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The Social Transmission of Overconfidence

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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.

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The Social Transmission of Overconfidence

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

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

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

Little is known about the processes that produce social clustering of overconfidence. Although a complex set of factors is likely responsible, here we examine one possible mechanism: social transmission. Social transmission is defined as the process by which attitudes, values, beliefs, and behavioral scripts are passed onto and acquired by individuals and groups (Cavalli-Sforza & Feldman, 1981; Richerson & Boyd, 2005). We test whether exposure to others' expressions of confidence (even when it is unwarranted) increases one's own propensity towards inflated self-assessments. If so, transmission processes that operate on an interpersonal and micro level might help explain within-group similarities and between-group variation in self-assessments that appear on a broader macro level. Such a process could explain, in part, how cultures of overconfidence emerge and persist within social groups and collective entities, as they did among employees of the former Enron corporation, and not among the !Kung people.

Overconfidence: A Prevalent but Also Highly Variable Cognitive Bias

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

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

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

How Do Group Effects in Overconfidence Emerge?

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

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

The Social Transmission of Overconfidence

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

At the core of this hypothesis of overconfidence transmission is the notion of phenotypic transmission: the degree of inflated beliefs in any given individual is influenced by the overconfidence of one's social partners (peers). As an initial foray into this question, here we seek to first document evidence of such a pattern of phenotypic transmission (overconfidence *can* spread), without attempting to pinpoint the specific proximate mechanisms that might generate this transmission (*how* this transmission occurs), owing in part to the well-known difficulty of empirically distinguishing between the mechanisms responsible for social transmission processes (Quispe-Torreblanca & Stewart, 2019).[1]

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

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

[1] 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).

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

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

The Present Research

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

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

[2] Importantly, we emphasize that this theoretical account also applies to *under*confidence. 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 *under*confidence 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.

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

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

We note two shortcomings of this study. First, by soliciting confidence in accuracy at the item level, their measure of overconfidence confounds overestimation and overprecision (Moore & Healy, 2008). Second, in the repeated test, participants actually showed a tendency to rely on peer input, readily revising their own answers by copying the peer's answers on the knowledge test. Given that the self-assessments elicited on the second test captured their confidence in the peer's answers, these assessments in principle conflated confidence in one's own answers with confidence in the peer's. It is therefore unclear whether participants' changes in beliefs reflected increased overconfidence in their own abilities or simply greater confidence in the *peer's* answers. Consistent with this possibility, in an exit survey completed at the end of the experiment, participants in the overconfident-peer condition rated their partner as *more* knowledgeable, suggesting that they indeed placed greater confidence in this overconfident peer. Given the conceptual ambiguity, these results are inconclusive as to whether and what kind of overconfidence spreads socially. The current research, by proposing and testing a framework for understanding the clustering of overconfidence—by isolating and focusing on overplacement in particular—aims to fill this gap.

Overview of Studies

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

Analytically, to calculate the discrepancy between self-estimated placement and actual placement, we simply subtracted actual from estimated placement for all tests of mean differences. For tests of covariation involving overplacement, we used the residuals when regressing self-estimated placement on actual placement, which capture aspects of beliefs that cannot be explained by true performance, consistent with existing approaches (C. Anderson et al., 2012; Cronbach & Furby, 1970; Dubois, 1957; John & Robins, 1994a; see Supplemental Materials for expanded discussion on calculating discrepancy).

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

The data for all present studies are archived and available at https://figshare.com/articles/Social_Transmission_of_Overconfidence/6663200/1. The procedures for data collection in these studies were approved by the Institutional Review Board

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

Study 1: Overconfidence Spreads in Assigned Dyads in the Lab

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

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

Method

Participants.

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

Material and procedure.

Sessions included 4-8 participants, paired randomly into 52 dyads of variable gender composition. After arriving to the laboratory, participants sat at individual computer stations and learned that the study consisted of two parts: an individual component and a dyadic component. In both components, they would guess the personality of target individuals from photographs shown on the computer screen. Each target would be rated on ten traits from 1 (*Does not describe this person at all*) to 7 (*Describes this person very well*). Participants were informed that a rating was considered correct if it was within .50 points above or below the target's "true" personality, which was operationalized as the actual average rating made by the target and eight knowledgeable informants who were friends or coworkers.[3] To incentivize attention and task engagement, the dyad with the highest number of correct answers on the dyadic component received a $200 cash prize.

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

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

Key variables: Overplacement pre- and post-collaboration.

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

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[3] 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).

[4] 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.

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

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

[5] 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).

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

Results and Discussion

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

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

[6] 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.

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

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

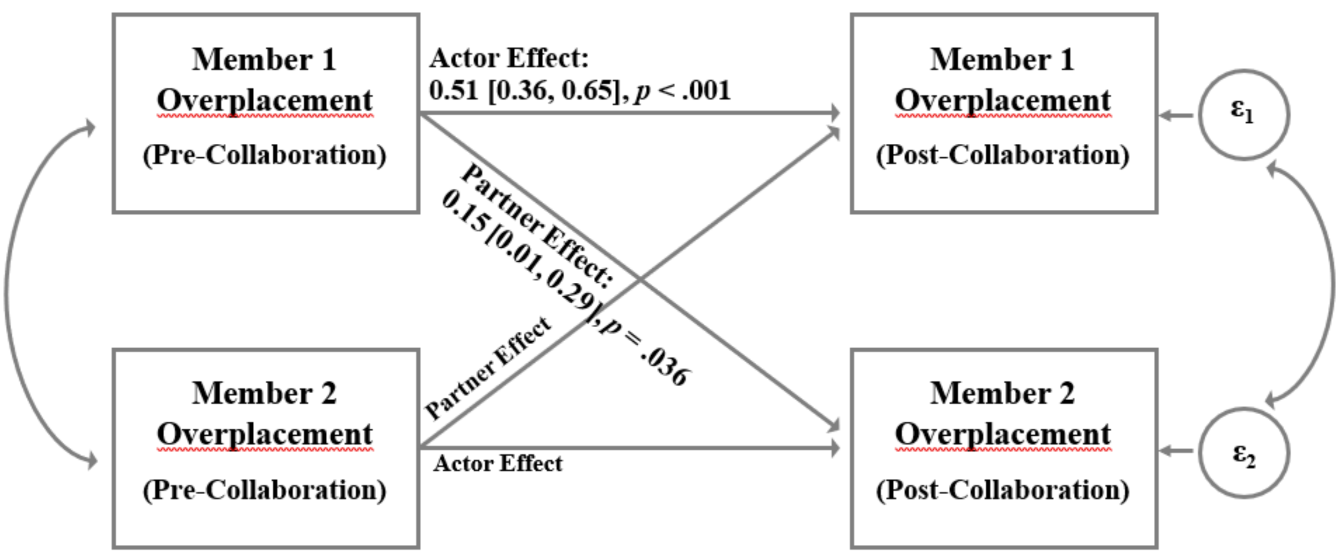

Figure 1. Overplacement post-collaboration explained by the actor-partner interdependence model (APIM; Kenny & Kashy, 2014; Kenny, Kashy, & Cook, 2006) for indistinguishable dyads. The predictor variables are overplacement pre-collaboration for member 1 and member 2, the outcome variables are overplacement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

