target photos and the responses that a real participant (their partner) supplied in Study 2. Partners  
664 were randomly selected with replacement—meaning that a given partner could be selected more  
665 than once, to simulate simple random sampling. Only partners assigned to the overplacing  
666 partner condition or the calibrated partner condition in Study 3 were selected; those in the control  
667 condition were not drawn. Together, our 255 actors in this study were paired with 163 unique  
668 partners. Second, new target photographs (that differed from those used in Study 2) were used  
669 for the two task trials.

### 670 **Key variables.**

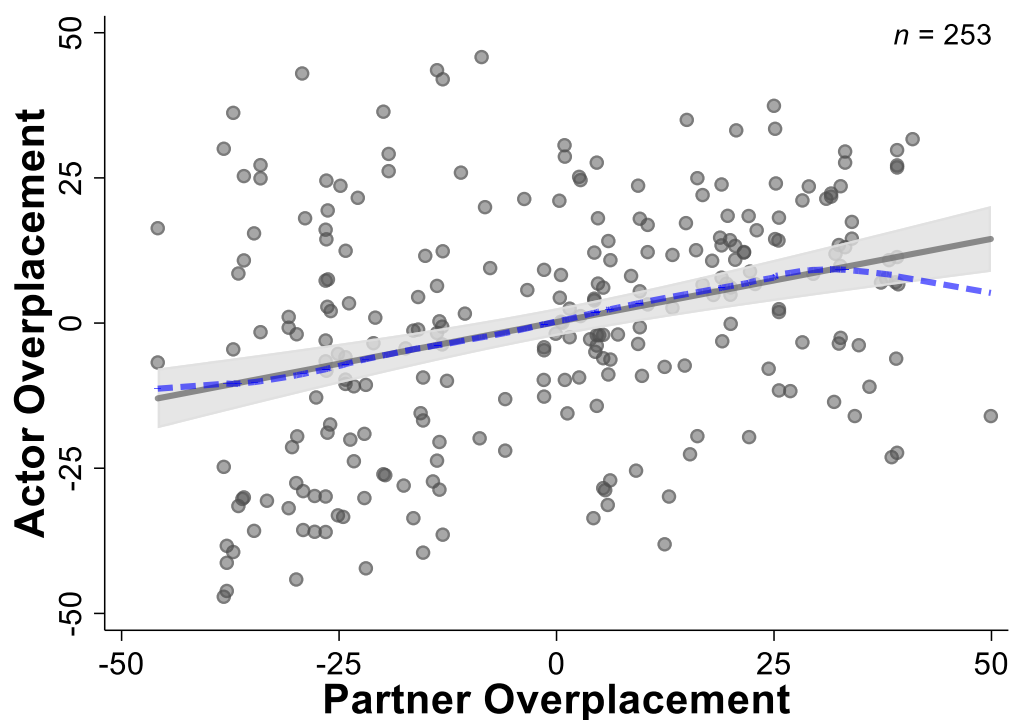
671 This set-up yields three key variables of interest: actor overplacement (a continuous  
672 variable), partner overplacement (a continuous variable), and partner of partner overconfidence  
673 (a dichotomous variable that refers to the experimental condition to which the partner was  
674 assigned in Study 2: overplacing vs. calibrated partner). Overplacement for all parties was  
675 calculated using the same scoring procedure as described in Study 2.

### 676 **Results and Discussion**

677 We present three key sets of analyses that address specific predictions derived from the  
678 overconfidence transmission hypothesis.

679 **Does overplacement transmit directly, from partner to actor?** To test our prediction  
 680 of direct, person-to-person transmission, we examined the association between partner  
 681 overplacement and actor overplacement. Consistent with predictions, partner overplacement was  
 682 significantly and positively associated with actor's overplacement ( $r = .33, p < .0001$ ; see Figure  
 683 3). This indicates that actors' estimated placement, once again, was swayed by the estimated  
 684 placement expressed by their partner. By comparison, one's own objective placement played no  
 685 detectible role in influencing levels of estimated placement (i.e., participants did not have insight  
 686 into their actual relative performance).

687 **Figure 3. Actor overplacement plotted against partner overplacement (Study 3).**  
 688



689 *Figure 3.* Raw scatter plot showing a positive relation between partner overplacement and actor  
 690 overplacement in Study 3. Both variables were computed using the residual score approach and  
 691 reflect variability in self-estimated placement that cannot be linearly predicted from actual  
 692 placement. Also shown are the line of best fit (in solid line), 95% confidence interval (in shaded  
 693 gray region), and lowess curve (in dotted blue line).  
 694  
 695

696           **Does overplacement transmit indirectly, from partner's partner to actor?** To test for  
697 patterns of *indirect*, person-to-person-to-person transmission, we compared the mean level of  
698 overplacement expressed by actors who were indirectly connected to either a partner's partner  
699 who overplaced or a partner's partner who was calibrated using the same regression models in  
700 Study 2.

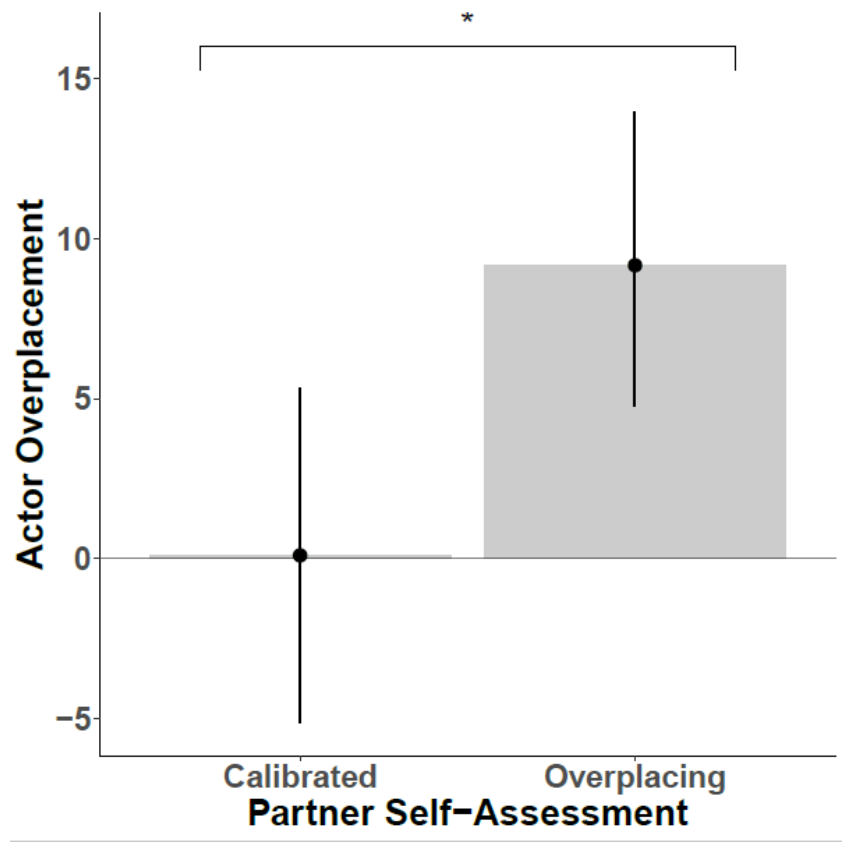
701           The raw mean overplacement levels are shown in Figure 4. Actors expressed  
702 significantly greater overplacement when indirectly yoked to a partner's partner who overplaced  
703 than when yoked to a partner's partner who was calibrated. Actor overplacement was 8.83  
704 percentile points higher if the partner's partner overplaced ( $M = 8.92$ ;  $SD = 2.56$ ) than if the  
705 partner's partner was calibrated ( $M = .09$ ;  $SD = 2.40$ ;  $t(251) = 2.52$ ,  $p = .013$ ,  $d = .32$ , CI of mean  
706 difference = [1.92, 15.75]) (Table 2). This mean difference was stable across the alternative  
707 specifications that adjusted for covariates: actor gender, age, and ethnicity. Additional analyses  
708 (reported in the Supplemental Materials) confirm that the indirect spread of overplacement  
709 occurred via a chain of direct pairwise effects; consistent with the notion of person-to-person  
710 spread of overplacement, partner overplacement fully mediated the effect of a partner's partner  
711 on actors. Although our actors never directly interacted with their partner's partner, they were  
712 nevertheless influenced by the effect that the partner's partner had upon their partner, who  
713 subsequently influenced their own overplacement. Being connected to a partner who witnessed  
714 another person express overplacement was sufficient to increase one's own overplacement,  
715 indicating that overplacement can spread to indirect social ties.

716



717

**Figure 4. Actor overplacement by partner condition (Study 3).**



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*Figure 4.* Raw overplacement in percentiles (and 95% confidence intervals) expressed by participants *indirectly* exposed to different partner self-assessment conditions (calibrated or overplacing). Positive percentile values index overplacement, 0 indexes perfect calibration, and negative values index underplacement. Participants indirectly tied to an overplacing partner expressed overplacement, whereas those indirectly tied to a calibrated partner were well calibrated.

728 **Table 2. OLS regression of actor overplacement on the partner of partner's self-assessment**  
 729 **condition (indirect tie; Study 3). Subsequent models control for actor gender, ethnicity, and**  
 730 **age (centered). Values are unstandardized regression coefficients followed by 95%**  
 731 **confidence interval and *p*-value in parentheses. The key results highlighted in gray indicate**  
 732 **that indirect tie to an overplacing partner of partner led to more inflated actor**  
 733 **overplacement.**  
 734

	Baseline Model	Model with Covariates	Model with Covariates	Model with Covariates
Partner of Partner's Self-Assessment Condition (0 = Calibrated; 1 = Overplacing)	8.83*	8.76*	8.34*	7.94*
	[1.92,15.75] (0.0125)	[1.84,15.68] (0.0133)	[1.43,15.25] (0.0182)	[0.94,14.94] (0.0264)
Gender (1 = Male)		3.24 [-3.82,10.30] (0.3672)	2.74 [-4.31,9.80] (0.4442)	3.22 [-3.86,10.31] (0.3715)
Ethnicity (0 = Caucasian; 1 = Non-Caucasian)			7.08* [-0.87,15.03] (0.0805)	5.92 [-2.33,14.17] (0.1587)
Age (centered)				-0.45 [-1.25,0.34] (0.2628)
Constant	0.09 [-4.63,4.81] (0.9705)	-1.16 [-6.61,4.29] (0.6759)	-6.06 [-13.78,1.67] (0.1238)	-5.10 [-13.08,2.89] (0.2097)
<i>R</i> <sup>2</sup>	0.025	0.028	0.040	0.046
Adjusted <i>R</i> <sup>2</sup>	0.021	0.020	0.028	0.031
<i>AIC</i>	2403.5398	2404.7154	2403.6054	2395.0552
<i>BIC</i>	2410.6066	2415.3155	2417.7390	2412.7023
Observations	253	253	253	252

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

736 **Summary.** These results converge with those from Study 2 to demonstrate the spread of  
 737 overplacement. As in Study 2, merely witnessing overplacement in another person was sufficient  
 738 to promote overly inflated self-placements, suggesting that individuals can “catch” this cognitive  
 739 bias after they observe it in others. Moreover, beyond spreading directly from person to person,  
 740 overplacement can transmit indirectly across ties to others who are not part of the original  
 741 interaction, cascading from person to person to person in sequence. This provides suggestive  
 742 evidence that, by diffusing in a chain-like fashion, overconfidence may spread widely and  
 743 extensively in social groups and networks.  
 744

745         **Study 4: The Transmission of Overplacement from Overplacing and (Justifiably)**  
746   **Confident Peers**

747             Studies 1 through 3 suggest that overplacement can transmit between individuals.  
748     However, it is unclear what tendencies individuals acquire precisely. One possibility is that  
749     individuals align with others' overplacement, in that they observe others expressing an overly  
750     positive self-assessment and adopt an overplacing mindset (e.g., Jane observes Harold  
751     overplacing his performance by 20 percentile points and aligns with his overplacement). Another  
752     possibility is that individuals acquire others' confidence and not their *biased* beliefs per se (e.g.,  
753     Jane hears Harold say he performed in the 90<sup>th</sup> percentile and similarly places her own  
754     performance highly, regardless of how well Harold actually performed). The primary aim of  
755     Study 4 is to provide a more direct test of whether individuals acquire confidence more  
756     generally, or whether they strictly acquire overplacement.

