Power poses – where do we stand?

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INTRODUCTION

CRSP special issue on power poses: what was the point and what did we learn?

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The possibility that holding an expansive nonverbal display for two minutes could affect a person's behavioral, psychological, and physiological states was a provocative idea when first proposed (Carney, Cuddy, & Yap, 2010). Specifically, the notion that a static nonverbal expression could affect a person's endocrine profile – namely their cortisol and testosterone levels – was so provocative it was almost preposterous. However, the field of social psychology took notice. Additional claims were then made about how such poses might positively impact a person's life, particularly for people "with no resources and no technology and no status and no power" (Cuddy, 2012).

This exciting proposition ignited a wave of popular interest, evidenced in one way by the enormous popularity of a TED talk about the idea that has, at the end of 2016, already been viewed 38 million times. Beyond this specific finding, Carney et al. (2010) hoped to offer an important theoretical contribution to theories of mind–body interaction such as the James–Lange theory of emotion (James, 1884; Lange, 1912) and Jamesian notions of ideomotor action (for a review, see Laird & Lacasse, 2014). This work hoped to offer support for a bidirectional link between a nonverbal display of a powerful-looking posture and the mental and physiological states that were indicative of possessing power.

This idea was, to put it mildly, subject to the hard glare of scientific inquiry not long after its debut. Approximately 5 years after the original paper was published, a conspicuous failure to replicate (Ranehill et al., 2015) caught the attention of many who were already skeptical – including Carney herself. A response (Carney, Cuddy, & Yap, 2015) to Ranehill et al. dutifully listed the many differences between the original 2010 paper and the 2015 failure to replicate that might have served as possible moderators. Further variations on the original power pose work were published, many by the original research team members – albeit based on data collected before the Ranehill publication – including work demonstrating that power posing could positively impact job interview performance (Cuddy, Wilmuth, Yap, & Carney, 2015).

At the same time, among some circles the idea became a shorthand for flashy social psychological work that could not be replicated (see, e.g., Andrew Gelman's blog posts on the topic and additional failures to replicate such as Garrison, Tang, & Schmeichel,

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2016). Further, it became increasingly clear to some authors of “power pose” work that effects were fragile at best, and likely not replicable as researchers across the world wanted to discuss minor differences between the original study and theirs – because their work did not replicate the original (Carney, 2016). Much of this work was never published. Perhaps the authors of the failed replications did not wish to throw fuel on the fire, but the fire was already burning when a p-curve analysis of the reply to Ranehill written by Carney et al. (2015) suggested strongly that even though some papers showed effects, overall the body of work had no evidentiary value (Simmons & Simonsohn, 2015).

In this context, one purpose of this Special Issue by CRSP was to gather additional data – but this time transparent and preregistered – about the effects of power poses, including direct replications and tests of possible moderators. Our aim in this regard was to demonstrate the possibility of a different path than the unproductive cycle that social psychology has come to know all too well in recent years: a provocative finding with a small sample size, a failure to replicate, a rebuttal outlining all the possible reasons (some would say excuses) for the failure, researchers entrenching in their views and refusing to update based on new data, and so on. Part of our aim, then, was to show how researchers could coordinate and cooperate in an effective and efficient manner.

We admit that we were expecting this special issue to be a “final word” on the topic, providing a consistent and definitive answer on both the replication and novel effects. As the results rolled in, we initially believed that we were on exactly this path, as the preregistered predictions failed to show strong evidence of effects. We further expected that the issue-wide analysis, overseen by E.J. Wagenmakers, would confirm these expectations. However, a surprise awaited us as the novel Bayesian meta-analysis conducted by Gronau (Gronau et al., this issue) in fact showed a reliable non-zero effect on felt power, despite the majority of individual studies failing to reject the null hypothesis. (Unfortunately, the measure of felt power was the only measure included in enough papers to provide such a test, meaning that on the behavioral indicators we are left with the analyses from each individual study only.)

While this special issue did not provide the kind of definitive final word which we hoped for, we believe something much more interesting has emerged. This coordinated research effort has now outlined a clear program of research for anyone who wishes to take power poses seriously. Given the failures to find effects on actual power-related behaviors, and only a small effect on felt power, then the following questions must be answered for power poses to continue to be recommended: (1) is the self-reported measure of felt power anything beyond an experimental demand characteristic? and (2) are there conditions under which feeling powerful from power poses could lead to beneficial behavioral changes? (It seems unlikely to us that the latter question will be answered in the affirmative because if increases in self-reported power did lead to changes in behavior, such behavioral effects would likely have been observed in at least some of the articles published in this special issue.) One positive report of this special issue, then, is that there may be some value of power poses on self-reported sense of power, but whether this effect is a methodological artifact or is meaningful is an open question and this issue of CRSP has charted out a clear path toward future research for those interested in the topic.