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

Summary. Results from Study 1 suggest that individuals demonstrate an increased tendency towards overplacement when their partner overplaces, consistent with the overconfidence transmission hypothesis. While we are unable to make strong inferences of causality from these correlational data, we find evidence that after working together, initially non-similar strangers became more similar to each other in overplacement, suggesting the convergence of overconfidence. Importantly, the use of random assignment of partners in a controlled laboratory rules out the possibility that the observed convergence results from the tendency to affiliate with similar others, or from shared exposure to contextual factors prior to participating that shaped both individuals' psychology, both of which are processes that commonly operate in the real-world and thus are difficult to rule out otherwise. Nevertheless, our subsequent studies adopt an experimental approach by testing whether individuals align their self-estimated placement with those seeded in a social partner, and by doing so will provide an effective means of testing whether overplacement transmits under more controlled experimental conditions.

Study 2: Overplacement Spreads from Person to Person

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

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

Method

Participants.

Through a campus-wide solicitation at a large public university in Canada, we recruited 425 participants (65.25% women) for an in-person computerized study on judgment and decision-making. This sample size was determined based on a power analysis in which we assumed an effect size of d = .35 (equivalent to r = .17), using an alpha level of .05 and power of .80, which suggests sampling 130 participants in each of three conditions (targeted N = 390 combined). Data collection terminated at the end of the week in which we attained the target sample size. Participants' ages ranged from 16 to 56 (M = 21.27, SD = 3.59). We informed participants that their responses may be presented to future participants (for the purposes of Study 3; see below), but that their identities and other demographic information would remain confidential. Analyses below include data from all participants.

Experimental procedure.

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

After receiving these instructions, participants (hereafter termed “actors”) were presented with the answers that a “previous, randomly selected respondent” (“partner”) purportedly provided. Actors learned that the partner’s responses were presented merely as an example and may or may not be helpful towards their own performance in the task. More specifically, for each of the two “sample” trials, actors viewed the full-body photograph that the partner had seen, followed by the partner’s purported: (a) weight estimate (in lbs); (b) self-estimated placement, in the form of a numeric value between 1st and 99th percentile to capture her self-perceived performance rank for that guess, relative to all other participants in the study; (c) actual placement (also in percentile); and (d) correct answer (the target’s actual weight). In actuality, however, the responses of the partner were experimentally created and pre-determined. In the two partner conditions, the partner always guessed weights for the same two target photos in the sample trials, and always gave weight estimates of 139 lbs and 195 lbs, which placed her actual performance rank in the 24^{th} and 26^{th} percentile, respectively. Critically, despite the partner’s substantially below-average performance, her estimated placement differed across conditions. The correct answer (i.e., the target’s actual weight) on these two respective trials is 134 lbs and 118 lbs.

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

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

Dependent measure: Overplacement.

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

Results and Discussion

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

The raw mean overplacement levels for each partner condition appear in Figure 2. We found that overplacing partners significantly increased actors' overplacement compared to calibrated partners or no partner (Table 1). Actor overplacement was 25.95 percentile points higher on average if the partner overplaced (M = 15.12; SD = 2.67) than if the partner was calibrated {M = -10.84; SD = 2.59; $t(422) = 6.87$, $p < .001$, d = .85, CI of mean difference = [18.64, 33.27]}, and 17.12 percentile points higher than if there was no partner {M = -2.00; SD = 2.41; $t(422) = 4.76$, $p < .001$, d = .58, CI of mean difference = [10.05, 24.17]}. These effects are consistent and large across all additional specifications that include controls. Note the control condition revealed that, without any potential for influence from partners, actors' self-estimated placement on this task was well calibrated. Descriptively, their weak negative score was not distinguishable from zero, the point of perfect calibration [$t(158) = -.85$, p = .399, d = -.07]; however, actors who were exposed to an overplacing partner exhibited self-estimated placement that was strongly positively biased. Thus, as predicted, observing overplacement led to greater overplacement.

Figure 2. Actor overplacement by partner condition (Study 2).

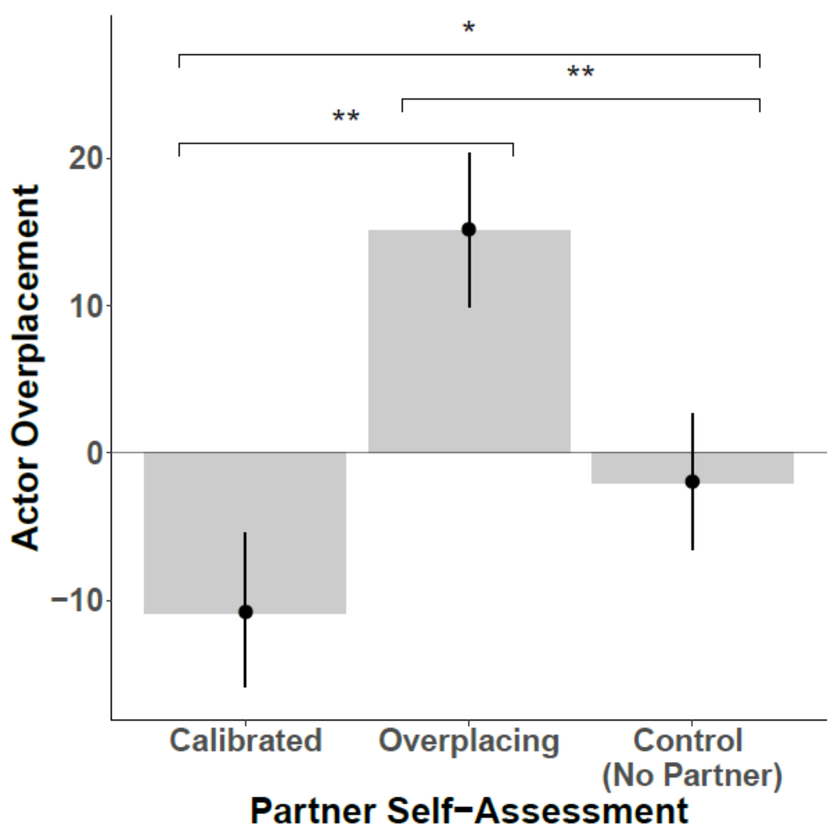

Figure 2. Raw overplacement in percentiles (and 95% confidence intervals) expressed by participants directly exposed to different partner self-assessment conditions (calibrated, overplacing, or no partner control). Positive percentile values index overplacement, 0 indexes perfect calibration, and negative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