757             We used a modified version of the weight-guessing task deployed in Studies 2 and 3, and  
758     included two additional conditions: a partner with high confidence (high self-placement) and  
759     high skill (high actual placement; who was therefore well calibrated and confident), and a partner  
760     with low confidence and high skill (who was therefore underplacing). These conditions were  
761     combined with the two other conditions used in Studies 2 and 3 to yield four partner conditions:  
762     overplacing, calibrated-and-unskilled, underplacing, and calibrated-and-skilled. Our prediction is  
763     that actors who observe both overplacing partners and confident (and skilled) partners will adopt  
764     their partner's high confidence, regardless of the partner's true performance. That is, actors will  
765     increase their confidence and not strictly just overplacement per se. However, when individuals  
766     adopt high levels of confidence (without a corresponding increase in their actual placement),  
767     they subsequently become overplacing. Conversely, we expected actors who observed both

768 calibrated—and-unskilled partners and underplacing partners to align their self-estimated  
769 placement with their partner’s low confidence.

770 A second and more exploratory aim of Study 4 was to examine the persistence of  
771 transmission effects over several trials. The task design included a baseline *practice phase* in  
772 which participants were not yet exposed to a partner’s information, a *test phase* in which  
773 participants were exposed to a partner’s information, and a *post-partner* phase in which  
774 participants were no longer exposed to a partner’s information.

## 775 **Method**

### 776 **Participants.**

777 We recruited 248 participants (39% women) from Amazon Mechanical Turk online labor  
778 market (Buhrmester et al., 2011; Paolacci et al., 2010). The effect sizes of the direct influence of  
779 partners in Studies 2 and 3 were  $d_s = .85$  and  $.58$  (Study 2) and  $r = .33$  (equal to  $d = .58$ ; Study  
780 3), respectively. A power analysis based on  $d = .58$ —the weaker, and thus more conservative, of  
781 these effect sizes obtained—suggests the need to sample 48 participants in each condition for a  
782 power of .80 (given an alpha level of .05). We thus sought to recruit 60 participants in each of 4  
783 conditions (targeted  $N = 240$  combined). Participants’ ages ranged from 18 to 64 ( $M = 29.18$ ,  $SD$   
784  $= 10.29$ ). All participants received \$3.00 and an entry into two \$50 raffles (conducted after the  
785 completion of data collection) that gave everyone an equal chance of winning irrespective of  
786 their responses. Analyses below include data from all participants.

### 787 **Materials and procedure.**

788 Participants (hereafter termed “actors”) read initial instructions about the weight-guessing  
789 task, which consisted of 15 trials. Actors began by completing five practice trials (Trials 1-5),  
790 which were designed to both familiarize them with the task and index their baseline

791 overplacement before our experimental manipulation of the “partner’s” information. In each of  
792 these practice trials, actors viewed a full-body photograph of a target individual, provided a  
793 weight estimate, and indicated their self-estimated placement (percentile rank), using the same  
794 prompts as in Studies 2 and 3 (see Supplemental Materials for other minor methodological  
795 divergence from Studies 1-3).

796         After completing the baseline practice phase, actors were assigned to one of four  
797 experimental conditions in a 2 (partner confidence: high vs. low)  $\times$  2 (partner performance: high  
798 vs. low) between-subjects design. Actors in the *overplacing partner* condition (high confidence,  
799 low performance;  $n = 60$ ) learned that, on average across all five photos to which the partner  
800 responded, she placed herself in the 90<sup>th</sup> percentile, despite actually scoring on average only in  
801 the 24<sup>th</sup> percentile. Actors in the *calibrated-and-unskilled partner* condition (low confidence, low  
802 performance;  $n = 64$ ) witnessed a partner who, on average, placed herself in the 27<sup>th</sup> percentile  
803 and performed at the 24<sup>th</sup> percentile. These two conditions parallel the partner conditions used in  
804 Studies 2 and 3. Actors in the *confident partner* condition (high confidence, high performance;  $n$   
805 = 69) witnessed a calibrated-and-skilled partner who, on average, placed herself in roughly the  
806 90<sup>th</sup> percentile and performed at the 91<sup>st</sup> percentile. Finally, actors in the *underplacing partner*  
807 condition (low confidence, high performance;  $n = 55$ ) witnessed a partner who, on average,  
808 placed herself in approximately the 27<sup>th</sup> percentile despite scoring in the 91<sup>st</sup> percentile.

809         Note that this partner information was presented only in the first five test trials (Trials 6-  
810 10). In these test trials where participants were exposed to partner information, actors first  
811 responded to the photo shown—by providing a weight estimate and self-estimated placement—  
812 and then immediately viewed the responses that their “partner” had purportedly given for the  
813 same photo. In actuality, however, as in Study 2, all partner responses were experimentally

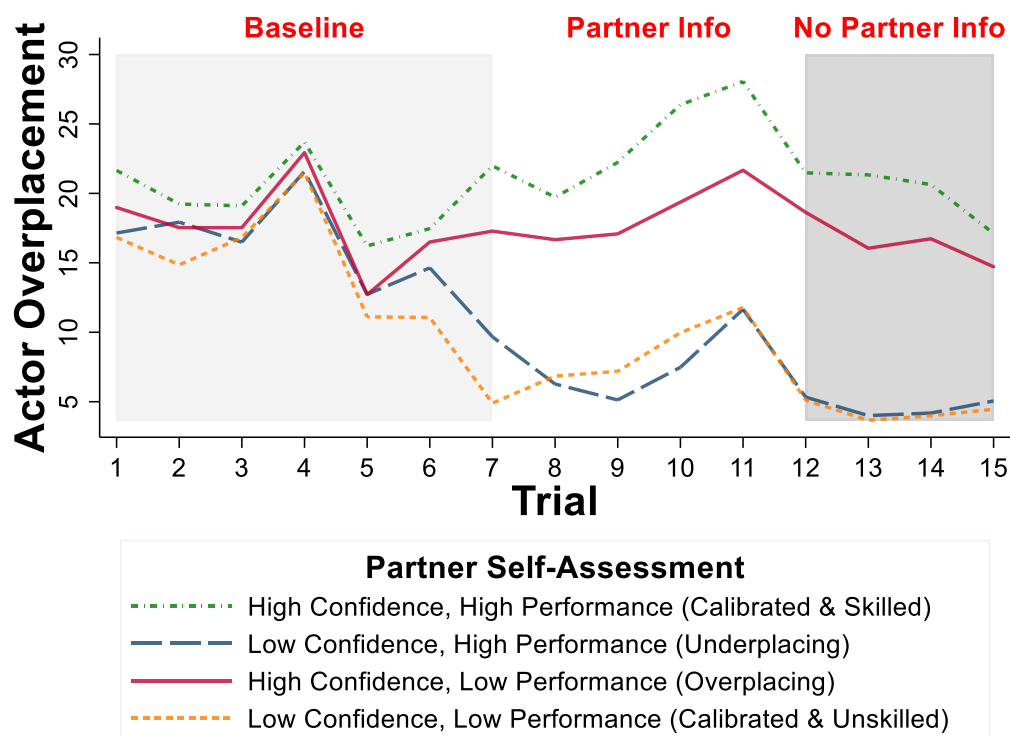
814 created and pre-determined to vary across the four experimental conditions. Because actors  
815 always provided their weight and self-placement estimate for each photo before (rather than  
816 after) receiving the partner's input for the same photo, this means that the partner's self-  
817 placement could only affect actor overplacement on new trials that the partner had not yet  
818 completed.

819         In the last five test trials (Trials 11-15), no partner information was provided. Actors  
820 simply responded to five photos without viewing any partner responses. This enabled  
821 comparisons of participants' beliefs in these trials (that lack partner information) against those in  
822 the immediately preceding trials (that co-occur with partner information). Such comparisons  
823 allow us to tentatively explore whether the transmission effect "wears off" when reminders of a  
824 partner's (overplacing) responses have ceased, or if it persists beyond initial contact to influence  
825 observers even in subsequent trials wherein the overplacing model was no longer presented.

826         After completing all 15 task trials, actors self-reported their perceptions of the partner's  
827 confidence and task ability. These ratings confirm the effectiveness of our experimental  
828 manipulations. Specifically, the perceived confidence of the partner is higher among participants  
829 assigned to the high partner confidence conditions (compared to the low partner confidence  
830 conditions), and the perceived task competence of the partner is higher among participants  
831 assigned to the high partner performance conditions (compared to the low partner performance  
832 conditions (see Supplemental Materials for further details on manipulation check results)).  
833 Finally, participants reported the perceived influence of the partner over their own decisions (for  
834 results exploring subjective awareness of partner influence, also see Supplemental Materials),  
835 and completed a series of demographic questions.

836 **Analytic Plan**

837 Overplacement in each trial was calculated using the same scoring procedure as described  
 838 in Studies 2 and 3, using difference scores. As shown in Figure 5, the raw mean trial-by-trial  
 839 results show that actors' overplacement levels diverged across partner conditions. As expected,  
 840 in the baseline trials, similar levels of overplacement are seen across conditions, before actors  
 841 observed any partner responses, confirming the success of our random assignment procedure  
 842 (see Supplemental Materials). Upon the onset of partner responses (after Trial 6), however, actor  
 843 overplacement immediately began to diverge across conditions. These differences in  
 844 overplacement persisted even in trials for which information about the partner was no longer  
 845 presented (beginning in Trial 12).

846 **Figure 5. Actor Overplacement across Trials by Partner Condition (Study 4).**

848  
 849 *Figure 5.* Raw trial-by-trial mean overplacement shown by participants exposed to different  
 850 partner self-assessment conditions. In the baseline phase (Trials 1-6), before exposure to partner,  
 851 actors' overplacement (in percentiles) did not differ across conditions. Immediately after viewing

852 the partner's responses, actor overplacement in the test phase (Trials 7-15) systematically  
 853 diverged across conditions, consistent with the transmission hypothesis. This pattern persisted  
 854 into the post-partner-information phase (Trials 12 to 15), wherein partner responses were no  
 855 longer presented. Note that such between-condition comparisons are more meaningful than  
 856 examining within-condition trajectories, given that differences in overplacement between trials  
 857 in part reflect trial difficulty.  
 858

859 To statistically analyze the differences visible in Figure 5, we created three aggregate  
 860 measures to capture mean overplacement expressed by actors in each of the following phases of  
 861 the experiment: baseline phase (before exposure to partner), test phase (during *and* after  
 862 exposure to partner), and post-partner phase (after exposure to partner). These measures were  
 863 computed by averaging actors' overplacement scores across Trials 1 to 6 for the baseline phase,  
 864 Trials 7 to 15 for the test phase, and Trials 12 to 15 for the post-partner-information phase.<sup>7</sup>  
 865 Creating aggregate measures reduced noise resulting from trial to trial differences in difficulty—  
 866 wherein some targets' weight might appear easier to guess than others and thus generate greater  
 867 overplacement (Larrick et al., 2007; Moore & Small, 2007)—and are thus more reliable than  
 868 single trial scores.<sup>8</sup>

## 869 **Results and Discussion**

870 Did exposure to confidence (high self-placements), regardless of whether it accurately  
 871 reflected underlying skill and ability, increase overplacement? To address this key question, we

---

<sup>7</sup> It might be useful to briefly note how the 15 trials were divided into 3 phases. Trial 6 was the last trial to which actors responded *before* exposure to the partner, Trial 7 was the first trial to which actors responded *after* exposure to the partner, and Trial 12 was the first trial to which actors responded *after* exposure to the partner had ceased.