At the same time, there was a more important goal for this Special Issue beyond merely providing additional data on a narrow research question. The broader goal was to demonstrate the benefits of peer-reviewed preregistration for enhancing the quality of research. The benefit of preregistration is clear. (And in other outlets we have described the benefits of peer-reviewed preregistration; see Jonas & Cesario, 2015.) While previous, non-preregistered replication projects often faced discussions about the validity of the obtained results, the approach taken, or the analyses applied, a preregistered and *peer-reviewed* route is fully transparent and also benefits from the input of reviewers. To this end, it is instructive to ask, *how was any individual research proposal meaningfully changed by this process?*

Without exception, the method, design, or analysis of every proposed study was modified in some way following the initial Stage 1 review. True experts in the fields of embodied cognition, hormones, and other relevant areas of expertise provided advice to researchers *before* they spent precious resources conducting these studies. It is certainly the case that the quality of the research was improved by the preregistration process, a point confirmed by the many emails and positive comments we received by the authors during and after the review process. Indeed, the authors' comments reveal something we have long argued: That the peer-review preregistration process changes the dynamic and tone of the review process from destructive and negative to constructive and exciting. Reviewers were not on the lookout for the many ways the researchers failed to do what they should have (often a reflection of reviewers trying to show how smart they are) but instead approached these proposals with the mindset of, "What would I do to make this the best research possible?" Refreshing indeed.

Most important, every researcher received feedback from a true expert in power poses – Dana Carney, first author on the original power pose manuscript (Carney et al., 2010). Carney reviewed all proposals with replication plans (direct or conceptual) and provided detailed feedback, including experimenter scripts, programmed experiments, stimuli, instructions on how to get the participants to hold the poses exactly, and nuanced and highly specific information not obvious from or included in the original publication. Researchers often bemoan learning such "insider information" only after the fact or not at all. The peer-review preregistration process at CRSP solves this problem and, as indicated from the comments of the researchers, is a much-appreciated change.[1]

A final benefit of the preregistration process at CRSP is the ability to coordinate among researchers prior to any researcher beginning data collection. This allowed us to make two contributions with this special issue that would not otherwise have been possible. First, we were able to ensure that multiple researchers had the basic conditions present in their studies – comparing expansive and contractive poses – which allowed us to provide an overall, cross-lab analysis testing replication of the basic power pose effect (Gronau et al., this issue). Moreover, we were able to have all researchers include a measure of whether participants had seen the TED talk on power poses, which allowed for cross-lab testing of a key potential moderator of power pose effects. This analysis, testing whether awareness influences the effect of expansive poses, appears as the final paper in this special issue and supports the possibility that the observed effect of powerful poses on felt power might be understood as a demand characteristic, as the effect was weaker with those participants unfamiliar with the TED talk (Gronau et al., in press).

As a final note, it is important to state that all three of us have, at one point or other, supported the possibility that power poses could have meaningful effects on behavior, psychology, physiology, or cognition (e.g. Carney et al., 2010; Cesario & McDonald, 2013). It is also important to note that the research presented here did not include any tests of whether power poses could impact others, as an expression of nonverbal behavior (for a review, see Hogue & Lord, 2007; and specifically for power poses, Rennung, Blum, & Göritz, 2016). This distinction is important because there is a long history of research in communication studies, anthropology, primatology, and social psychology on the way in which power and status are displayed to others in social context. This vast body of work on *nonverbal displays* is not to be confused with the power pose work and the focus of this special issue which was on *embodied effects of these postures*.

Moreover, it is obvious that the researchers contributing to this special issue framed their research as a productive and generative enterprise, not one designed to destroy or undermine past research. We are compelled to make this point given the tendency for researchers to react to failed replications by maligning the intentions or integrity of those researchers who fail to support past research, as though the desires of the researchers are fully responsible for the outcome of the research. (Curiously, the desires of researchers to *find* effects never play a role in these defensive arguments.) The very costly expense (in terms of time, money, and effort) required to chip away at published effects, needed to attain a "critical mass" of evidence given current publishing and statistical standards, is a highly inefficient use of resources in psychological science. Of course, science is to advance incrementally, but it should do so *efficiently* if possible. One cannot help but wonder whether the field would look different today had peer-reviewed preregistration been widely implemented a decade ago.

Note

1. From Bailey et al., "The peer reviewed preregistration process made the reviewer/author dynamic less adversarial and more collaborative." From Jackson et al., "[Were able to make] amendments before data collection even began, strengthening the quality of our study procedures." From Klaschinski et al., "We greatly appreciated the fact of getting constructive feedback on our design by well-informed experts before data collection started." From Latu et al., "We very much enjoyed the peer-reviewed preregistration process … the openness of the process helped improve our research by allowing us to communicate directly with reviewers, to get materials and advice for designing our study."

Disclosure statement

No potential conflict of interest was reported by the authors.

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Could a woman be superman? Gender and the embodiment of power postures

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ABSTRACT

Physical postures can instill a sense of power in the self as well as communicate power to others. Recent work indicates that a target's gender interferes with the rapid identification of power postures; men in low-power postures and women in high-power postures slow viewers' identification. We hypothesized that how long people enact low and high-power postures will vary as a function of their own gender and the gender of the person modeling the posture. We presented male and female participants with images of male and female models in low and high-power postures and asked them to enact the postures for an unspecific duration. We measured enacted duration, risky behavior, and felt power. The preregistered hypothesis that posture condition and participant gender would interact was not supported. Instead, overall, participants enacted the high-power postures longer than the low-power posture. Supporting the preregistered hypothesis that target gender would interact with posture condition and participant gender, only male participants' time in the postures was sensitive to posture and model gender combinations. Consistent with theories proposing greater rigidity of male gender roles, male participants enacted the low-power postures for the least amount of time when duplicating a female model in a low-power posture. Finally, we did not strongly replicate prior work, but found some support that enacting high-power postures led to riskier behavior and more felt power for some aspects of the sample in exploratory analyses.