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

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

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

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

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

Study 3: Overplacement Spreads to Indirect Ties

Our studies thus far have focused on the transmission of overplacement between directly connected individuals. However, many relationships between group members, especially within larger groups, are indirect (Christakis & Fowler, 2009; Granovetter, 1977). For example, suppose that Agnes and Paul work in the same organization but have never worked nor interacted with each other directly. Both of them, however, work closely with Peter. Is it possible that Agnes' overconfidence may influence Peter, who in turn influences Paul, even though Paul has never met Agnes? Such effects, which have been the focus of an extensive empirical literature on social influence (Bond et al., 2012; Christakis & Fowler, 2008; Fowler & Christakis, 2010; Gruenfeld et al., 2000), would suggest that social transmission may play an important role in the emergence of group and cultural differences in overconfidence on a broader scale (Mesoudi & Whiten, 2008; Whiten & Mesoudi, 2008).

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

Method

Participants.

Through a campus-wide solicitation at a large public university in Canada, we recruited 255 participants (59.29% women; 3 participants did not disclose gender) for an in-person computerized study on judgment and decision-making. As in Study 2, we initially targeted 130 participants in each of two conditions (targeted N = 260 combined), as guided by a power analysis in which we assumed a typical effect size of d = .35, using an alpha level of .05 and power of .80. Data collection was terminated immediately after this target sample size was reached. However, data from 5 participants were not recorded due to experimenter error, leaving a final sample of 255 participants. Participants' ages ranged from 17 to 50 (*M* = 21.37, *SD* = 4.56). As in Study 2, participants received a candy bar for participating and were entered into a raffle to win $30 based on performance and calibration. Data from the 255 individuals who completed the study were included in our analyses below.

Materials and procedure.

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

Key variables.

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

Results and Discussion

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

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

Figure 3. Actor overplacement plotted against partner overplacement (Study 3).

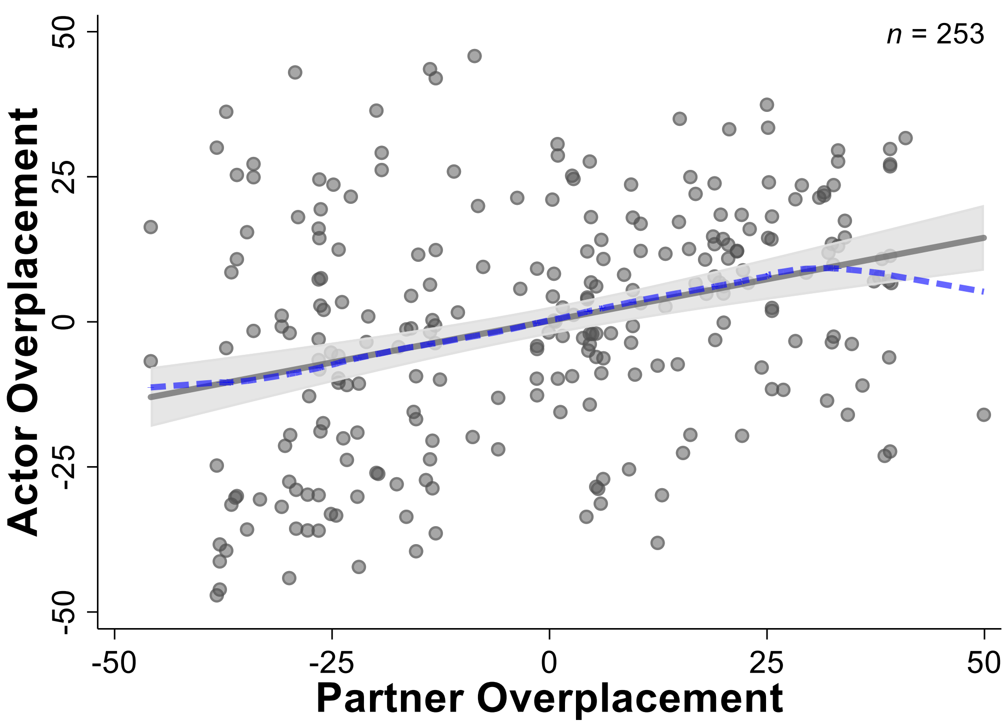

Figure 3. Raw scatter plot showing a positive relation between partner overplacement and actor overplacement in Study 3. Both variables were computed using the residual score approach and reflect variability in self-estimated placement that cannot be linearly predicted from actual placement. Also shown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

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

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

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

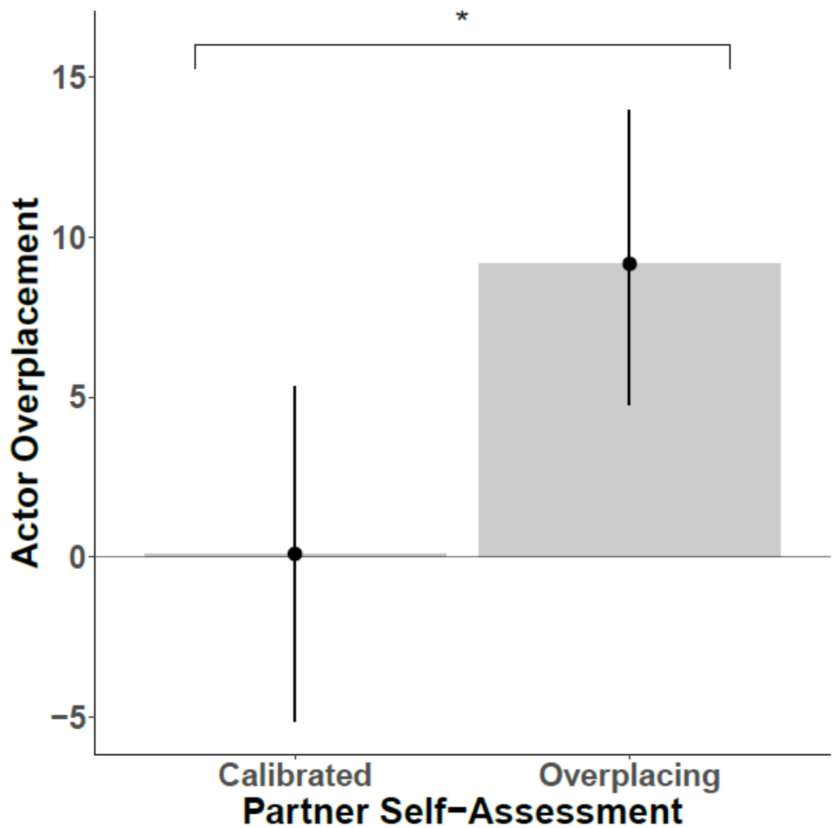

Figure 4. Raw overplacement in percentiles (and 95% confidence intervals) expressed by participants *indirectly* exposed to different partner self-assessment conditions (calibrated or overplacing). Positive percentile values index overplacement, 0 indexes perfect calibration, and negative values index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