<sup>8</sup> Our analyses below focus on comparing *between*-actor overplacement within the test phase across conditions, rather than the *within*-actor trajectory of overplacement across trials. Such within-person analyses yield ambiguous results because existing work indicates that the absolute level of overplacement exhibited on a given task is in part driven by perceived task difficulty (Ehrlinger et al., 2016; Moore & Small, 2007). Thus, within-actor trajectories (and the absolute level of actor overplacement in a given trial), though interesting, are expected to naturally vary with task domain and perhaps even minor modifications to the task trials (e.g., swapping in new target photos that appear more difficult would yield lower overplacement than observed here); hence they fall short of documenting meaningful change over successive trials and offer limited substantive meaning (see Supplemental Materials).



872 compared actor overplacement across conditions in the entire test phase, regressing actor  
 873 overplacement on the main effects and interaction of partner confidence (self-placement) and  
 874 performance (actual placement), and in subsequent specifications control for potential covariates.  
 875 These regression results are presented in Table 3. The coefficient on partner confidence is large  
 876 and significant at conventional levels across all models, independent of the controls, as predicted.  
 877 By contrast, there is no detectible main effect of partner performance or partner confidence  $\times$   
 878 performance interaction. This suggests that actors aligned with their partner's confidence  
 879 regardless of whether the confidence was warranted or not.

880 **Table 3. OLS regression of actor overplacement in 3 phases of the experiment—(a) baseline**  
 881 **phase (trials before exposure to partner), (b) test phase (trials during and after exposure to**  
 882 **partner), and (c) post-partner phase (only trials after exposure to partner)—on partner**  
 883 **confidence condition and partner performance condition (Study 4). Some subsequent**  
 884 **models control for gender, ethnicity, and age (centered). Printed are coefficients followed**  
 885 **by 95% confidence interval and *p*-value in parentheses. The key results highlighted in gray**  
 886 **indicate that, following exposure to partner, partner confidence significantly predicts actor**  
 887 **overplacement. Note this effect is not conditional on partner performance (no partner**  
 888 **confidence  $\times$  partner performance interaction).**  
 889

	DV = Baseline Phase: Trials Pre-Exposure to Partner		DV = Test Phase: Trials During and After Partner Feedback			DV = Post-Partner Phase: Trials Post-Partner Feedback
	Baseline Model	Baseline Model	Model with Covariates	Model with Covariates	Model with Covariates	Baseline Model
Partner Confidence Condition (0 = Low Self-Placement; 1 = High Self-Placement)	2.34	11.15***	12.54***	12.55***	12.53***	12.23**
	[-4.10,8.78] (0.4750)	[4.74,17.55] (0.0007)	[6.09,19.00] (0.0002)	[6.08,19.02] (0.0002)	[6.02,19.05] (0.0002)	[4.14,20.32] (0.0032)
Partner Performance Condition (0 = Low Actual Placement; 1 = High Actual Placement)	1.39	0.10	0.94	0.91	0.87	0.34
	[-5.20,7.98] (0.6781)	[-6.46,6.65] (0.9764)	[-5.60,7.47] (0.7778)	[-5.65,7.47] (0.7847)	[-5.83,7.56] (0.7987)	[-7.94,8.62] (0.9356)
Partner Confidence Condition $\times$ Partner Performance Condition	0.47	4.43	3.14	3.15	3.21	3.27
	[-8.66,9.60] (0.9192)	[-4.66,13.52] (0.3381)	[-5.92,12.21] (0.4953)	[-5.94,12.24] (0.4953)	[-6.06,12.48] (0.4958)	[-8.21,14.75] (0.5754)
Gender (1 = Male)			5.59* [0.91,10.27]	5.59* [0.90,10.28]	5.55* [0.67,10.42]	

			(0.0194)	(0.0196)	(0.0260)	
Ethnicity (0 = Caucasian; 1 = Non-Caucasian)				0.31	0.27	
				[-4.48,5.09]	[-4.62,5.16]	
				(0.9001)	(0.9128)	
Age (centered)					-0.01	
					[-0.24,0.23]	
					(0.9462)	
Constant	15.36***	6.43**	2.24	2.14	2.20	4.30
	[10.88,19.84]	[1.97,10.89]	[-3.40,7.88]	[-3.72,8.00]	[-3.89,8.28]	[-1.33,9.93]
	(0.0000)	(0.0049)	(0.4351)	(0.4721)	(0.4779)	(0.1336)
<b>R<sup>2</sup></b>	0.008	0.130	0.150	0.150	0.150	0.090
<b>Adjusted R<sup>2</sup></b>	-0.005	0.120	0.136	0.132	0.129	0.078
<b>AIC</b>	2146.4986	2144.1783	2140.5920	2142.5758	2144.5711	2259.9460
<b>BIC</b>	2160.5523	2158.2320	2158.1592	2163.6564	2169.1651	2273.9997
<b>Observations</b>	248	248	248	248	248	248

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

890  
891

892 Further, as revealed in Figure 6 and estimated in the baseline model, actors'

893 overplacement was strongest and roughly 13.36 percentile points higher if they were exposed to

894 a partner with high self-placement ( $M = 20.00$ ;  $SD = 16.73$ ), than when exposed to a partner with

895 low self-placement ( $M = 6.48$ ;  $SD = 19.48$ ;  $t(244) = 5.79$ ,  $p < .001$ ,  $d = .75$ , CI of mean

896 difference = [8.82, 17.91]). Moreover, overplacement was just as high if the partner overplaced

897 (that is, had low actual placement;  $M = 17.58$ ;  $SD = 16.76$ ) as if the partner was justifiably high

898 self-placing (that is, had high actual placement;  $M = 22.10$ ;  $SD = 16.53$ ); these two conditions

899 did not differ significantly ( $t(244) = 1.42$ ,  $p = .158$ ,  $d = .27$ , CI of mean difference = [-1.77,

900 10.82]). Furthermore, providing a direct replication of our prior studies, when the partner's

901 performance was low, actors showed substantially greater overplacement if said partner's

902 confidence was high (that is, an overplacing partner) compared to if it was low (that is, a

903 calibrated but unskilled partner;  $M = 6.43$ ;  $SD = 17.37$ ;  $t(244) = 3.43$ ,  $p = .001$ ,  $d = .65$ , CI of

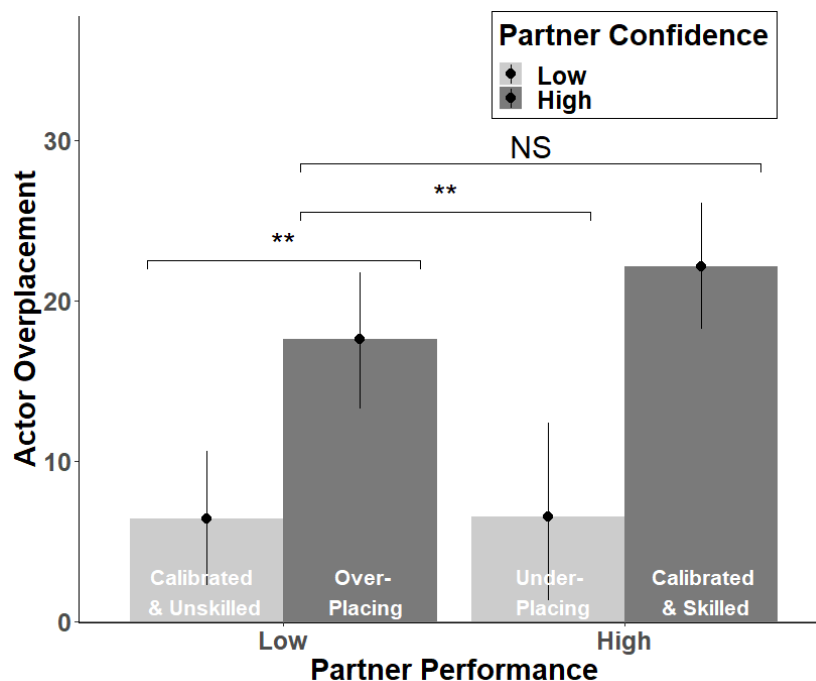
904 mean difference = [4.74, 17.55]). The same pattern of results is obtained for the post-partner

905 phase, suggesting that these effects persist when exposure to partner ceases (see Supplemental

906 Materials).

907

908 **Figure 6. Actor Overplacement in the Test Phase by Partner Self-Placement and Actual**  
 909 **Placement Condition (Study 4).**  
 910



911 *Figure 6.* Raw overplacement in percentiles (and 95% confidence intervals) expressed by  
 912 participants exposed to different partners who vary in self-placement (confidence) and actual  
 913 placement (performance) in the test phase (i.e., the mean across all trials following initial  
 914 exposure to partner, corresponding to the test phase in Table 3). Positive percentile values index  
 915 overplacement, zero indexes perfect calibration. Actors paired with highly self-placing partners  
 916 expressed significantly greater overplacement than actors paired with lowly self-placing partners,  
 917 regardless of whether the partner's confidence was warranted (i.e., a calibrated-and-skilled  
 918 partner) or not (i.e., an overplacing partner).  
 919

920  
 921 We note three other relevant sets of findings, all of which are detailed more thoroughly in  
 922 the Supplemental Materials. First, our manipulation check confirms that the current results  
 923 emerged despite participants' awareness that the overplacing partner's beliefs was unrealistic  
 924 (and thus overplacing). That is, actors were influenced by their partner's confidence despite  
 925 being fully aware that their partner's confidence was unwarranted, as evidenced by the lower  
 926 ratings of task competence assigned to these overplacing partners. Second, we found that the  
 927 transmission effect persisted even after the exposure to partner ceased, such that actors' self-

928 estimated placement in the overplacement condition remained skewed in the post-partner-  
929 information phase. These regression results (reported in Table 3 above), which are also visible in  
930 the trends illustrated in Figure 6, indicate that these effects only showed a slight diminution in  
931 the later trials when the partner's presence was removed. The social influence of overplacing  
932 others demonstrated persistence. Third, despite the clear effect that witnessing overplacement in  
933 others had on participants' own overplacement, participants subjectively perceived overplacing  
934 partners as the *least* influential over their own behavior, highlighting that they were explicitly  
935 unaware of (or at least unable to report) their partner's extensive social influence over them.

936 **Summary.** In sum, we again found that participants who observed an overplacing partner  
937 displayed higher overplacement. Moreover, observing a justifiably highly self-placing partner—  
938 whose confidence was, by contrast, warranted by superior performance—similarly produced  
939 high levels of overplacement. Thus, these results offer a crucial insight: confidence transmits,  
940 even if it is shown by overconfident social partners. Individuals align their confidence with the  
941 level observed in others, and by doing so increase the likelihood of being positively biased.  
942 Finally, we found that participants who “caught” high levels of confidence from their partner  
943 remained confident for several trials even after the partner's information was no longer visible,  
944 suggesting that the transmission effect persists even in the absence of the influencing partner.