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Gender; norms; pose; posture; power

Power organizes social life. Among both human (Fisek & Ofshe, 1970) and nonhuman primates (Hsu, Earley, & Wolf, 2006) perceived power, defined as the ability to control outcomes (Dépret & Fiske, 1993; Fiske, Cuddy, & Glick, 2007), can establish a hierarchy and determine who emerges toward the top. Hierarchal relations diminish the need for repetitive conflicts and unnecessary confrontations, which can be costly for all involved (Halevy, Chou, & Galinsky, 2011). To signal their power, people implement a variety of strategies.

Postural displays can indicate an individual's power. Powerful people enact expansive postures while those low in power enact contractive postures (Buss, 2004; Hall, Coats, &

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LeBeau, 2005). Such postural displays lead to different patterns of neural activation in the ventromedial prefrontal cortex, an area connected to the processing of social cues (Freeman, Rule, Adams, & Ambady, 2009; Marsh, Blair, Jones, Soliman, & Blair, 2009). Further, perceivers rely on postural displays to assess novel others' power and leadership potential (Aguinis & Henle, 2001; Henley, 1995; Maricchiolo, Livi, Bonaiuto, & Gnisci, 2011; McGinley, LeFevre, & McGinley, 1975; Ridgeway, 1987). For instance, Schmid Mast and Hall (2004) found that participants could accurately judge organizational rank between two colleagues using only the colleagues' nonverbal displays as depicted in an image of them interacting. In sum, posture can convey power.

In addition to impacting perceptions of others' power, enacted expansive and contractive postures might also influence how an individual feels and behaves. Carney, Cuddy, and Yap (2010) reported that participants who maintained two high-power postures for a minute each engaged in riskier gambling behavior, reported feeling more powerful, and experienced increased testosterone levels and decreased cortisol levels relative to participants engaging in low-power postures. Consistent with classic embodiment findings (e.g. Wells & Petty, 1980), this work suggests that expansive and contractive postures can make the actor *feel* more powerful by inducing physiological changes, which in turn impact risk-taking behavior (Anderson & Galinsky, 2006).

The effects of embodied power postures are most pronounced when participants are unaware of the purpose of the study and are placed in an explicitly social context (Carney, Cuddy, Yap, 2015; Cesario & McDonald, 2013). The amount of time spent in a posture has also been implicated as a potential moderator. Ranehill and colleagues (2015) used a similar methodology to that of Carney and colleagues (2010) but had participants maintain each posture for 3 min instead of 1 min; they did not replicate the effect of posture on risky behavior or on hormonal modulations. In their recent review, Carney et al. (2015) suggest that one reason Ranehill and colleagues (2015) did not replicate their initial findings could be due to this difference in duration.

Even so, Carney and colleagues (2015) conclude that expansive postures impact power-related feelings, behaviors, and physiological responses. Thus, it may be the case that high-power postures are strategically enacted to convey high power to observers (DePaulo, 1992), while further instilling a sense of power in the actors themselves. However, because of the associations between power and gender found in a separate literature (e.g. Berger, Cohen, & Zelditch, 1972), gender may impact the extent to which power postures' can have these embodied effects.

Several theories suggest a complex relationship between gender and power. For example, expectation states theory construes gender as a diffuse status characteristic, one that confers greater power to men than to women across situations (Berger et al., 1972; Ridgeway, 2001). Similarly, role congruity theory argues that the instrumental traits associated with men overlap with those associated with leaders, giving men greater access to high-power leadership roles than women (Eagly, 1987; Eagly & Karau, 2002; Eagly, Wood, & Diekman, 2000; Koenig, Eagly, Mitchell, & Ristikari, 2011; Rudman, Moss-Racusin, Phelan, & Nauts, 2012). Such gender-power associations are thought to contribute to biases, including the decision to hire a man rather than a woman with identical credentials (Moss-Racusin, Dovidio, Brescoll, Graham, Handelsman, 2012). Thus, gender stereotypes associate men with high power and women with low power.

Given the association of gender and power, it is not surprising that gender may influence how power-related postures are perceived (Aguinis & Henle, 2001; Aguinis, Simonsen, & Pierce, 1998; Carli, LaFleur, & Loeber, 1995). For instance, Bailey, Lambert, LaFrance, and McCarthy (unpublished manuscript) found that participants were slower to identify images of low-power postures as low power when the images were of a man compared to a woman. Conversely, participants are slower to identify images of high-power postures as high power when the images were of a woman compared to a man. Similarly, Bailey and Kelly (2015) found that images of men in low-power postures do not readily activate low-power constructs. In both studies, although target gender was task irrelevant, it still influenced speed of categorization. Thus during perception, counter-stereotypical target gender disrupts the power information otherwise conveyed by posture. However, during enactment, the role of gender is less clear.