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

	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]	[1.84,15.68]	[1.43,15.25]	[0.94,14.94]
	(0.0125)	(0.0133)	(0.0182)	(0.0264)
Gender (1 = Male)		3.24	2.74	3.22
		[-3.82,10.30]	[-4.31,9.80]	[-3.86,10.31]
		(0.3672)	(0.4442)	(0.3715)
Ethnicity (0 = Caucasian; 1 = Non-Caucasian)			7.08^{+}	5.92
			[-0.87,15.03]	[-2.33,14.17]
			(0.0805)	(0.1587)
Age (centered)				-0.45
				[-1.25,0.34]
				(0.2628)
Constant	0.09	-1.16	-6.06	-5.10
	[-4.63,4.81]	[-6.61,4.29]	[-13.78,1.67]	[-13.08,2.89]
	(0.9705)	(0.6759)	(0.1238)	(0.2097)
R^2	0.025	0.028	0.040	0.046
Adjusted R^2	0.021	0.020	0.028	0.031
AIC	2403.5398	2404.7154	2403.6054	2395.0552
BIC	2410.6066	2415.3155	2417.7390	2412.7023
Observations	253	253	253	252

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

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

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

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

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

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

Method

Participants.

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

Materials and procedure.

Participants (hereafter termed "actors") read initial instructions about the weight-guessing task, which consisted of 15 trials. Actors began by completing five practice trials (Trials 1-5), which were designed to both familiarize them with the task and index their baseline overplacement before our experimental manipulation of the “partner’s” information. In each of these practice trials, actors viewed a full-body photograph of a target individual, provided a weight estimate, and indicated their self-estimated placement (percentile rank), using the same prompts as in Studies 2 and 3 (see Supplemental Materials for other minor methodological divergence from Studies 1-3).

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

Note that this partner information was presented only in the first five test trials (Trials 6-10). In these test trials where participants were exposed to partner information, actors first responded to the photo shown—by providing a weight estimate and self-estimated placement—and then immediately viewed the responses that their “partner” had purportedly given for the same photo. In actuality, however, as in Study 2, all partner responses were experimentally created and pre-determined to vary across the four experimental conditions. Because actors always provided their weight and self-placement estimate for each photo before (rather than after) receiving the partner’s input for the same photo, this means that the partner’s self-placement could only affect actor overplacement on new trials that the partner had not yet completed.

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

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

Analytic Plan

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

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

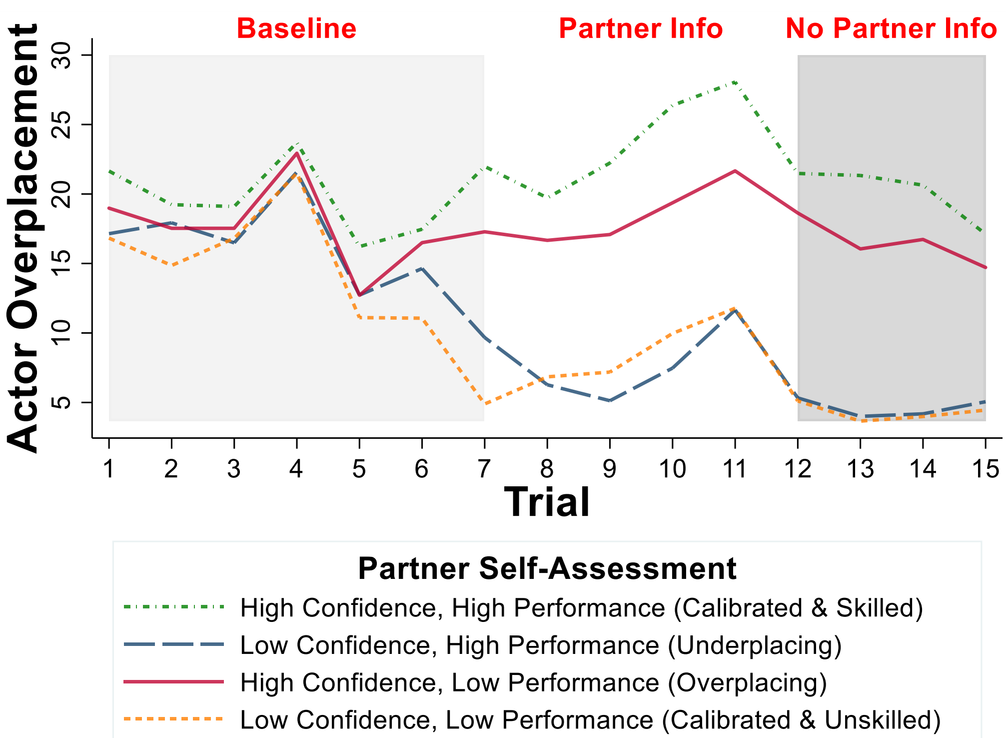

Figure 5. Raw trial-by-trial mean overplacement shown by participants exposed to different partner self-assessment conditions. In the baseline phase (Trials 1-6), before exposure to partner, actors' overplacement (in percentiles) did not differ across conditions. Immediately after viewing

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

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

Results and Discussion

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

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[7] 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.

[8] 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).

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

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

	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 × 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*	5.59*	5.55*	
			[0.91,10.27]	[0.90,10.28]	[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)
R^2	0.008	0.130	0.150	0.150	0.150	0.090
Adjusted *R^2*	-0.005	0.120	0.136	0.132	0.129	0.078
AIC	2146.4986	2144.1783	2140.5920	2142.5758	2144.5711	2259.9460
BIC	2160.5523	2158.2320	2158.1592	2163.6564	2169.1651	2273.9997
Observations	248	248	248	248	248	248

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

Further, as revealed in Figure 6 and estimated in the baseline model, actors' overplacement was strongest and roughly 13.36 percentile points higher if they were exposed to a partner with high self-placement (*M* = 20.00; *SD* = 16.73), than when exposed to a partner with low self-placement (*M* = 6.48; *SD* = 19.48; *t*(244) = 5.79, *p* < .001, *d* = .75, CI of mean difference = [8.82, 17.91]). Moreover, overplacement was just as high if the partner overplaced (that is, had low actual placement; *M* = 17.58; *SD* = 16.76) as if the partner was justifiably high self-placing (that is, had high actual placement; *M* = 22.10; *SD* = 16.53); these two conditions did not differ significantly (*t*(244) = 1.42, *p* = .158, *d* = .27, CI of mean difference = [-1.77, 10.82]). Furthermore, providing a direct replication of our prior studies, when the partner's performance was low, actors showed substantially greater overplacement if said partner's confidence was high (that is, an overplacing partner) compared to if it was low (that is, a calibrated but unskilled partner; *M* = 6.43; *SD* = 17.37; *t*(244) = 3.43, *p* = .001, *d* = .65, CI of mean difference = [4.74, 17.55]). The same pattern of results is obtained for the post-partner phase, suggesting that these effects persist when exposure to partner ceases (see Supplemental Materials).