#### 945 **Study 5: The Transmission of Overplacement across Time and Task Domains**

946 In Study 5, we further investigate the persistence and power of overconfidence  
947 transmission in two ways. First, we test longitudinally whether the effect of being exposed to  
948 confidence endures after several days. Second, we test whether the transmission effect also  
949 “carries over” to influence self-assessments in different task domains. If so, this study would  
950 provide important initial evidence that the effects of overconfidence transmission are not short-

951 lived and can continue to affect a person’s self-assessments over time, and that the effects are not  
952 limited to the domain in which overplacement is “caught”—but instead can bleed into other  
953 domains.

954 To these ends, we first administered the same weight-guessing task used in Studies 2-4  
955 and exposed participants to partners with different self-assessment levels. Several days later,  
956 participants completed an additional and unrelated word task. Key to this procedure is that  
957 participants were not reminded of their partner’s self-assessment in the first task. Therefore, any  
958 effect of partner’s initial overplacement on participants’ overplacement in the word task would  
959 not only suggest that overplacement transmission persists longitudinally, but that it even “spills  
960 over” to affect self-assessments in a different task domain.

961 An additional goal of Study 5 was to further examine the generalizability of  
962 overconfidence transmission. Specifically, would it extend even to task domains in which people  
963 tend to have more accurate self-assessments? In contrast to the weight-guessing task used in  
964 Studies 2-4, for which self-evaluated performance was uncorrelated with actual performance,  
965 people have a moderate degree of self-insight about their ability in the word task used here  
966 (Caputo & Dunning, 2005).

## 967 **Method**

### 968 **Participants.**

969 We recruited 405 participants from the Amazon Mechanical Turk online labor market  
970 (54.8% women, .2% other) whose ages ranged from 19 to 78 ( $M = 36.22$ ,  $SD = 1.46$ ).

971 Participants received \$0.30 for completing the initial survey (at Time 1) and were entered into a  
972 raffle to win a \$25 bonus payment based on both performance and calibration.

973 Participants received an additional \$0.50 for completing a (previously unannounced)  
974 follow-up survey (at Time 2), several days later, and were entered into an additional raffle to win  
975 a \$25 bonus payment based on similar criteria as at Time 1. Two-hundred participants (49.38%  
976 of all Time 1 participants; 57.5% women, .5% other) responded to the Time 2 survey.<sup>9</sup>

977 **Materials and procedure.**

978 **Design.** The design was 2 (partner self-assessment: overplacing vs. calibrated; between-  
979 subjects)  $\times$  2 (time: Time 1 vs. Time 2; within-subjects). To explore the relative strength of  
980 transmission of overplacement in the same vs. a novel task domain, at Time 2 participants first  
981 completed a word scramble task, followed by the same weight-guessing task they had completed  
982 at Time 1. This task order was chosen to prioritize our test of cross-domain transmission. For the  
983 weight-guessing task at Time 2, in order to examine whether transmission within the same task  
984 domain operates on novel stimuli (beyond merely repeated stimuli), we presented the same  
985 photographs as at Time 1 (same targets) and new photographs (new targets), and  
986 counterbalanced their order across subjects.

987 **Time 1.** At Time 1, the materials and procedure were similar to Study 2 with one  
988 exception. In addition to learning about a partner's answers in the weight guessing task, some  
989 participants first read an 'introductory description' of the partner's personality. These  
990 descriptions aimed to increase the perceived authenticity of, and memory for, the partner  
991 (Tiedens et al., 2007). These descriptions came from a pilot study in which a separate group of

---

<sup>9</sup> No differences were found between these participants who completed both surveys and those who completed only the Time 1 survey on our key demographic and dependent variables (gender, actual performance on all tasks, and overplacement), apart from the higher mean age of the former group ( $M_{\text{age}} = 38.50$ ,  $SD = .88$ ) compared to the latter group [ $M_{\text{age}} = 33.99$ ,  $SD = .79$ ;  $t(402) = -3.80$ ,  $p = .0002$ ]. One participant was excluded from the study for providing implausible weight estimates of persons in the photographs at Time 1 (i.e., below 10 lbs), leaving a final sample size of 404 participants. None of the conclusions reported below change as a result of excluding this participant.

992 participants described, in a few lines, a person they knew.<sup>10</sup> The remainder of the materials and  
993 procedure at Time 1 (as well as a control condition that did not view a partner description) were  
994 identical to Study 2. Actors were randomly assigned to either the *overplacing partner* condition  
995 (high confidence, low performance;  $n = 200$ ) or the *calibrated partner* condition (low  
996 confidence, low performance;  $n = 204$ ).

997 **Time 2.** Actors were invited, without prior notice, to participate in a follow-up survey.  
998 The invitation reminded them that they had completed a survey in which they guessed the weight  
999 of persons in photographs. However, the invitation did *not* remind them about the partner's self-  
1000 estimated or actual placement. Actors began the Time 2 survey between 53 and 124 hours after  
1001 they had begun the Time 1 survey ( $M = 71$  hours,  $SD = 13.3$ ). The rate of completing the Time 2  
1002 survey did not differ by condition (*overplacing partner* condition,  $n = 96$ ; *calibrated partner*  
1003 condition,  $n = 104$ ;  $\chi^2(1) = .36, p = .549$ ).

1004 In the Time 2 survey, actors began by completing the word task. They saw an example 3  
1005  $\times$  3 matrix word scramble and learned the rules of the task (which were similar to the popular  
1006 game Boggle), and then were presented with a new 3  $\times$  3 matrix word scramble and given 30  
1007 seconds to find as many words as they could, up to a maximum of 15 words. Next, they provided  
1008 their self-estimated placement on the word task, on a scale from 1<sup>st</sup> percentile to 99<sup>th</sup> percentile.  
1009 Unlike our previous studies using the weight-guessing task, but consistent with other work  
1010 employing this type of word task (Caputo & Dunning, 2005), participants demonstrated self-  
1011 knowledge in their performance; self-estimated placement and actual placement correlated  
1012 positively ( $r = .37, p < .001$ ).

---

<sup>10</sup> Pilot participants were instructed to write about someone with specific personality characteristics (e.g., someone especially nice). The personality prompts did not ultimately have any main or interactive effects on actors' overconfidence, nor did they influence self-reported memory of task. Thus, these results are not discussed further.

1013           Following this word task, actors completed the familiar weight-guessing task. They were  
1014 either shown the same two photographs as at Time 1 first or two new photographs first (order  
1015 counterbalanced across subjects), seeing four photographs total. They answered the same  
1016 questions as at Time 1. For the two photographs that were also shown at Time 1, actors were  
1017 reminded that the photographs also appeared in the previous survey. They provided self-  
1018 estimated placement at the end of the two repeated photographs, and then again at the end of the  
1019 two novel photographs.

## 1020 **Results and Discussion**

1021           **Analytic plan.** Our analytic approach here parallels that in Studies 2-4. In each  
1022 regression model, actor overplacement was regressed on partner self-assessment condition (0 =  
1023 calibrated partner; 1 = overplacing partner). A baseline model was estimated along with an  
1024 additional model that added covariates, including gender, age (centered), and memory of task  
1025 (centered; in Time 2 outcomes only, see Supplemental Materials). The resulting coefficient of  
1026 the partner self-assessment predictor estimates the effect of exposure to an overplacing partner,  
1027 controlling for the covariates' effects. Results from these regression models are displayed in  
1028 Table 4.

1029



**Table 4. OLS regression of actor overplacement (in different tasks) on partner self-assessment condition (Study 5). For each outcome variable, presented are the baseline model and a covariate model that additionally controls for gender, age (centered), and memory of task (centered; for Time 2 outcomes only). Printed are coefficients followed by 95% confidence interval and *p*-value in parentheses. The key results highlighted in gray indicate that overplacing partners led to more inflated actor overplacement.**

	DV #1: Overplacement at Time 1: weight-guessing task (2 trials)		DV #2: Overplacement at Time 2: weight-guessing task (4 trials)		DV #3: Overplacement at Time 2: weight-guessing task (2 identical trials as in Time 1)		DV #4: Overplacement at Time 2: weight-guessing task (2 novel trials not played at Time 1)		DV #5: Overplacement at Time 2: word task (2 trials)	
	Baseline Model	Model With Covariates	Baseline Model	Model With Covariates	Baseline Model	Model With Covariates	Baseline Model	Model With Covariates	Baseline Model	Model With Covariates
Partner Self-Assessment Condition (0 = Calibrated; 1 = Overplacing)	20.93*** [15.28,26.59] (0.0000)	20.75*** [15.20,26.30] (0.0000)	12.61*** [5.59,19.63] (0.0005)	12.61*** [5.66,19.55] (0.0004)	13.45** [5.20,21.71] (0.0015)	13.99*** [5.93,22.04] (0.0008)	11.77** [4.05,19.48] (0.0030)	11.22** [3.51,18.94] (0.0046)	9.09* [1.48,16.71] (0.0195)	9.97* [2.39,17.55] (0.0102)
Gender (1 = Male)		7.38** [1.78,12.98] (0.0099)		4.25 [-2.88,11.37] (0.2409)		-0.89 [-9.16,7.37] (0.8312)		9.39* [1.48,17.31] (0.0203)		6.34 [-1.44,14.12] (0.1098)
Age (centered)		-0.37** [-0.60,-0.14] (0.0017)		-0.44** [-0.72,-0.17] (0.0019)		-0.65*** [-0.97,-0.33] (0.0001)		-0.24 [-0.55,0.07] (0.1287)		0.30* [-0.01,0.60] (0.0540)
Memory of Task (centered)				0.87 [-1.80,3.53] (0.5206)		0.82 [-2.27,3.91] (0.6019)		0.92 [-2.04,3.88] (0.5409)		2.76* [-0.15,5.67] (0.0630)
Constant	-4.72* [-8.70,-0.74] (0.0203)	-7.86*** [-12.46,-3.25] (0.0009)	1.97 [-2.89,6.84] (0.4246)	1.33 [-4.28,6.95] (0.6397)	1.41 [-4.31,7.12] (0.6280)	3.27 [-3.25,9.78] (0.3236)	2.54 [-2.80,7.89] (0.3497)	-0.60 [-6.84,5.64] (0.8498)	-15.29*** [-20.56,-10.01] (0.0000)	-18.86*** [-24.99,-12.73] (0.0000)
<i>R</i> <sup>2</sup>	0.116	0.158	0.060	0.118	0.050	0.126	0.044	0.085	0.027	0.070
Adjusted <i>R</i> <sup>2</sup>	0.114	0.151	0.055	0.100	0.045	0.108	0.039	0.066	0.022	0.050
<i>AIC</i>	3866.9293	3842.2777	1859.6341	1844.1468	1924.2342	1903.3819	1897.3359	1886.1401	1892.0275	1879.1026
<i>BIC</i>	3874.9321	3858.2735	1866.2307	1860.6133	1930.8309	1919.8484	1903.9325	1902.6066	1898.6241	1895.5691
Observations	404	403	200	199	200	199	200	199	200	199

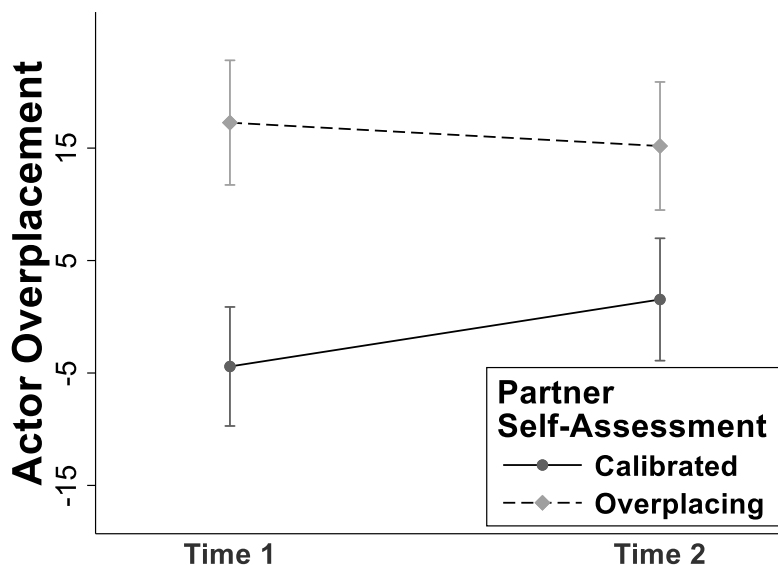
+  $p < 0.10$ , \*  $p < 0.05$ , \*\*  $p < .01$ , \*\*\*  $p < .001$

**Did overplacement transmit at Time 1, immediately after exposure to overplacing others?** Replicating our prior effects, exposure to overplacing partners led actors to increase their overplacement in the weight-guessing task at Time 1 by 20.93 percentile points. Overplacement was significantly higher if the partner overplaced ( $M = 16.22$ ;  $SD = 29.06$ ) than if the partner was calibrated ( $M = -4.72$ ;  $SD = 28.78$ ;  $t(402) = 7.28$ ,  $p < .001$ ,  $d = .72$ , CI of mean difference = [15.27, 26.59]).