Research concerning men and women's spontaneous enactment of power relevant nonverbal displays is more mixed. Some early research shows that men and women enact different patterns of nonverbal displays (Bente, Donaghy, & Suwelack, 1998; Ickes & Barnes, 1977) consistent with high power and low power, respectively (Henley, 1995). For instance, Hai, Khairullah, and Coulmas (1982) found that even when controlling for body size, men are more likely than women to take up more space and use the shared armrest on an airplane. However, Dovidio, Heltman, Brown, Ellyson, and Keating (1988) showed that men's and women's nonverbal displays are contextually variable, differing as a function of domain expertise. They found that in feminine tasks, women enacted more high-power nonverbal displays compared to men. However, men enacted more high-power nonverbal displays *both* in masculine and in neutral tasks. Thus, when it comes to power, men and women tend to avoid stereotype-inconsistent nonverbal displays (i.e. for men, low-power postures, and for women, high-power postures), but this difference is contingent on context (de Lemus, Spears, & Moya, 2012; Dovidio et al., 1988; Hall, 1998).

Because of its relevance to power, gender interacts with the perception of and, to a lesser extent, enactment of power-relevant postures. Thus, men in low-power postures and women in high-power postures may represent norm violations (Asch, 1946; Kahneman & Miller, 1986). Some norm violations elicit negative affect and ratify existing norms while other violations serve as role models for norm inconsistent behavior (Burgoon & Hale, 1988). Norm violations due to gender may affect participants' willingness to engage in directed postures, with implications for embodiment (e.g. Carney et al., 2010).

If perceivers view men in low-power postures and women in high-power posture negatively, perceivers will engage in norm-consistent behavior to avoid eliciting a similarly negative response. Alternatively, if the perceivers view the norm violators positively, this may eventually erode the norm. Perceivers will instead mark the norm violators as role models, leading the perceivers to engage in norm-inconsistent behavior themselves. Considering the consequential ramifications of the embodied effects of power postures, it is important to understand how gender influences men and women's willingness to fully engage in power-relevant postures.

The present study

In the present study, we presented male and female participants with four images of men and women in either low-power or high-power postures. Participants were instructed to adopt each pictured posture for an unspecified duration and were videotaped. The primary dependent measure was how long participants maintained each posture as a function of their own gender, the type of posture, and the model's gender. At the end of the study, after participants had enacted either four high-power postures or four low-power postures, we measured their risk-taking behavior and felt power to replicate Carney and colleagues (2010).

Concerning duration in the posture, we made the confirmatory predication that participants would engage in postures that are counter-stereotypical with respect to their own gender for shorter durations (e.g. Dovidio et al., 1988). Thus, male participants would engage in low-power postures for shorter durations than high-power postures, and female participants would engage in high-power postures for shorter durations than low-power postures.

Further, we hypothesized that the effect of model gender would be most pronounced for counter-stereotypic postures. Thus, model gender would affect posture duration for male participants concerning low-power postures and for female participants concerning high-power postures. Regarding the nature of this effect we had two opposing hypotheses. These predictions were exploratory as we allowed for either possibility to be true. The alternative predictions were:

(a) If counter-stereotypical posture and model gender combinations ratify existing norms (Burgoon & Hale, 1988), gender counter-stereotypical models will elicit shorter posture durations than stereotype-consistent models. For example, male participants enacting low power postures will enact that posture for *less* time when exposed to the counter-stereotypical male model compared to the stereotype-consistent female model.

(b) However, if counter-stereotypical gender and posture combinations counter the existing norm, gender counter-stereotypical models will lead to longer posture durations compared to stereotype-consistent models. For example, male participants enacting low power postures will enact the posture for *more* time when exposed to the counter-stereotypical male model compared to the stereotype-consistent female model.

In our analyses, we allowed for the possibility that (a) may be true for one gender whereas (b) may be true for the other gender considering differences in reactions to gender role violations perpetrated by men compared to women (e.g. Brescoll & Uhlmann, 2008; Hilgenkamp & Livingston, 2002; Vandello, Bosson, Cohen, Burnaford, & Weaver, 2008).

The risky behavior and felt power measures were included to replicate Carney et al. (2010). Thus, we made confirmatory predications that participants in the high-power posture condition would be more likely to engage in risky behavior and express more felt power relative to those in the low-power posture condition. Further, we hypothesized that duration in the postures would moderate the strength of these effects; specifically, longer durations would lead to more robust effects on both felt power and the likelihood of risky behavior. Thus, a conceptual link between model gender and felt power and between model gender and risky behavior can be drawn but only in that model gender is hypothesized to affect duration, which in turn is hypothesized to affect felt power and risky behavior. In Ranehill et al. (2015), participants enacted each postures for 3 min compared to 1 min in the study by Carney et al. (2010) and did not find that posture affected risky behavior. This suggests that there may be a threshold to how long participants can maintain each posture and still elicit the anticipated outcomes. Thus, we allowed for a curvilinear relationship in our analyses, only anticipating that it would be predictive if participants held postures for longer than 3 min (Ranehill et al., 2015). Finally, we also tested if sample (recruited vs. subject pool) or familiarity with the topic (naïve vs. familiar) moderated the results.