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

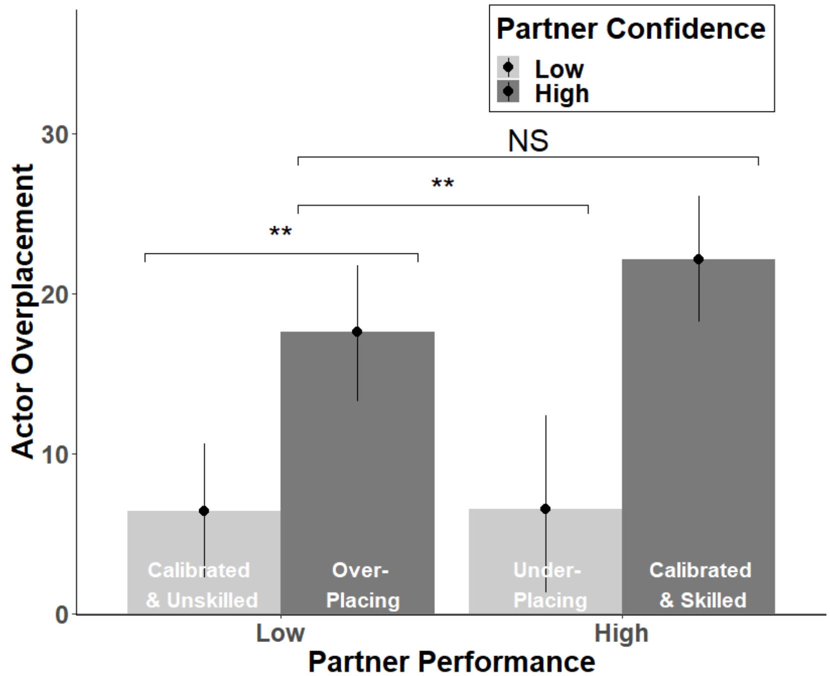

Figure 6. Raw overplacement in percentiles (and 95% confidence intervals) expressed by participants exposed to different partners who vary in self-placement (confidence) and actual placement (performance) in the test phase (i.e., the mean across all trials following initial exposure to partner, corresponding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

We note three other relevant sets of findings, all of which are detailed more thoroughly in the Supplemental Materials. First, our manipulation check confirms that the current results emerged despite participants' awareness that the overplacing partner's beliefs was unrealistic (and thus overplacing). That is, actors were influenced by their partner's confidence despite being fully aware that their partner's confidence was unwarranted, as evidenced by the lower ratings of task competence assigned to these overplacing partners. Second, we found that the transmission effect persisted even after the exposure to partner ceased, such that actors' self-estimated placement in the overplacement condition remained skewed in the post-partner-information phase. These regression results (reported in Table 3 above), which are also visible in the trends illustrated in Figure 6, indicate that these effects only showed a slight diminution in the later trials when the partner's presence was removed. The social influence of overplacing others demonstrated persistence. Third, despite the clear effect that witnessing overplacement in others had on participants' own overplacement, participants subjectively perceived overplacing partners as the *least* influential over their own behavior, highlighting that they were explicitly unaware of (or at least unable to report) their partner's extensive social influence over them.

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

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

In Study 5, we further investigate the persistence and power of overconfidence transmission in two ways. First, we test longitudinally whether the effect of being exposed to confidence endures after several days. Second, we test whether the transmission effect also "carries over" to influence self-assessments in different task domains. If so, this study would provide important initial evidence that the effects of overconfidence transmission are not shortlived and can continue to affect a person's self-assessments over time, and that the effects are not limited to the domain in which overplacement is "caught"—but instead can bleed into other domains.

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

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

Method

Participants.

We recruited 405 participants from the Amazon Mechanical Turk online labor market (54.8% women, .2% other) whose ages ranged from 19 to 78 (*M* = 36.22, *SD* = 1.46). Participants received $0.30 for completing the initial survey (at Time 1) and were entered into a raffle to win a $25 bonus payment based on both performance and calibration.

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

Materials and procedure.

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

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

[9] 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_{age}= 38.50, SD = .88) compared to the latter group [M_{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.

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

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

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

[10] 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.

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

Results and Discussion

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

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***	20.75***	12.61***	12.61***	13.45**	13.99***	11.77**	11.22**	9.09*	9.97*
	[15.28,26.59]	[15.20,26.30]	[5.59,19.63]	[5.66,19.55]	[5.20,21.71]	[5.93,22.04]	[4.05,19.48]	[3.51,18.94]	[1.48,16.71]	[2.39,17.55]
	(0.0000)	(0.0000)	(0.0005)	(0.0004)	(0.0015)	(0.0008)	(0.0030)	(0.0046)	(0.0195)	(0.0102)
Gender (1 = Male)		7.38**		4.25		-0.89		9.39*		6.34
		[1.78,12.98]		[-2.88,11.37]		[-9.16,7.37]		[1.48,17.31]		[-1.44,14.12]
		(0.0099)		(0.2409)		(0.8312)		(0.0203)		(0.1098)
Age (centered)		-0.37**		-0.44**		-0.65***		-0.24		0.30^{+}
		[-0.60,-0.14]		[-0.72,-0.17]		[-0.97,-0.33]		[-0.55,0.07]		[-0.01,0.60]
		(0.0017)		(0.0019)		(0.0001)		(0.1287)		(0.0540)
Memory of Task (centered)				0.87		0.82		0.92		2.76^{+}
				[-1.80,3.53]		[-2.27,3.91]		[-2.04,3.88]		[-0.15,5.67]
				(0.5206)		(0.6019)		(0.5409)		(0.0630)
Constant	-4.72*	-7.86***	1.97	1.33	1.41	3.27	2.54	-0.60	-15.29***	-18.86***
	[-8.70,-0.74]	[-12.46,-3.25]	[-2.89,6.84]	[-4.28,6.95]	[-4.31,7.12]	[-3.25,9.78]	[-2.80,7.89]	[-6.84,5.64]	[-20.56,-10.01]	[-24.99,-12.73]
	(0.0203)	(0.0009)	(0.4246)	(0.6397)	(0.6280)	(0.3236)	(0.3497)	(0.8498)	(0.0000)	(0.0000)
R^2	0.116	0.158	0.060	0.118	0.050	0.126	0.044	0.085	0.027	0.070
Adjusted R^2	0.114	0.151	0.055	0.100	0.045	0.108	0.039	0.066	0.022	0.050
AIC	3866.9293	3842.2777	1859.6341	1844.1468	1924.2342	1903.3819	1897.3359	1886.1401	1892.0275	1879.1026
BIC	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 partner was calibrated {$M = 1.97$; $SD = 26.03$; $t(198) = 3.54$, $p < .001$, $d = .50$, CI of mean difference = [5.59, 19.63]}. The same conclusions are reached in subsequent analyses using multi-level models to examine within-person trajectories, as well as when we examined only the novel weight-guessing trials (as opposed to combining both novel and repeated trials; see Supplemental Materials). Together, these results suggest that the transmission effect persisted over several days. In fact, actor overplacement in the overplacing-partner condition did not show a significant decline from Time 1 to Time 2 within-person ($z = -0.64$, $p = .522$, CI of mean difference = [-6.36, 3.23]).