**Did the transmission of overplacement persist into Time 2, days after the initial exposure to overplacing others?** The effect of observing overplacing partners persisted into Time 2. Actors' overplacement, as expressed across all 4 trials of the weight-guessing task, was 12.61 percentile points higher if the partner was overplacing ( $M = 14.59$ ;  $SD = 25.88$ ) than if the

1048 partner was calibrated  $\{M = 1.97; SD = 26.03; t(198) = 3.54, p < .001, d = .50, CI \text{ of mean}$   
 1049  $\text{difference} = [5.59, 19.63]\}$ . The same conclusions are reached in subsequent analyses using  
 1050 multi-level models to examine within-person trajectories, as well as when we examined only the  
 1051 novel weight-guessing trials (as opposed to combining both novel and repeated trials; see  
 1052 Supplemental Materials). Together, these results suggest that the transmission effect persisted  
 1053 over several days. In fact, actor overplacement in the overplacing-partner condition did not show  
 1054 a significant decline from Time 1 to Time 2 within-person ( $z = -0.64, p = .522, CI \text{ of mean}$   
 1055  $\text{difference} = [-6.36, 3.23]$ ).

1056 **Figure 7. Actor Overplacement Change (Within-Person Trajectory) at Initial Partner**  
 1057 **Exposure (Time 1) and Days Later (Time 2) by Partner Condition (Study 5).**  
 1058



1059  
 1060

1061 *Figure 7.* Model estimated overplacement in percentiles (and 95% confidence intervals) of  
 1062 participants on two identical trials of a weight-guessing task at two time points (separated by  
 1063 several days). Participants were randomly assigned (between-subjects) to view a partner who  
 1064 was either calibrated or overplacing at Time 1. Positive percentile values index overplacement, 0  
 1065 indexes perfect calibration, and negative values index underplacement. An overplacing partner  
 1066 led to substantial overplacement at Time 1. Moreover, these inflated self-estimated placements  
 1067 persisted and remained elevated even days later at Time 2.

1068

1069           **Did the transmission of overplacement extend to a novel task domain at Time 2?** The  
1070 transmission of overplacement spilled over from the weight-guessing task to the word scramble  
1071 task. Actors' self-placement on the word task was 9.09 percentile points higher if the partner  
1072 overplaced on the weight-guessing game ( $M = -6.19$ ;  $SD = 27.86$ ) than if the partner was  
1073 calibrated ( $M = -15.29$ ;  $SD = 26.74$ ;  $t(198) = 2.36$ ,  $p = .019$ ,  $d = .33$ , CI of mean difference =  
1074 [1.48, 16.71]). Thus, using this task on which participants had some insight into where they  
1075 actually place (as revealed by  $r = .37$  between estimated and actual placement), we obtain the  
1076 same general pattern of results as the weight-guessing task on which they lacked insight, though  
1077 the effect is slightly attenuated. Note that in this task, the majority of participants underplaced  
1078 (65% of participants were underplacing, compared to 35% on the weight guessing task). This is  
1079 likely due to the perceived difficulty of this task (Moore & Small, 2007). Therefore, it is more  
1080 appropriate to describe actors as being *less underplacing* in the overplacing partner condition  
1081 than in the calibrated partner condition. Nonetheless, these results still suggest that the social  
1082 influence of peers is non-domain-sensitive, shaping overplacement in a distinct and unrelated  
1083 domain.

1084           **Summary.** Taken together, Study 5 extends our understanding of the reach of  
1085 overconfidence transmission. Exposure to confidence in the form of high self-placement  
1086 produces effects that are temporally persistent and resistant to erosion. Said exposure not only  
1087 influenced confidence in the original domain in which others' confidence was observed, but even  
1088 in a new task domain and environment. Moreover, as was found in Study 4, participants were  
1089 unaware of the influence of overplacing peers on their own self-assessments (see Supplemental  
1090 Materials). Overall, by documenting the longevity, persistence, and domain-generality of the

1091 transmission of overplacement, the current results begin to offer insights into the extensive scale  
1092 at which overconfidence may spread.

### 1093 **Study 6: The Transmission of Overplacement and Coalitional Membership**

1094 In the previous studies, confidence was expressed by a partner who was portrayed as a  
1095 participant in the same study. Therefore actors might have seen the partner as being similar to  
1096 themselves. Theories of cultural evolution propose a self-similarity bias (Henrich & Broesch,  
1097 2011; Henrich & Henrich, 2007; McElreath et al., 2003), or a proclivity for individuals to  
1098 preferentially learn from models who are “like them”—for example, models of the same sex or  
1099 ethnicity, or who share similar personality and physical attributes, or who are part of their in-  
1100 group. This form of selective learning offers individuals the best chance of acquiring traits and  
1101 mental representations (practices, skills, values, beliefs, social norms) that permit them to  
1102 effectively coordinate, interact, and cooperate with other members of their social group (Chudek  
1103 & Henrich, 2011).

1104 Based on this reasoning, we test in Study 6 whether individuals are more likely to acquire  
1105 overplacement expressed by models more similar to the self. The specific domain of self-  
1106 similarity we focus on here is coalitional member in-group bias, a dimension of similarity that  
1107 both predicts fitness and has been relevant for eons (i.e., documented in other primates and in  
1108 small-scale societies; (Kurzban et al., 2001; Silk, 2007; von Rueden et al., 2011), and guides  
1109 social decision-making beginning as early as infancy (Bian et al., 2018; Wilks et al., 2018; Wynn  
1110 et al., 2018). This focus on coalition membership is consistent with our aforementioned interest  
1111 in understanding variation existing within and between groups (including cultural groups) in  
1112 overconfidence. Evidence demonstrating a stronger tendency towards acquiring overconfidence  
1113 from in-group members relative to out-group members would indicate that selective learning

1114 biases such as these may help explain how similarities in overconfidence within cultural groups  
1115 and differences between cultural groups are maintained.

1116         In Study 6, we experimentally manipulate a model's coalition status (in- vs. out-group)  
1117 by drawing on recent empirical work indicating that sports rivalry is a potent social category that  
1118 incites an in- vs. out-group psychology in many modern societies (Kruger et al., 2018; Winegard  
1119 & Deaner, 2010). Consistent with the notion of a selective in-group bias in internalizing  
1120 confidence standards, we expect individuals to readily acquire overplacement when it is  
1121 displayed by in-group members, but to be less or not at all influenced by overplacing out-group  
1122 members. Put differently, we predict that partner coalitional membership will moderate the effect  
1123 of exposure to partner overplacement. These results offer a first examination of the boundary  
1124 conditions under which confidence standards do and do not spread, and, by implication, how  
1125 selective social transmission maintains within-group similarity and between-group heterogeneity.

## 1126 **Method**

### 1127 **Participants.**

1128         Through a campus-wide solicitation at the University of Illinois at Urbana-Champaign,  
1129 we recruited 248 participants (63.71% women) to complete, in-person, a computerized study on  
1130 judgment and decision-making. We chose a target sample size of 60 participants per condition  
1131 (targeted  $N = 240$  for all 4 conditions combined). Participants' ages ranged from 17 to 33 ( $M =$   
1132  $19.88$ ,  $SD = 10.29$ ). Similar to Studies 3 and 4, participants received a candy bar for participating  
1133 and were entered into a raffle to win \$10 based on their performance and calibration. In our  
1134 analyses below we report results from all participants.

1135           **Experimental Procedure.**

1136           Our procedure was similar to Study 2. Participants (hereafter termed “actors”) viewed the  
1137 ostensive responses of a previous participant (hereafter termed “partner”) in a weight-guessing  
1138 task, and subsequently completed two trials of the task. However, in Study 6 we also  
1139 manipulated the group membership of the partner, thereby creating four experimental conditions  
1140 in a 2 (partner self-assessment: overplacing vs. calibrated) × 2 (partner group membership: in-  
1141 group vs. out-group) between-subjects design.

1142           We manipulated partner group membership by varying the partner’s university affiliation.  
1143 Specifically, just before viewing the partner’s responses, actors in the in-group partner conditions  
1144 read that “... like you, [this person] also attends University of Illinois”. By contrast, actors in the  
1145 out-group partner conditions read that “... unlike you, [this person] attends The Ohio State  
1146 University, our biggest rival in college football” (for full instructions, see Supplemental  
1147 Materials). To strengthen this manipulation, actors were asked to reflect on and describe in 3-4  
1148 sentences the ways in which they were similar (in the in-group partner treatment) or dissimilar  
1149 (in the out-group partner treatment) to the partner.<sup>11</sup>

1150           Actors then completed two trials of the weight-guessing task, after which they reported  
1151 their demographic details, knowledge of football news and events, and identification with the in-  
1152 group to serve as control variables. Finally, actors responded to open-ended questions that  
1153 probed for suspicion about the study (no participant indicated concerns with the veracity of the  
1154 purported partner).

---

<sup>11</sup> After viewing the partner description, we administered a vigilance check. Actors were asked to select the university affiliation of the partner whose response they just viewed from a list of 14 universities. 82.66% of actors correctly identified the university of the partner (84% in the in-group partner condition, 81.3% in the out-group partner condition, respectively). In our analyses below, we report results from all actors regardless of their response. However, we note that the same pattern of results was obtained in follow-up analyses restricted only to actors who passed this vigilance check.

1155 The key dependent measure was actor overplacement, which was computed using the  
 1156 same procedure as in Studies 2-5. Again, actors' self-estimated placement and actual placement  
 1157 were uncorrelated ( $r = .07, p = .255$ ), consistent with the prior studies that employ the same task.

## 1158 Results and Discussion

1159 The self-similarity argument predicts a greater likelihood to adopt the confidence of a  
 1160 coalitional in-group member, relative to an out-group member. To test this prediction, we  
 1161 regressed actor overplacement on the main effects and interaction of partner self-assessment  
 1162 condition (calibrated vs. overplacing partner) and partner group membership condition (in-group  
 1163 vs. out-group). In the other specifications, we additionally include a number of control variables:  
 1164 actor gender, age, ethnicity, knowledge of collegiate football, and identification with the  
 1165 university in-group.

1166 Our regression models (displayed in Table 5) show that the coefficient for the partner  
 1167 self-assessment  $\times$  partner group membership interaction is large and significant at conventional  
 1168 levels across all models, with and without the controls.