Method

Participants

Target sample size was determined *a priori* to be at least 80 participants and as many as 120. The program G*power indicated that 40 participants were needed to achieve 0.80 power (1 – β) for an interaction between posture (between-subjects) and model gender (within-subjects) (Faul, Erdfelder, Lang, & Buchner, 2007). Because we anticipated a three-way interaction among participant gender (between-subjects), posture condition (between-subjects), and model gender (within-subjects), we took this estimate and doubled it to account for the additional between-subjects factor. The analysis was based on an anticipated small to medium effect size, $\eta_p^2 = 0.05$ (Cohen, 1992), alpha level, $p = 0.05$, and correlation among representative measures, $r = 0.50$. The upper limit of 120 participants provides 30 participants per cell in accordance with convention. We ensured comparable numbers of male and female participants to allow testing for participant gender differences. Participants came from a recruited sample and from an introductory psychology course subject pool.

Ultimately, we collected data from 95 participants; one participants' video data was lost due to a technical error. This resulted in 94 participants (49 women, 39 white, 18 black, 14 Asian, 8 Hispanic or Latino, 7 South Asian, and 8 of other races and ethnicities, $M_{age} = 20.60$, $SD_{age} = 3.79$). A total of 62 participants were recruited and 32 came from the subject pool. Supplementary analyses considered differences between the samples; details are reported below. Participants were compensated \$5 or the equivalent course credit. The Institutional Review Board reviewed and approved this experiment.

Materials

Model images consisted of an existing normed stimulus set (Figure 1). Three heterosexual-identifying Caucasian men and women displayed four low-power and four high-power postures culminating in 48 images. The models wore dark colored clothing and no jewelry. Each model's face showed a neutral expression, which was digitally transposed onto all images for that model. Models heights were representative of the average heights for Caucasian men and women (Vissher, 2008). Postures were drawn from the relevant literature (Buss, 2004; Carli, LaFluer, & Loeber, 1995; Carney et al., 2010; de Lemus et al., 2012; Hall et al., 2005; Henley, 1995; Ridgeway, 1987; Tiedens & Fragale, 2003); all emphasized contractiveness versus expansiveness. Previous work confirmed that the low-power and high-power postures were consistently rated as such and that the male and female models did not differ in perceived attractiveness (Bailey & Kelly, 2015). Each participant saw four images culled from the larger set in a semi-random

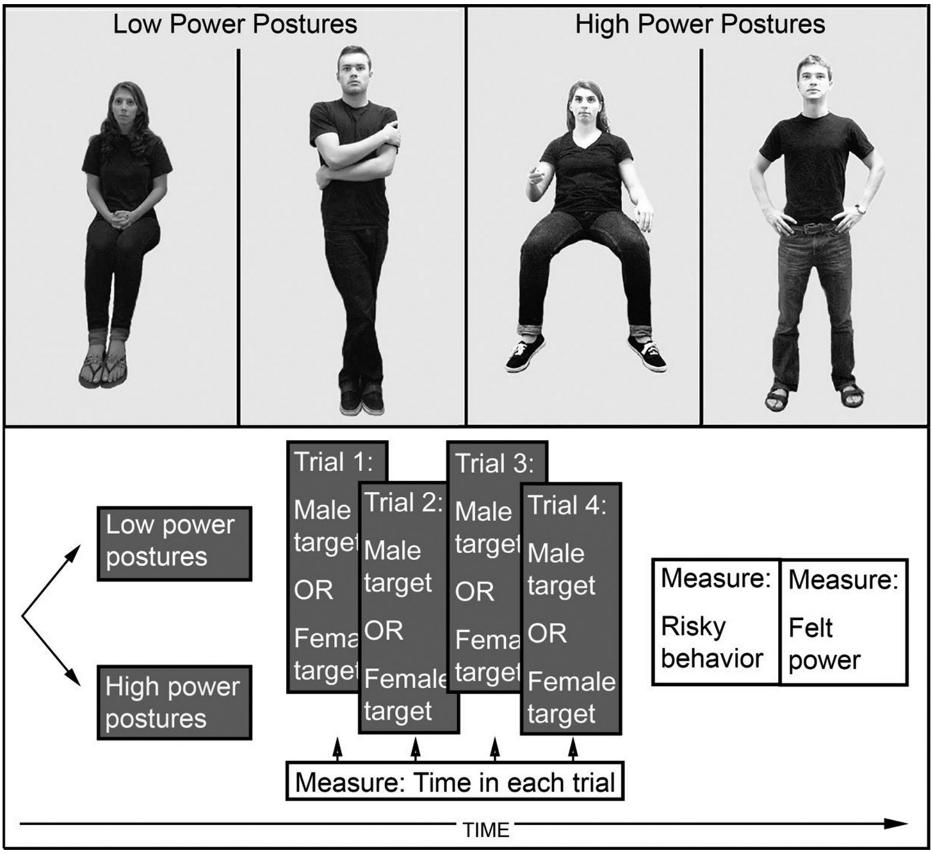

Figure 1. Example stimuli and a schematic of the procedure. Participants saw only either low-power postures or high-power postures, but saw both male and female models. We measured time in the posture after each trial but measured risky behavior and felt power only once at the end.

fashion ensuring that each participant only saw either low-power or high-power postures enacted by two men and two women.

To measure risk-taking behavior, we used the Carney et al. (2010) measure for our recruited sample and a modified version using tokens instead of money for the subject pool sample. In the latter case, participants exchanged the tokens for snacks at the end of the study. This version was used to comply with Departmental policies. Participants were given \$2 (or two tokens) and told that they could either keep them, or roll a die with a 50/50 chance of receiving no payment or \$4 (or four tokens). Felt power was measured as in Carney et al. (2010); participants indicated how *powerful* and how *in charge* they felt on a scale from 1 (*not at all*) to 4 (*a lot*). These two items were combined into a single index (r = 0.78).