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

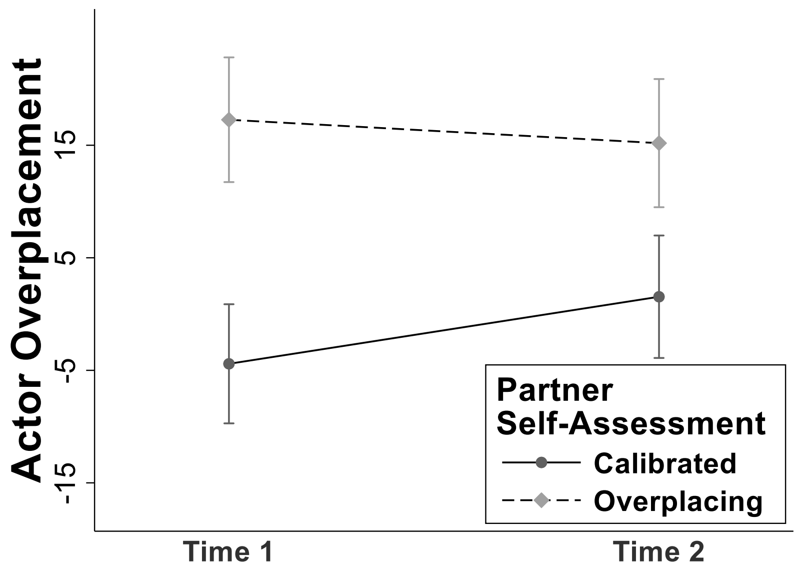

Figure 7. Model estimated overplacement in percentiles (and 95% confidence intervals) of participants on two identical trials of a weight-guessing task at two time points (separated by several days). Participants were randomly assigned (between-subjects) to view a partner who was either calibrated or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

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

Summary. Taken together, Study 5 extends our understanding of the reach of overconfidence transmission. Exposure to confidence in the form of high self-placement produces effects that are temporally persistent and resistant to erosion. Said exposure not only influenced confidence in the original domain in which others' confidence was observed, but even in a new task domain and environment. Moreover, as was found in Study 4, participants were unaware of the influence of overplacing peers on their own self-assessments (see Supplemental Materials). Overall, by documenting the longevity, persistence, and domain-generality of the transmission of overplacement, the current results begin to offer insights into the extensive scale at which overconfidence may spread.

Study 6: The Transmission of Overplacement and Coalitional Membership

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

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

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

Method

Participants.

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

Experimental Procedure.

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

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

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

[11] 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.

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

Results and Discussion

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

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

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

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

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

To probe this significant interaction that emerged, we next examined simple effects separately for each partner group membership condition (the moderator). Our key finding, based on the baseline model (with no controls), is depicted in Figure 8. In the in-group partner condition, actor overplacement was significantly higher if the partner overplaced (*M* = 13.89; *SD* = 26.03) than if the partner was calibrated {*M* = -5.65; *SD* = 29.35; *t*(123) = 3.87, *p* < .001, CI of mean difference = [6.13, 32.95]}, indicating the spread of overplacement between in-group members.[12] This result mirrors our findings in Studies 2-5. By contrast, in the out-group partner condition, actor overplacement did not significantly differ as a function of exposure to an overplacing partner (*M* = 2.05; *SD* = 26.84) or a calibrated partner {*M* = -1.40; *SD* = 30.36; *t*(121) = .68, *p* = .496; CI of mean difference = [-9.99, 16.89]}. Finally, these simple effects produce the same basic findings across our other specifications with controls—all of which indicate a significant effect of partner self-assessment on actor overplacement only in the in-group partner condition, but null effects in the out-group partner condition, consistent with a selective learning bias.

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

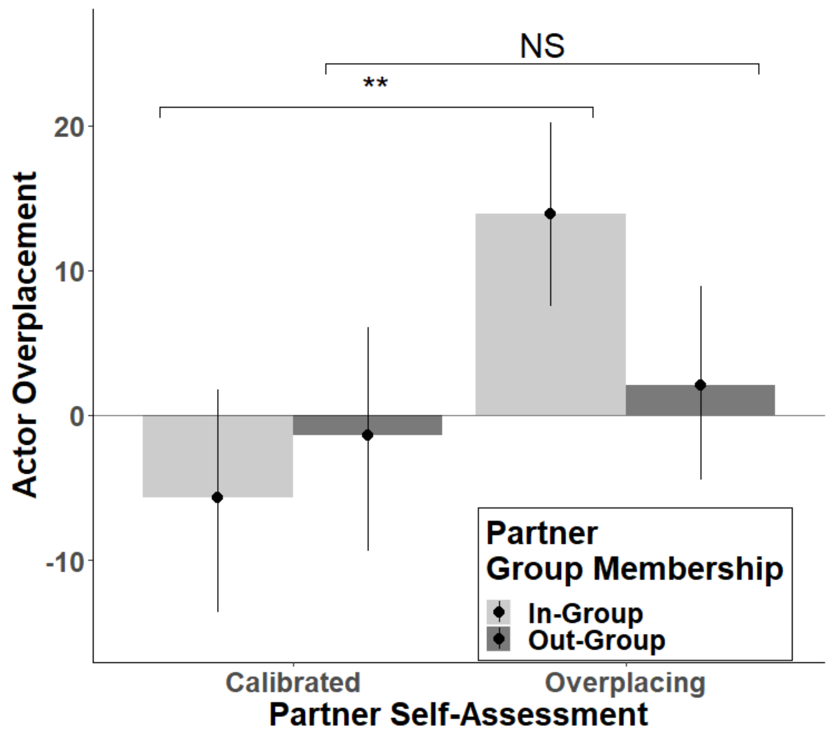

Figure 8. Raw overplacement in percentiles (and 95% confidence intervals) expressed by participants exposed to a partner who varied in self-assessment (overplacing vs. calibrated) and

[12] 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.