1169 **Table 5. OLS regression of actor overplacement scores on partner self-assessment**  
 1170 **condition and partner group membership condition, and their interaction (Study 6).**  
 1171 **Subsequent models control for gender, ethnicity, age (centered), football knowledge**  
 1172 **(centered), identification with in-group (centered), and ethnicity. Printed are coefficients**  
 1173 **followed by 95% confidence interval and  $p$ -value in parentheses. The key results**  
 1174 **highlighted in gray indicate that partner group membership significantly moderates the**  
 1175 **effect of partner self-assessment on actor overplacement.**  
 1176

	Baseline Model	Model with Covariates	Model with Covariates	Model with Covariates	Model with Covariates	Model with Covariates
Partner Self- Assessment Condition (0 = Calibrated; 1 = Overplacing)	19.54***	19.61***	19.46***	19.63***	20.40***	20.50***
	[9.59,29.50] (0.0001)	[9.66,29.56] (0.0001)	[9.49,29.43] (0.0002)	[9.60,29.65] (0.0001)	[10.38,30.43] (0.0001)	[10.47,30.53] (0.0001)
Partner Group Membership Condition (0 = In-Group; 1 = Out-Group)	4.25	4.11	4.05	4.13	5.19	5.18

	[-5.99,14.49] (0.4146)	[-6.13,14.36] (0.4298)	[-6.21,14.30] (0.4378)	[-6.15,14.42] (0.4294)	[-5.13,15.51] (0.3226)	[-5.13,15.50] (0.3234)
Partner Self-Assessment Condition × Partner Group Membership Condition	-16.09*	-15.46*	-15.29*	-15.49*	-16.87*	-16.96*
	[-30.19,-1.99] (0.0255)	[-29.60,-1.33] (0.0322)	[-29.45,-1.13] (0.0345)	[-29.71,-1.27] (0.0329)	[-31.13,-2.62] (0.0206)	[-31.22,-2.70] (0.0199)
Gender (1 = Male)		4.12	4.39	4.25	3.89	4.20
		[-3.25,11.49] (0.2724)	[-3.03,11.81] (0.2453)	[-3.22,11.71] (0.2639)	[-3.56,11.34] (0.3048)	[-3.28,11.68] (0.2697)
Ethnicity (0 = Caucasian; 1 = Non-Caucasian)			-2.44	-2.33	-1.51	-1.05
			[-9.55,4.68] (0.5006)	[-9.48,4.81] (0.5209)	[-8.69,5.67] (0.6791)	[-8.29,6.20] (0.7761)
Age (centered)				0.30	0.49	0.55
				[-1.21,1.81] (0.6917)	[-1.03,2.00] (0.5296)	[-0.97,2.08] (0.4762)
Football Knowledge (centered)					1.94 <sup>+</sup>	1.62
					[-0.30,4.18] (0.0893)	[-0.71,3.95] (0.1723)
Identification with In-Group (centered)						1.70
						[-1.78,5.18] (0.3374)
Constant	-5.65 [-13.05,1.75] (0.1336)	-7.27 <sup>+</sup> [-15.21,0.67] (0.0726)	-6.20 [-14.74,2.34] (0.1539)	-6.28 [-14.84,2.29] (0.1500)	-7.30 <sup>+</sup> [-15.92,1.31] (0.0961)	-7.66 <sup>+</sup> [-16.31,0.98] (0.0820)
<b>R<sup>2</sup></b>	0.066	0.071	0.072	0.073	0.084	0.088
<b>Adjusted R<sup>2</sup></b>	0.054	0.055	0.053	0.050	0.057	0.057
<b>AIC</b>	2362.3374	2363.1056	2364.6396	2366.4775	2365.4883	2366.5313
<b>BIC</b>	2376.3911	2380.6727	2385.7202	2391.0715	2393.5957	2398.1522
<b>Observations</b>	248	248	248	248	248	248

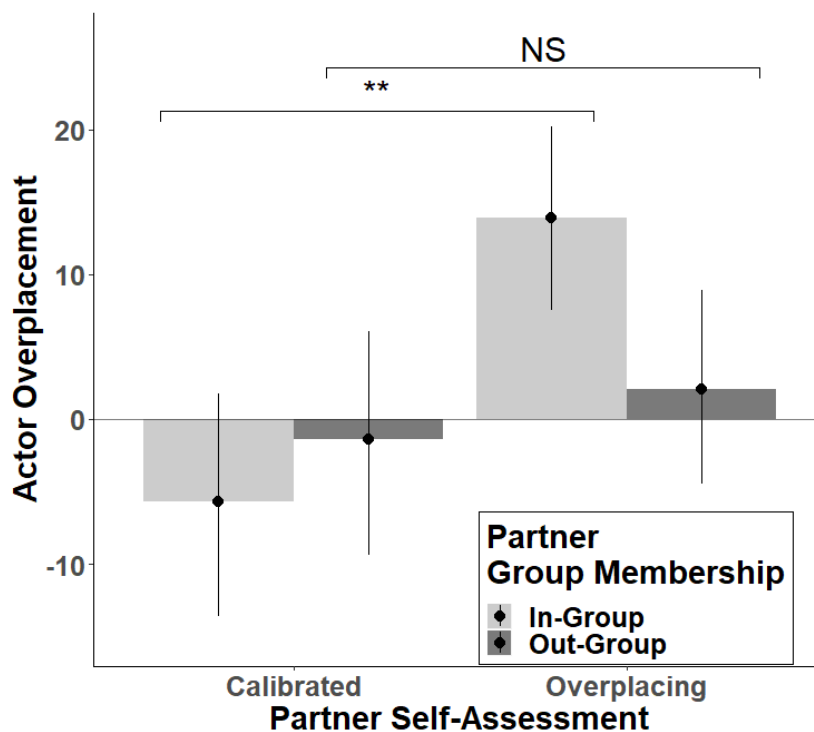
1177 + p < 0.10, \* p < 0.05, \*\* p < .01, \*\*\* p < .001

1178  
1179 To probe this significant interaction that emerged, we next examined simple effects  
1180 separately for each partner group membership condition (the moderator). Our key finding, based  
1181 on the baseline model (with no controls), is depicted in Figure 8. In the in-group partner  
1182 condition, actor overplacement was significantly higher if the partner overplaced ( $M = 13.89$ ;  $SD$   
1183  $= 26.03$ ) than if the partner was calibrated ( $M = -5.65$ ;  $SD = 29.35$ ;  $t(123) = 3.87$ ,  $p < .001$ , CI of  
1184 mean difference =  $[6.13, 32.95]$ }, indicating the spread of overplacement between in-group



1185 members.<sup>12</sup> This result mirrors our findings in Studies 2-5. By contrast, in the out-group partner  
 1186 condition, actor overplacement did not significantly differ as a function of exposure to an  
 1187 overplacing partner ( $M = 2.05$ ;  $SD = 26.84$ ) or a calibrated partner ( $M = -1.40$ ;  $SD = 30.36$ ;  
 1188  $t(121) = .68$ ,  $p = .496$ ; CI of mean difference =  $[-9.99, 16.89]$ ). Finally, these simple effects  
 1189 produce the same basic findings across our other specifications with controls—all of which  
 1190 indicate a significant effect of partner self-assessment on actor overplacement only in the in-  
 1191 group partner condition, but null effects in the out-group partner condition, consistent with a  
 1192 selective learning bias.

1193 **Figure 8. Actor Overplacement by Partner Self-Assessment and Coalitional Status**  
 1194 **Condition (Study 6).**  
 1195



1196  
 1197 *Figure 8.* Raw overplacement in percentiles (and 95% confidence intervals) expressed by  
 1198 participants exposed to a partner who varied in self-assessment (overplacing vs. calibrated) and

<sup>12</sup> The magnitude of this effect ( $d = .70$ ) is similar to and closely replicates Study 2 ( $d = .85$ ). This is a close replication because in Study 2, the observed partner was similarly described as a peer from the same university, mirroring the in-group manipulation deployed here.

1199 coalitional group membership (in-group vs. out-group). Positive percentile values on actor  
1200 overplacement indexes overplacement, 0 indexes perfect calibration, and negative values index  
1201 underplacement. Consistent with an in-group bias for acquiring norms and behaviors,  
1202 participants selectively aligned their self-estimated placements with that of an in-group member  
1203 but not with that of an out-group member. Overplacement peaked and was strongest when  
1204 exposed to an overplacing in-group partner, compared to when this overplacing partner was an  
1205 out-group member. By contrast, when the partner is an out-group member, their self-assessment  
1206 did not significantly produce changes in actor overplacement, consistent with a significant  
1207 interactive effect between partner group membership and partner self-assessment.

1208  
1209 **Summary.** Together, these results provide clear and robust evidence of in-group biased  
1210 transmission of overplacement, and in doing so delineate the boundary conditions under which  
1211 overplacement spreads. Participants readily used the overplacement of in-group others to adjust  
1212 their own self-assessments, while discounting the overplacement of out-group others, who they  
1213 observed but selectively ignored. Thus, despite the tendency to align our expressed confidence  
1214 with that of our social partners, the characteristics of the partner matter; social transmission is  
1215 attenuated when one's interaction partner is highly dissimilar. This pattern is consistent with  
1216 existing work showing that people use cues of self-similarity to tailor their cultural learning  
1217 (Boyd & Richerson, 1987; Chudek & Henrich, 2011; Henrich & Broesch, 2011; Henrich &  
1218 Henrich, 2007; McElreath et al., 2003), demonstrating for example a heightened preference to  
1219 learn from those who share, for example, their ethnic markers (e.g., dialect, accent; Kinzler,  
1220 Shutts, DeJesus, & Spelke, 2009; Shutts, Kinzler, McKee, & Spelke, 2009), gender (Bandura et  
1221 al., 1961; Bussey & Bandura, 1984; Shutts et al., 2010), and taste and beliefs (Hilmert et al.,  
1222 2006). Our results add to this work by highlighting how in- vs. out-group membership is yet  
1223 another self-similarity cue used by social learners to (try to) equip themselves with the most  
1224 relevant and fitness-enhancing cultural information.

1225 These results also shed new light on how differences in overconfidence across groups can  
1226 emerge (Chudek et al., 2012; Henrich, 2016; Henrich & Broesch, 2011). Selectively acquiring

1227 the overconfidence from one's own social group means that, when operating across occasions  
1228 and individuals, the kinds of in-group biased imitative processes demonstrated here can  
1229 potentially generate substantial variation between groups, while maintaining relative  
1230 homogeneity among entities within these local contexts. As a result, these micro-level  
1231 transmission processes operating within interactions among individual entities could aggregate to  
1232 generate population-level patterns of cultural variation, accelerating the emergence and  
1233 stabilizing of group-level differences in overconfidence to explain how groups, organizations,  
1234 and states come to differ in this trait.<sup>13</sup>

### 1235 **General Discussion**

1236 Of the many psychological biases, fallacies, and illusions that humans exhibit,  
1237 overconfidence has been described as one of the most powerful, widespread, and perplexing  
1238 (Johnson & Fowler, 2011; Kahneman, 2011). Why do different levels of overconfidence cluster  
1239 within a variety of ecological contexts, such that individuals within the same group, team,  
1240 culture, or organization often have a correlated degree of bias? Prior explanations addressing this  
1241 question have primarily focused on “evoked culture” and ecology-specific responses to local  
1242 constraints and rewards as factors that give rise to false, exaggerated beliefs in some contexts,  
1243 and accurate, unbiased assessments in others (Haselton et al., 2015; Johnson et al., 2013;  
1244 Johnson & Fowler, 2011; Schwardmann & Weele, 2019; Sharot, 2011, 2012). The current

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<sup>13</sup> Note that these results also indicate, suggestively, that the patterns we have observed across studies captures a social transmission process, rather than competitive matching. That is, emerging work has linked overconfidence to success in competitions (C. Anderson et al., 2012; Murphy et al., 2015; Niederle & Vesterlund, 2007), revealing how competitive contexts may even spur unrealistic confidence (Cain et al., 2015; Moore et al., 2007; Radzevick & Moore, 2011). Therefore, an alternative explanation for our findings is that actors aspire to match or even out-compete their partner by expressing even more confidence (and thus also a stronger overconfidence bias). The designs of our prior studies suggest that this explanation is unlikely, given that actors were not in direct competition with partners, and in Study 1, were even collaborating with them. These findings from Study 6 further refute a competition account: Participants were more likely to align with in-group partners' overplacement than out-group partners' overplacement, even though there are presumably more competitive feelings toward out-group than in-group members.