Procedure

Participants were brought into a private room by the experimenter and sat in front of a computer monitor. After consenting to participate, the experimenter provided a cover story that the experiment concerned the effect of posture on people's cognitive performance (Cesario & McDonald, 2013). Participants then completed a practice trial with the experimenter in the room to monitor task comprehension. The practice trial was identical to the experimental trials with the exception that the posture was a neutral posture (Tiedens & Fragale, 2003). Participants were reminded that they were being videotaped to ensure adequate compliance. The experimenter then left the room. The experimenter did not know whether the participants were in the low-power or high-power posture condition.

For each trial, participants saw an image of a person in a particular posture and examined it for 10 s. The image then disappeared to ensure that participants' maintenance of the posture was indicative of their embodied experience rather than reflecting a visual preference for the image. After the image disappeared, participants were instructed to enact the posture themselves. While enacting the presented posture, participants viewed a series of faces presented in a continuous loop, which they believed they would be asked to remember later. The faces were taken directly from Carney et al. (2010); this was done to convey a social context (Carney et al., 2015).

To bolster the cover story, following each trial participants were given a short word identification task (Hass & Eisenstadt, 1990; Kawakami, Dovidio, & Dijksterhuis, 2003; Rastle, Harrington, & Coltheart, 2002) and a short modified Stroop (1935) task.

After the completion of the experimental trials, the experimenter administered the measure of risky behavior adapted from Carney et al. (2010) and then again left the room. The participants then completed the two-item measure of felt power adapted from Carney and colleagues (Carney et al., 2010). Thus, risky behavior and felt power were measured once after all four experimental trials were complete (Figure 1). Participants then completed additional measures not relevant to the present study.

Finally, participants were asked to guess the purpose of the study and were explicitly asked about their familiarity with Cuddy's (2012) well-publicized TED talk to test whether or not familiarity with the topic influenced the results. Overall, 27 participants reported familiarity with the topic; 13 reported having seen the TED talk (Cuddy, 2012); 10 were familiar with research on power postures though they did not remember viewing the TED talk; and 3 correctly guessed that the study involved power postures. Supplementary analyses considered the effect of familiarity with the topic; details are reported below. Finally, participants were debriefed and compensated. The experiment took approximately 30 min.

Preliminary analyses

Two independent coders, blind to hypotheses and to target gender condition, determined how long each posture was maintained. To do this, each coder initially viewed all six models in each posture to develop a template of what the postures should look like. They then viewed the video clips arranged such that male and female participants and high-power and low-power postures were distributed throughout the coding process to prevent any systematic impact of coder fatigue or practice effects. As anticipated, there was variability in participants' ability or willingness to enact each posture. For instance, when instructed to engage in an expansive posture, some participants copied it exactly while others placed their arms correctly but neglected the leg position. Further, some participants attempted multiple iterations of the same posture. The coders recorded how long participants maintained each iteration of the posture and indicated how well each iteration matched the models on a sliding scale of 0 (*not at all*) to 1 (*a lot*). This was an important metric to include given that the design of the present study did not facilitate the exact placement achieved in Carney and colleagues (Carney et al., 2010). To assess inter-coder reliability we used a two-way average intra-class correlation (ICC) as is appropriate for continuous measures where all subjects are rated by both the coders (Hallgren, 2012; Shrout & Fleiss, 1979). The resulting ICC was high, > 0.99, and the two coders estimates were averaged.

Time in the posture was calculated using two different approaches, a transformed time measure and a raw time measure. The transformed time measure reflected participants' ability and/or willingness to enact the presented posture. For this measure, duration in the posture was a weighted sum of the different iterations for that participant, weighted according to the matching score. Duration was also $\log_{10}$ transformed as time measures are positively skewed (Osborne, 2008). Thus, if the coders determined that the participant enacted four iterations, each iteration, *excluding* any time in-between attempts, contributed to the total time in the posture as a function of how well the iteration matched the template. For the second measure, we calculated time in the posture as the total time from the initiation of the first iteration to the cessation of the final iteration. If the coders determined that the participant enacted four iterations, all four and any time in-between contributed equally to the total time in the posture. Our hypotheses applied to both and analyses were conducted separately; see below for the results concerning the transformed time measure; Appendix A1 reports the analyses using the raw time measure. Results for these two approaches to calculating the time-dependent measure were largely consistent.

Results

Preregistered analyses

Transformed posture duration

Participants maintained each posture for an average of 36.89 s (SD = 14.88 s, range: 3.88 s – 89.50 s). Given that participants maintained four postures in total, this translates to an average total time in the postures of approximately 2 min and 30 s, which is comparable to the set time of 2 min employed by Carney and colleagues' (Carney et al., 2010). Note that these values reflect the raw time measure. The majority of the time participants followed instructions and attempted only a single iteration of the posture (71.81%).

To assess participants' time in each posture, we conducted a 2 (Participant Gender: male or female) × 2 (Posture condition: high or low power) × 2 (Model Gender: male or female) mixed analysis of variance (ANOVA) with model gender as a repeated measures factor using IBM SPSS software. Here, analyses reflect the transformed time measure, see Appendix A1 for the analyses using the raw time measure.