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

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

These results also shed new light on how differences in overconfidence across groups can emerge (Chudek et al., 2012; Henrich, 2016; Henrich & Broesch, 2011). Selectively acquiring the overconfidence from one's own social group means that, when operating across occasions and individuals, the kinds of in-group biased imitative processes demonstrated here can potentially generate substantial variation between groups, while maintaining relative homogeneity among entities within these local contexts. As a result, these micro-level transmission processes operating within interactions among individual entities could aggregate to generate population-level patterns of cultural variation, accelerating the emergence and stabilizing of group-level differences in overconfidence to explain how groups, organizations, and states come to differ in this trait.[13]

General Discussion

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

[13] 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.

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

Here, results from six studies, using both correlational and experimental designs, provide support for the overconfidence transmission hypothesis. These studies utilize methodologies that elicit overplacement in a manner that addresses important methodological concerns raised in prior work, including deploying financial incentive to increase motivation for accurate self-assessments (and decrease self-presentation motivation; Camerer & Hogarth, 1999; Hoelzl & Rustichini, 2005) and disentangling warranted confidence from unwarranted confidence by measuring actual performance (Moore & Healy, 2008). Study 1 revealed that, under controlled laboratory conditions, face-to-face collaboration led individuals randomly assigned to work in a dyad to converge in overplacement, such that a positive correlation between dyad members' overplacement emerged following (but not before) the interaction. Moreover, consistent with the proposed social transmission process, one partner's pre-interaction overplacement predicts the change in the other person's overplacement from pre- to post-interaction. In subsequent studies (Studies 2-6) we build on this initial evidence to more firmly establish the causal influence of overplacing peers on observers. Overconfidence was found to spread as a direct result of individuals' tendency to align with the confidence tendencies observed in peers, even when they are unwarranted and represent overplacement. Combined, our major finding across all six studies suggests that, by operating on our existing proclivities for social learning, locally relevant confidence traditions, even when cued by overconfident models, are readily acquired and act to increase our propensity towards overplacement.

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

1. *Indirect transmission*: overplacement spreads not only from one person to another, but also across indirect ties from person to person to person. Third-parties' propensity towards overplacement is heightened by an overconfident model to whom they are only *in*directly connected through another peer (Study 3), highlighting the extensive reach of confident peers.

2. *Temporal stability*: the transmission effect may be temporally stable to a certain degree. In our studies, overplacing peers continued to induce biased beliefs in the later stages of the experiment when exposure to peer ceased (Study 4), as well as, quite remarkably, several days following this initial exposure (Study 5).

3. *Outside of conscious awareness*: the influence of overplacing peers on self-estimated placements appears to operate "stealthily", occurring largely outside of conscious awareness. Individuals failed to detect the substantial influence of overplacing peers (Studies 4-5). Efforts to resist acquiring bias from overconfident peers, and reduce bias more generally, may be especially challenging in the absence of personal awareness and self-knowledge (Cassam, 2017).

4. *Cross-domain generality*: the transmission effect may operate across domains. Observing peers express unwarranted confidence in weight-guessing carries over and produces greater overplacement in word tasks (Study 5). Note that while these results are necessarily tentative due to the relative brief time-span and limited domains examined here, and should be further examined in future studies, the current data nonetheless open up important new avenues for future research by highlighting the possible temporal persistence and cross-domain generality of overconfidence transmission.

5. *In-group biased transmission*: the general effect of overconfidence transmission is qualified by an important factor: in-group selective social learning. That is, individuals do not copy indiscriminately. Instead, they are sensitive to *whose* mental representations are on display and selectively acquire the overplacement of in-group but not out-group members, consistent with the long emphasis on the acquisition of self-relevant and adaptive information in theories of cultural learning.

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

Theoretical Implications

Social transmission and clustering of overconfidence within groups. This research began by seeking to address a puzzling question: Why does the degree of confidence often cluster between individuals who belong to the same community, to the point of producing what appears to be group- or even culture-wide traditions of overconfidence? Our findings suggest that cultural transmission may be one mechanism that partially explains how group-level differences in overconfidence are maintained (Boyd & Richerson, 1985; Cohen, 2001; Cohen et al., 1996; Morgan et al., 2011; Nisbett & Cohen, 1996). Theorists have proposed that cultural learning is "the primary engine that produces the bulk of stable variation across groups" (Heine & Norenzayan, 2006, p. 260; also see Richerson & Boyd, 2005), explaining why genetically similar individuals living in similar environments, but in different social groups, may possess strikingly different beliefs, practices, and psychological tendencies. Empirically, there is a swelling tide of supportive evidence from across the social sciences confirming that many of these patterns of cross-group variation stem from social transmission (Boyd et al., 2011; Boyd & Richerson, 1985; Henrich & Gil-White, 2001; Mesoudi, 2011; Mesoudi et al., 2004; Nisbett et al., 2001; Rendell et al., 2010, 2011; Richerson & Boyd, 2005). Applying this approach to the case of overconfidence, it stands to reason that, similar to these culturally varying behaviors and psychological tendencies, the observed variation in overconfidence across human populations may be rooted in social transmission that occurs among regularly interacting social entities. Of course, these studies focused solely on overplacement. An important direction for future work is to test whether the transmission account proposed here extends to other separable forms of overconfidence, including overestimation and overprecision (Moore & Healy, 2008; Muthukrishna et al., 2018).

The origins of overconfidence. A second contribution of this research involves adding to the growing theoretical and empirical interest across psychology, economics, evolutionary biology, organizational behavior, and other disciplines in understanding how individual differences in overconfidence arise—that is, the proximate explanations for why some individuals are more overconfident than others (C. Anderson et al., 2012; Johnson et al., 2006, 2011; Johnson & Fowler, 2011; Marshall et al., 2013; Murphy et al., 2015, 2017; Van den Steen, 2004; von Hippel & Trivers, 2011). Traditional answers to this question generally invoke biological and personality trait-like factors to explain inter-individual differences in the degree (and direction) of bias towards overconfidence. For example, this work reveals that the magnitude of inflated beliefs is higher in men compared to women, and intensifies with increased testosterone and psychological traits that propel pride and hubris, such as narcissism, sense of power, and perception of control (e.g., Fast, Sivanathan, Mayer, & Galinsky, 2012; Gneezy, Niederle, & Rustichini, 2003; Pallier et al., 2002; Paulhus et al., 2003; Tracy & Robins, 2007).