1245 research extends this existing literature by testing a new social transmission account of  
1246 overconfidence, which proposes that individuals acquire overconfident tendencies from others in  
1247 their social environment through social learning. In this account, confident others (particularly  
1248 in-group members) create and heighten the propensity to adopt an overconfident cognitive style.  
1249 This social learning propensity allows individuals to rapidly and efficiently acquire local  
1250 confidence norms, shapes their propensity to exhibit overconfidence, and, on a broader scale, the  
1251 strength of this bias within groups. Thus, the acquisition of confidence norms may play a key  
1252 role in how within-group similarities (and between-group differences) in overconfident  
1253 tendencies are maintained.

1254         Here, results from six studies, using both correlational and experimental designs, provide  
1255 support for the overconfidence transmission hypothesis. These studies utilize methodologies that  
1256 elicit overplacement in a manner that addresses important methodological concerns raised in  
1257 prior work, including deploying financial incentive to increase motivation for accurate self-  
1258 assessments (and decrease self-presentation motivation; Camerer & Hogarth, 1999; Hoelzl &  
1259 Rustichini, 2005) and disentangling warranted confidence from unwarranted confidence by  
1260 measuring actual performance (Moore & Healy, 2008). Study 1 revealed that, under controlled  
1261 laboratory conditions, face-to-face collaboration led individuals randomly assigned to work in a  
1262 dyad to converge in overplacement, such that a positive correlation between dyad members'  
1263 overplacement emerged following (but not before) the interaction. Moreover, consistent with the  
1264 proposed social transmission process, one partner's pre-interaction overplacement predicts the  
1265 change in the other person's overplacement from pre- to post-interaction. In subsequent studies  
1266 (Studies 2-6) we build on this initial evidence to more firmly establish the causal influence of  
1267 overplacing peers on observers. Overconfidence was found to spread as a direct result of

1268 individuals' tendency to align with the confidence tendencies observed in peers, even when they  
 1269 are unwarranted and represent overplacement. Combined, our major finding across all six studies  
 1270 suggests that, by operating on our existing proclivities for social learning, locally relevant  
 1271 confidence traditions, even when cued by overconfident models, are readily acquired and act to  
 1272 increase our propensity towards overplacement.

1273 Our results also reveal five other patterns that characterize the transmission effect and  
 1274 that operate to allow overconfidence to spread widely:

- 1275 1. *Indirect transmission*: overplacement spreads not only from one person to another, but  
 1276 also across indirect ties from person to person to person. Third-parties' propensity  
 1277 towards overplacement is heightened by an overconfident model to whom they are only  
 1278 indirectly connected through another peer (Study 3), highlighting the extensive reach of  
 1279 confident peers.
- 1280 2. *Temporal stability*: the transmission effect may be temporally stable to a certain degree.  
 1281 In our studies, overplacing peers continued to induce biased beliefs in the later stages of  
 1282 the experiment when exposure to peer ceased (Study 4), as well as, quite remarkably,  
 1283 several days following this initial exposure (Study 5).
- 1284 3. *Outside of conscious awareness*: the influence of overplacing peers on self-estimated  
 1285 placements appears to operate "stealthily", occurring largely outside of conscious  
 1286 awareness. Individuals failed to detect the substantial influence of overplacing peers  
 1287 (Studies 4-5). Efforts to resist acquiring bias from overconfident peers, and reduce bias  
 1288 more generally, may be especially challenging in the absence of personal awareness and  
 1289 self-knowledge (Cassam, 2017).
- 1290 4. *Cross-domain generality*: the transmission effect may operate across domains. Observing  
 1291 peers express unwarranted confidence in weight-guessing carries over and produces  
 1292 greater overplacement in word tasks (Study 5). Note that while these results are  
 1293 necessarily tentative due to the relative brief time-span and limited domains examined  
 1294 here, and should be further examined in future studies, the current data nonetheless open  
 1295 up important new avenues for future research by highlighting the possible temporal  
 1296 persistence and cross-domain generality of overconfidence transmission.
- 1297 5. *In-group biased transmission*: the general effect of overconfidence transmission is  
 1298 qualified by an important factor: in-group selective social learning. That is, individuals do  
 1299 not copy indiscriminately. Instead, they are sensitive to *whose* mental representations are  
 1300 on display and selectively acquire the overplacement of in-group but not out-group  
 1301 members, consistent with the long emphasis on the acquisition of self-relevant and  
 1302 adaptive information in theories of cultural learning.

1303           These results emerged despite several features of our methodological procedures that  
1304 may temper overconfidence (and its transmission). Overplacement spreads from one person to  
1305 another even when: (a) individuals have perfect information that the peer is overplacing, rather  
1306 than well-calibrated, through information that highlights how their self-estimated placement  
1307 exceeds actual placement (Studies 2-6); (b) individuals lack perfect information about the peer's  
1308 overplacement but must instead infer it from behavior (Study 1); (c) calibration is incentivized  
1309 (over bias), which aligns the costs of overconfidence expressed in our studies with the potential  
1310 costs of faulty decisions driven by overconfidence in the real world (Studies 2-4 and 6); and (d)  
1311 peers and observers respond to different, rather than identical, stimuli, indicating the  
1312 transmission of an overconfident mindset in assessing one's capabilities on novel items, beyond  
1313 simply copying a peer's responses (and their confidence) to identical stimuli (cf. Paese &  
1314 Kinnaly, 1993; Studies 2-6).

### 1315 **Theoretical Implications**

1316           **Social transmission and clustering of overconfidence within groups.** This research  
1317 began by seeking to address a puzzling question: Why does the degree of confidence often  
1318 cluster between individuals who belong to the same community, to the point of producing what  
1319 appears to be group- or even culture-wide traditions of overconfidence? Our findings suggest  
1320 that cultural transmission may be one mechanism that partially explains how group-level  
1321 differences in overconfidence are maintained (Boyd & Richerson, 1985; Cohen, 2001; Cohen et  
1322 al., 1996; Morgan et al., 2011; Nisbett & Cohen, 1996). Theorists have proposed that cultural  
1323 learning is “the primary engine that produces the bulk of stable variation across groups” (Heine  
1324 & Norenzayan, 2006, p. 260; also see Richerson & Boyd, 2005), explaining why genetically  
1325 similar individuals living in similar environments, but in different social groups, may possess

1326 strikingly different beliefs, practices, and psychological tendencies. Empirically, there is a  
1327 swelling tide of supportive evidence from across the social sciences confirming that many of  
1328 these patterns of cross-group variation stem from social transmission (Boyd et al., 2011; Boyd &  
1329 Richerson, 1985; Henrich & Gil-White, 2001; Mesoudi, 2011; Mesoudi et al., 2004; Nisbett et  
1330 al., 2001; Rendell et al., 2010, 2011; Richerson & Boyd, 2005). Applying this approach to the  
1331 case of overconfidence, it stands to reason that, similar to these culturally varying behaviors and  
1332 psychological tendencies, the observed variation in overconfidence across human populations  
1333 may be rooted in social transmission that occurs among regularly interacting social entities. Of  
1334 course, these studies focused solely on overplacement. An important direction for future work is  
1335 to test whether the transmission account proposed here extends to other separable forms of  
1336 overconfidence, including overestimation and overprecision (Moore & Healy, 2008;  
1337 Muthukrishna et al., 2018).

1338         **The origins of overconfidence.** A second contribution of this research involves adding  
1339 to the growing theoretical and empirical interest across psychology, economics, evolutionary  
1340 biology, organizational behavior, and other disciplines in understanding how individual  
1341 differences in overconfidence arise—that is, the proximate explanations for why some  
1342 individuals are more overconfident than others (C. Anderson et al., 2012; Johnson et al., 2006,  
1343 2011; Johnson & Fowler, 2011; Marshall et al., 2013; Murphy et al., 2015, 2017; Van den Steen,  
1344 2004; von Hippel & Trivers, 2011). Traditional answers to this question generally invoke  
1345 biological and personality trait-like factors to explain inter-individual differences in the degree  
1346 (and direction) of bias towards overconfidence. For example, this work reveals that the  
1347 magnitude of inflated beliefs is higher in men compared to women, and intensifies with  
1348 increased testosterone and psychological traits that propel pride and hubris, such as narcissism,

1349 sense of power, and perception of control (e.g., Fast, Sivanathan, Mayer, & Galinsky, 2012;  
1350 Gneezy, Niederle, & Rustichini, 2003; Pallier et al., 2002; Paulhus et al., 2003; Tracy & Robins,  
1351 2007).

1352         While these existing studies offer valuable insights, individual differences turn out to  
1353 have relatively limited explanatory power (Moore & Dev, 2019), arguably because they fail to  
1354 incorporate the crucial roles of social influence and peer effects. Our results here, combined with  
1355 the existence of within-group similarity and between-group variation in average overconfidence,  
1356 discredit the idea that the endogenous traits or attributes of a person *alone* explains  
1357 overconfidence; the degree of confidence expressed by those around us must play a crucial role.  
1358 Thus our findings contribute to the existing literature by identifying social transmission as a key  
1359 mechanism—overconfidence can arise, in part, from proximity to (over)confident individuals.  
1360 We submit that a complete understanding of the roots of overconfidence requires acknowledging  
1361 that, like many other important human behaviors and practices, overconfidence is in part shaped  
1362 by local ecological environments and socially by the behavior of others. Note, however, we  
1363 suggest that these determinants and pathways are best seen as complementary, rather than  
1364 contradictory, explanations of the roots of overconfidence. We think it is only through  
1365 integrating and examining the interactions among the large suite of bias-inducing factors that we  
1366 can address and begin to fully understand how overconfidence traditions arise.

### 1367 **Alternative Explanations of How Different ‘Overconfidence Traditions’ Arise**

1368         There are other reasons we do not examine here that can also explain why overconfidence  
1369 proclivities converge within-groups and diverge across groups. After all, there is little doubt that  
1370 a complex set of mechanisms likely underlies this human cognitive diversity. One especially  
1371 prominent and compelling theoretical explanation for cultural variation emphasizes “evoked



1372 culture” and habitat-specific responses, which consider how behavioral and cognitive variation  
1373 arise as adaptive, evoked responses to differences in immediate environmental conditions  
1374 (Gangestad et al., 2006; Hill & Hurtado, 1996; Tooby & Cosmides, 1992). This logic, when  
1375 applied to overconfidence, proposes that variation in levels of false assessments is a response to  
1376 different ecological circumstances, with greater bias observed in environments that confer  
1377 greater rewards for confidence displays and competitive behavior incited by overconfidence  
1378 (Heine, 2011; Johnson & Fowler, 2011; Leibbrandt et al., 2013; Radzevick & Moore, 2011).  
1379 From this view, the pervasiveness of overconfidence observed in Wall Street investors stems  
1380 directly from the enormous financial and prestige incentives that reward overconfidence (and  
1381 that outweigh the occasional costs from risky investments and mistakes (Haselton et al., 2015;  
1382 Johnson et al., 2013; Johnson & Fowler, 2011; Sharot, 2011, 2012). Thus, the strength of the  
1383 overconfidence bias represents different cultural adaptations that arise from different ecological  
1384 and economic niches (Diamond, 1997; Triandis, 1994).