We predicted a two-way interaction, with male participants expected to enact low-power postures for less time than high-power postures and female participants expected to enact high-power postures for less time than low-power postures. We did not find evidence for this interaction, $F(1,90) = 0.58$, $p = .449$, $\eta_p^2 = .01$. However, we did find an unpredicted main effect such that participants, regardless of gender, maintained low-power postures ($M = 1.36$, $SD = 0.22$) for less time than high-power postures ($M = 1.45$, $SD = 0.19$), $F(1, 90) = 4.68$, $p = .033$, $\eta_p^2 = .05$. Thus contrary to predictions, female participants did not hesitate to enact the high-power postures.

Consistent with predictions, we found evidence for a three-way interaction whereby male and female participants' durations in low-power and high-power postures were differently influenced by model gender, $F(1,90) = 4.15$, $p = .045$, $\eta_p^2 = .04$. The obtained pattern is presented in the upper panel of Figure 2, while the predicted pattern that most closely matched the obtained results is displayed in the lower panel. Given that low-power postures are counter-stereotypic for men and high-power postures are counter-stereotypic for women, we considered the effect of posture condition and model gender separately for male and female participants.

Among male participants, there was evidence for the predicted interaction between model gender and posture type, $F(1,43) = 5.18$, $p = .028$, $\eta_p^2 = .11$. Male participants

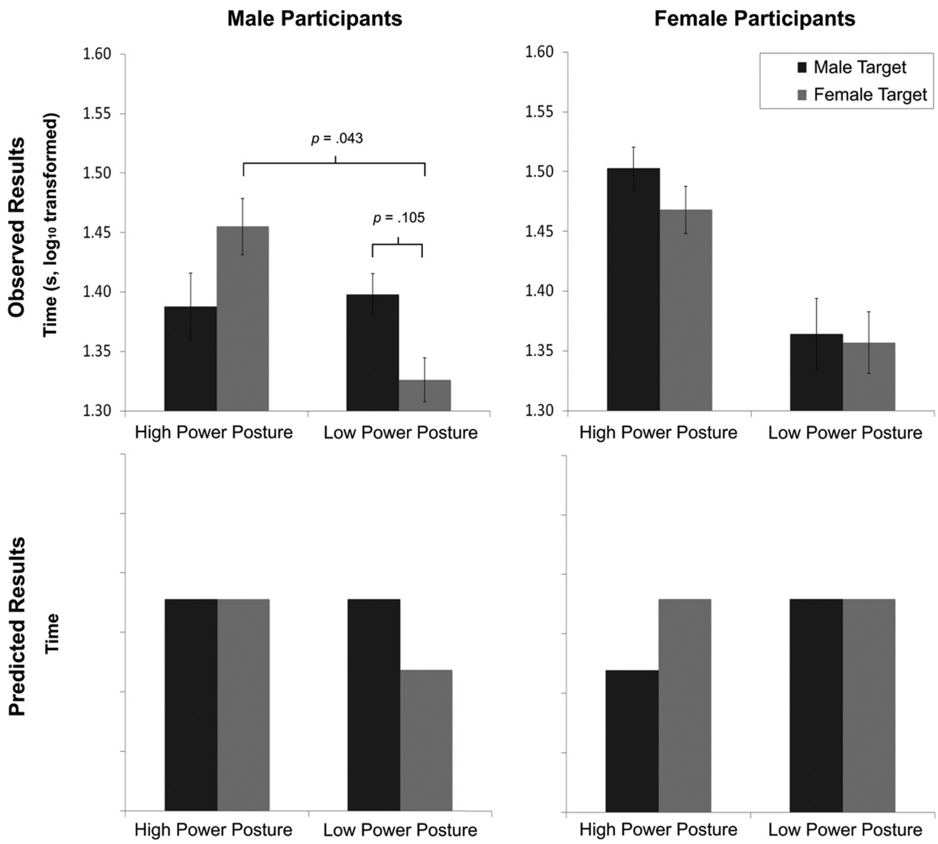

Figure 2. Obtained time spent in the postures (top) and predicted time (bottom). Male participants' (left) enacted the low-power postures for less time when they copied a female model; female participants' (right) time in the postures did not vary with model gender. Error bars represent standard error.

maintained the low-power postures for a distinctly short time when duplicating a female model. The planned contrast provided only directional support for this claim as the analysis did not reach significance between female models (M = 1.33, SD = 0.18) and male models (M = 1.40, SD = 0.17) in the low-power posture condition, $F(1,20) = 2.88$, $p = .105$, $\eta_p^2 = .13$. Supporting the idea that the stereotype-consistent (lower power) female models led to shorter durations, male participants maintained the postures for significantly less time when copying female models in the low-power posture condition (M = 1.33, SD = 0.18) compared to female models in the high-power posture condition (M = 1.46, SD = 0.23), $F(1,43) = 4.35$, $p = .043$, $\eta_p^2 = .09$. This latter contrast shows that it was not female models *per se* that led male participants to maintain the postures for less time, but rather the combination of female models in low-power postures. The complementary contrasts concerning the effect of model gender for high-power postures (female models: M = 1.33, SD = 0.18, male models: M = 1.39, SD = 0.27), $F(1,23) = 2.36$, $p = .136$, $\eta_p^2 = .09$, and the effect of condition for male models (low-power postures: M = 1.39, SD = 0.27, high-power postures: M = 1.40, SD = 0.17), $F(1,43) = 0.02$, $p = .886$, $\eta_p^2 < .01$, did not approach significance.