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

Alternative Explanations of How Different ‘Overconfidence Traditions’ Arise

There are other reasons we do not examine here that can also explain why overconfidence proclivities converge within-groups and diverge across groups. After all, there is little doubt that a complex set of mechanisms likely underlies this human cognitive diversity. One especially prominent and compelling theoretical explanation for cultural variation emphasizes “evoked culture" and habitat-specific responses, which consider how behavioral and cognitive variation arise as adaptive, evoked responses to differences in immediate environmental conditions (Gangestad et al., 2006; Hill & Hurtado, 1996; Tooby & Cosmides, 1992). This logic, when applied to overconfidence, proposes that variation in levels of false assessments is a response to different ecological circumstances, with greater bias observed in environments that confer greater rewards for confidence displays and competitive behavior incited by overconfidence (Heine, 2011; Johnson & Fowler, 2011; Leibbrandt et al., 2013; Radzevick & Moore, 2011). From this view, the pervasiveness of overconfidence observed in Wall Street investors stems directly from the enormous financial and prestige incentives that reward overconfidence (and that outweigh the occasional costs from risky investments and mistakes (Haselton et al., 2015; Johnson et al., 2013; Johnson & Fowler, 2011; Sharot, 2011, 2012). Thus, the strength of the overconfidence bias represents different cultural adaptations that arise from different ecological and economic niches (Diamond, 1997; Triandis, 1994).

Importantly, however, as we mentioned above, these two logically theoretical explanations—cultural evocation and transmission—are not mutually exclusive. Recognizing that overconfidence may arise from social transmission does not imply that it is irresponsive to local benefits (and costs). To the contrary, these two processes likely interact to maintain and reinforce intragroup similarities and intergroup differences in overconfidence (Mesoudi et al., 2006). Some individuals in a group or population may calibrate their overconfidence to the local optimal strategy, then these variants spread within a group and lead individuals to converge on a common degree of overconfidence. For example, in the United States, the most individualistic society in the world (Oyserman et al., 2002), unusually high levels of overconfidence may be triggered by cues of relatively large net payoffs associated with outcomes of competition and conflict (cues such as cultural values that emphasize success, freedom, and self-sufficiency), which then spread (and perhaps even become amplified) as individuals copy the expressed confidence and inflated beliefs observed in social interactions, perhaps especially from prestigious models who express a great deal of confidence. The point is that, insofar as cultural evocation alone is unlikely sufficient for explaining all forms of intergroup variation in overconfidence, a complete understanding of these patterns requires considering the social transmission of the propensity towards inflated assessments.

Limitations and Future Directions

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

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

A second area ripe for future studies concerns tackling the thorny yet crucial question: What specific mechanism(s) mediate this pattern of overconfidence transmission? While one limitation in these studies—as in much of other work demonstrating transmission effects—is that we are unable to empirically isolate the precise mechanisms involved, transmission in the real-world likely emerges via a diverse set of mechanisms such as imitation, peer pressure, or other psychosocial processes. We speculate that one particularly important avenue to explore is whether and how overconfidence transmission may arise from the spread of social norms, particularly as they interact with cultural learning biases such as prestige- or confidence-bias (i.e., the tendency to preferentially learn from highly respected members of the community, or those who express cues of confidence; Birch, Akmal, & Frampton, 2010; Henrich & Gil-White, 2001; Jiménez & Mesoudi, 2019; Rendell et al., 2011), including highly confident individuals (C. Anderson et al., 2012; Kennedy et al., 2013; Tenney et al., 2019). Prestige-bias may first allow overconfident individuals to introduce a new behavioral standard to the community, such as the norm to appear self-assured and confident. Once this practice takes hold, conformist tendencies may subsequently take over and allow this behavioral norm to spread even more widely to generate group-wide adoption and display of overconfidence. Consistent with this, existing work shows that these normative pressures have robust effects in homogenizing within-group behavior and generating between-group variation (Henrich & Boyd, 1998), suggesting that they may indeed be crucial mechanisms that undergird how cultural climates of overconfidence emerge and are maintained between groups.

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

A third opportunity for future investigation involves testing whether *under*confidence can also spread socially. Although our primary focus here is on overconfidence, the same reasoning predicts that exposure to underconfident others may increase an observer's propensity towards underconfidence. In fact, some supporting findings emerged from two of our studies that directly examined the effect of underconfident others. In Study 3, the positive association that emerged between model and observer overplacement indicates that, interpolating this trend, observing underplacing others increases one's bias towards underplacement as well. In Study 4, peers who expressed low confidence (even when underplacing) reduced observer confidence (though they still remained slightly overplacing on average). Thus, these results, combined with our other studies that reveal the confidence-reducing effect of peers who express low confidence (but are accurate and unbiased), are generally consistent with the corollary prediction that underconfidence is also socially transmissible. However, given the more limited evidence, the case of underconfidence transmission must remain tentative and future work is needed. Note, however, that this line of inquiry is important because—despite the aforementioned prevalence of overconfidence and its many perilous consequences (factors that led to our focus on overconfidence here)—underconfidence also brings with it costly mistakes. Individuals with a baseline negative bias who, by virtue of underestimating their chances of success, are prone to reduced aspirations, morale, and persistence, and a general avoidance of competitive and risky ventures that they, in actuality, stand a good chance to gain (Haselton et al., 2015; Johnson & Fowler, 2011; Murphy et al., 2017; Nettle, 2004; Niederle & Vesterlund, 2007; Sharot, 2012), undermining success in a broad range of domains ranging from mate attraction, social popularity, and mental health to education and career choices. Thus, even if it turns out that the costs and benefits of over- and underconfidence are not symmetrical (Nettle, 2004), establishing whether and how both of these errors transmit is required for a full understanding of the conditions that lead individuals to stray from accurate and truthful beliefs and associated rational assessment and decision-making.

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

Context of the Research

This work represents an extension of our team's ongoing research into the origins and consequences of accurate and inflated self-beliefs. For instance, our research team has explored how overconfidence may be rooted in individual-level factors such as the motivation to improve one's social standing, for example by pursuing prestige (C. Anderson et al., 2012) and honing one's skills (Tenney et al., 2015), as well as contextual factors such as the nature and difficulty of the task (Logg et al., 2018; Moore & Cain, 2007), the liability and falsifiability of confidence claims (Tenney et al., 2019), situational power and authority (Brion & Anderson, 2013), and why supplying arguments in verbal disagreements often fails to persuade (Logg et al., in prep). Despite these efforts, however, we increasingly recognize that cultural influences represent an important but neglected part of this puzzle on the origins of biased (and accurate) beliefs. As we note above, this lack of existing work is striking despite much empirical and anecdotal evidence documenting extensive cultural variation in the expression of confidence—with some groups typified by self-assurance and others by diffidence. This work is therefore motivated by our interest in bridging this gap by assessing how, on a micro-level, inter-individual differences in overconfidence may stem, in part, from social influence. Future work should investigate the precise mechanisms that explain why confidence transmits socially, how overconfidence spreads in large social networks beyond dyads, and how the transmission of overconfidence affects collective successes and failures.

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