1385       Importantly, however, as we mentioned above, these two logically theoretical  
1386 explanations—cultural evocation and transmission—are not mutually exclusive. Recognizing  
1387 that overconfidence may arise from social transmission does not imply that it is irresponsible to  
1388 local benefits (and costs). To the contrary, these two processes likely interact to maintain and  
1389 reinforce intragroup similarities and intergroup differences in overconfidence (Mesoudi et al.,  
1390 2006). Some individuals in a group or population may calibrate their overconfidence to the local  
1391 optimal strategy, then these variants spread within a group and lead individuals to converge on a  
1392 common degree of overconfidence. For example, in the United States, the most individualistic  
1393 society in the world (Oyserman et al., 2002), unusually high levels of overconfidence may be  
1394 triggered by cues of relatively large net payoffs associated with outcomes of competition and

1395 conflict (cues such as cultural values that emphasize success, freedom, and self-sufficiency),  
1396 which then spread (and perhaps even become amplified) as individuals copy the expressed  
1397 confidence and inflated beliefs observed in social interactions, perhaps especially from  
1398 prestigious models who express a great deal of confidence. The point is that, insofar as cultural  
1399 evocation alone is unlikely sufficient for explaining all forms of intergroup variation in  
1400 overconfidence, a complete understanding of these patterns requires considering the social  
1401 transmission of the propensity towards inflated assessments.

#### 1402 **Limitations and Future Directions**

1403         These findings lay the groundwork for a number of fertile avenues for future research.  
1404 One direction is to examine the spread of overconfidence in larger groups, such as in large-scale  
1405 face-to-face social networks, beyond the dyadic peer effects and interpersonal influence  
1406 outcomes examined here. Over the past decade, the study of people’s social networks and ties  
1407 within the communities to which they belong has generated considerable field evidence  
1408 documenting how a wide variety of psychological and behavioral phenomena spread across  
1409 social ties and in populations of thousands—from happiness, creativity, and loneliness to risk  
1410 preferences, moral norms, cooperation, and voting behavior (Bond et al., 2012; Cacioppo et al.,  
1411 2009; Christakis & Fowler, 2009, 2013; FeldmanHall et al., undefined/ed; Fowler & Christakis,  
1412 2008; Jordan et al., 2013; Liu & Zuo, 2019; Mitchell, 2019). Applying this approach to examine  
1413 the transmission of overconfidence, especially longitudinally within networks, would enable tests  
1414 of novel questions. These questions might, for example, address the scale and extent of  
1415 transmission or differences between models in social influence (e.g., is the overconfidence of  
1416 friends with higher income more transmissible than that of friends with lower income; the  
1417 relative influence of friends, spouses, siblings, coworkers, neighbors).

1418           Such field research, when combined with a non-experimental approach that assesses how  
1419 within-group homogeneity may arise through spontaneous transmission of biased beliefs, can  
1420 additionally overcome the potential confounding influence of experimenter demand effects in the  
1421 experimental studies presented here. In Studies 2-6, our inclusion of monetary incentives  
1422 encouraged calibration, and discouraged against strictly adopting partner behavior (which likely  
1423 leads to departures from accuracy), partly reduces this concern by pushing in the opposite  
1424 direction of our hypothesis (Zizzo, 2010). Moreover, Study 1 did not provide participants with  
1425 explicit information about partner's overconfidence and thus was not vulnerable to demand  
1426 effects. Nonetheless, in Studies 2-6 we cannot fully eliminate the concern that participants may  
1427 have in part adjusted their confidence levels due to inferring cues that aligning with their partner  
1428 constitutes appropriate behavior in experimental context. Future research should focus on  
1429 addressing this issue by assessing the transmission of naturally occurring overconfidence across  
1430 individuals, as in the assigned dyad study (Study 1).

1431           A second area ripe for future studies concerns tackling the thorny yet crucial question:  
1432 What specific mechanism(s) mediate this pattern of overconfidence transmission? While one  
1433 limitation in these studies—as in much of other work demonstrating transmission effects—is that  
1434 we are unable to empirically isolate the precise mechanisms involved, transmission in the real-  
1435 world likely emerges via a diverse set of mechanisms such as imitation, peer pressure, or other  
1436 psychosocial processes. We speculate that one particularly important avenue to explore is  
1437 whether and how overconfidence transmission may arise from the spread of social norms,  
1438 particularly as they interact with cultural learning biases such as prestige- or confidence-bias  
1439 (i.e., the tendency to preferentially learn from highly respected members of the community, or  
1440 those who express cues of confidence; Birch, Akmal, & Frampton, 2010; Henrich & Gil-White,

1441 2001; Jiménez & Mesoudi, 2019; Rendell et al., 2011), including highly confident individuals  
1442 (C. Anderson et al., 2012; Kennedy et al., 2013; Tenney et al., 2019). Prestige-bias may first  
1443 allow overconfident individuals to introduce a new behavioral standard to the community, such  
1444 as the norm to appear self-assured and confident. Once this practice takes hold, conformist  
1445 tendencies may subsequently take over and allow this behavioral norm to spread even more  
1446 widely to generate group-wide adoption and display of overconfidence. Consistent with this,  
1447 existing work shows that these normative pressures have robust effects in homogenizing within-  
1448 group behavior and generating between-group variation (Henrich & Boyd, 1998), suggesting that  
1449 they may indeed be crucial mechanisms that undergird how cultural climates of overconfidence  
1450 emerge and are maintained between groups.

1451         Yet another relevant mechanism that may facilitate the spread of confidence is informal  
1452 sanctions. Studies of highly collaborative team environments, in which relative modesty and  
1453 humility is the norm, reveal the use of punishment and social ostracism to sanction overconfident  
1454 individuals who violate prevailing norms (C. Anderson et al., 2006, 2008). It remains to be seen,  
1455 however, whether those who deviate from a norm that promotes overconfidence by exhibiting  
1456 underconfidence, for example, may face similar sanctions (Thoma, 2016). It may be the case that  
1457 groups typified by an especially high degree of competition (both within the group or with out-  
1458 groups)—a context that has been shown to promote and reward overconfidence (Radzevick &  
1459 Moore, 2011)—would establish and enforce norms and sanctioning systems that deter  
1460 underconfidence (Tetlock, 2000). Future work should attend to and measure perceptions of  
1461 norms concerning (over)confidence, the link between these norms and the competitive or  
1462 cooperative relationship of the interacting agents, how norms related to an optimal level of  
1463 expressed confidence are internalized and culturally enforced and sanctioned, and how these

1464 norms shape and respond to the transmission of overconfidence (for an expanded discussion of  
1465 the role of social norms and sanctioning, see Supplemental Materials).

1466         A third opportunity for future investigation involves testing whether *underconfidence* can  
1467 also spread socially. Although our primary focus here is on overconfidence, the same reasoning  
1468 predicts that exposure to underconfident others may increase an observer's propensity towards  
1469 underconfidence. In fact, some supporting findings emerged from two of our studies that directly  
1470 examined the effect of underconfident others. In Study 3, the positive association that emerged  
1471 between model and observer overplacement indicates that, interpolating this trend, observing  
1472 underplacing others increases one's bias towards underplacement as well. In Study 4, peers who  
1473 expressed low confidence (even when underplacing) reduced observer confidence (though they  
1474 still remained slightly overplacing on average). Thus, these results, combined with our other  
1475 studies that reveal the confidence-reducing effect of peers who express low confidence (but are  
1476 accurate and unbiased), are generally consistent with the corollary prediction that  
1477 underconfidence is also socially transmissible. However, given the more limited evidence, the  
1478 case of underconfidence transmission must remain tentative and future work is needed. Note,  
1479 however, that this line of inquiry is important because—despite the aforementioned prevalence  
1480 of overconfidence and its many perilous consequences (factors that led to our focus on  
1481 overconfidence here)—underconfidence also brings with it costly mistakes. Individuals with a  
1482 baseline negative bias who, by virtue of underestimating their chances of success, are prone to  
1483 reduced aspirations, morale, and persistence, and a general avoidance of competitive and risky  
1484 ventures that they, in actuality, stand a good chance to gain (Haselton et al., 2015; Johnson &  
1485 Fowler, 2011; Murphy et al., 2017; Nettle, 2004; Niederle & Vesterlund, 2007; Sharot, 2012),  
1486 undermining success in a broad range of domains ranging from mate attraction, social popularity,

1487 and mental health to education and career choices. Thus, even if it turns out that the costs and  
1488 benefits of over- and underconfidence are not symmetrical (Nettle, 2004), establishing whether  
1489 and how both of these errors transmit is required for a full understanding of the conditions that  
1490 lead individuals to stray from accurate and truthful beliefs and associated rational assessment and  
1491 decision-making.

1492         Finally, future work should explore the practical implications of the social transmission  
1493 of over- and under-confidence. One important area involves examining how overconfidence and  
1494 biased decision-making may be curbed in lieu of rational and optimal behavior. Overconfidence  
1495 is linked to an array of pernicious consequences, such as violence and warfare, entrepreneurial  
1496 failures, and stock market bubbles (Bernardo & Welch, 2001; Camerer & Lovallo, 1999;  
1497 Johnson et al., 2006), and thus understanding how to reduce this bias is crucial (Shariatmadari,  
1498 2015). Our results lend support to the overconfidence transmission hypothesis, which posits that  
1499 overconfident beliefs among a few may readily transmit to others and result in a cascade-like  
1500 spread of biased beliefs throughout a social group, team, organization, or society. This implies  
1501 that strategies and principles for designing the structure of organizations, building effective  
1502 teams, and selecting and cultivating aspiring leaders and decision makers ought to consider the  
1503 potentially profound and extensive social influence of an initially small pool of overconfident  
1504 individuals.

### 1505 **Context of the Research**

1506         This work represents an extension of our team's ongoing research into the origins and  
1507 consequences of accurate and inflated self-beliefs. For instance, our research team has explored  
1508 how overconfidence may be rooted in individual-level factors such as the motivation to improve  
1509 one's social standing, for example by pursuing prestige (C. Anderson et al., 2012) and honing  
1510 one's skills (Tenney et al., 2015), as well as contextual factors such as the nature and difficulty

1511 of the task (Logg et al., 2018; Moore & Cain, 2007), the liability and falsifiability of confidence  
1512 claims (Tenney et al., 2019), situational power and authority (Brion & Anderson, 2013), and why  
1513 supplying arguments in verbal disagreements often fails to persuade (Logg et al., in prep).  
1514 Despite these efforts, however, we increasingly recognize that cultural influences represent an  
1515 important but neglected part of this puzzle on the origins of biased (and accurate) beliefs. As we  
1516 note above, this lack of existing work is striking despite much empirical and anecdotal evidence  
1517 documenting extensive cultural variation in the expression of confidence—with some groups  
1518 typified by self-assurance and others by diffidence. This work is therefore motivated by our  
1519 interest in bridging this gap by assessing how, on a micro-level, inter-individual differences in  
1520 overconfidence may stem, in part, from social influence. Future work should investigate the  
1521 precise mechanisms that explain why confidence transmits socially, how overconfidence spreads  
1522 in large social networks beyond dyads, and how the transmission of overconfidence affects  
1523 collective successes and failures.

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