Female participants did not show evidence for the predicted two-way interaction between posture condition and model gender, $F(1,47) = 0.25$, $p = .623$, $\eta_p^2 = .01$. Further, model gender did not affect female participants' duration in the high-power postures concerning the planned contrast between high-power female models (M = 1.47, SD = 0.19) and low-power male models (M = 1.50, SD = 0.17), $F(1,21) = 0.53$, $p = .476$, $\eta_p^2 = .02$.

In sum, both male and female participants adopted low-power postures for less time than high-power postures. Male participants' time in the postures was sensitive to the combination of posture type and model gender. Specifically, a female model in the low-power postures led to particularly short durations for male participants. In contrast, female participants' time spent in the postures was unaffected by the combination of posture type and model gender.

Risky behavior

To assess participants' risky behavior, we conducted logistic regressions in R with dummy coded model posture condition interacting with each participants' mean centered time spent in the postures (Aiken, West, & Reno, 1991). Odds ratios (OR) serve as the effect sizes for our logistic regression analyses. We used chi-squares to compare sequential models, considering a possible curvilinear effect of time in the postures. Following prior work (Carney et al., 2010), we predicted that participants in the high-power posture condition would be more likely to make the risky choice, and that this would be particularly true for participants who maintained the high-power postures for longer durations.

However contrary to predictions, there was no evidence for an effect of time and posture condition on risky behavior, $B = -1.65$, $SE = 2.23$, $p = .459$, OR = 0.19, nor was there evidence for a curvilinear effect of time on risky behavior, $\chi^2(2, n = 94) = 1.42$, $p = .492$. The impact of posture type on risky behavior was in the anticipated direction, with more participants in the high-power posture condition taking the risk (60%) than in the low-power posture condition (52%); however, the effect was non-significant, $B = 0.36$, $SE = 0.42$, $p = .391$, OR = 1.43 (see Appendix A2 for the identical analysis using chi-square).

Felt power

To assess participants' felt power, we employed a similar approach but used linear regressions in lieu of logistic regressions with standardized Betas serving as effect sizes. We used ANOVA's to compare sequential models. Following prior work (Carney et al., 2010), we predicted that participants would report more felt power in the high-power posture condition compared to the low-power posture condition, and that this would be particularly true for participants who maintained the high-power postures for longer durations.

There was no evidence for the effect of time and posture condition, $B = 0.28$, $SE = 0.97$, $p = .772$, $Beta = 0.04$, or for a curvilinear effect of time, $F(2,88) = 2.17$, $p = .120$. The impact of posture condition on felt power was in the anticipated direction, with participants in the high-power posture condition reporting higher felt power ($M = 2.62$, $SD = 0.93$) than those in the low-power posture condition ($M = 2.39$, $SD = 0.94$), but the effect was non-significant, $B = 0.23$, $SE = 0.19$, $p = .227$, $Beta = 0.13$ (see Appendix A2 for the identical analysis using ANOVA).

Exploratory analyses

We ran additional analyses to rule out differences between the two samples and between naïve participants and those familiar with hypotheses. For posture duration, there was no evidence that sample (recruited vs. subject pool) or familiarity (naïve participants vs. participants familiar with the topic) significantly moderated the interaction between participant gender and posture condition, sample: $F(1, 86) < 0.01$, $p = .970$, $\eta_p^2 < .01$, familiarity: $F(1, 86) = 0.02$, $p = .904$, $\eta_p^2 < .01$. Similarly, there was no evidence that sample or familiarity moderated the interaction between participant gender, posture condition, and target gender, sample: $F(1, 86) = 0.54$, $p = .466$, $\eta_p^2 < .01$, familiarity: $F(1, 86) = 0.72$, $p = .400$, $\eta_p^2 < .01$.

For risky behavior, there was also no evidence that sample or familiarity moderated the predicted effect of posture condition on risky behavior, sample: $B < .01$, $SE = 0.88$, $p = .919$, OR = 1.09, familiarity, $B = -0.29$, $SE = 0.92$, $p = .754$, $OR = 0.75$, nor that familiarity moderated the predicted interaction between posture condition and posture duration on risky behavior, familiarity: $B = -0.57$, $SE = 6.04$, $p = .925$, $OR = 0.57$. The potential moderation of sample showed evidence of model over-fitting (OR = 279); but considering the posture condition and posture duration interactions separately for the two samples also did not revel additional insights.

However, concerning felt power, there was evidence that sample and familiarity effected participants' responses. The two samples responded to the posture conditions differently; the two-way interaction between sample and posture condition was significant, $B = 0.80$, $SE = 0.40$, $p = .050$, $Beta = 0.41$ (Figure 3). The recruited sample behaved according to predictions and reported feeling more powerful in the high-power posture condition ($M = 2.78$, $SD = 0.97$) compared to the low-power posture condition ($M = 2.28$, $SD = 1.03$), $B = 0.50$, $SE = 0.25$, $p = .036$, $Beta = 0.25$. The subject pool sample's felt power did not significantly differ between posture conditions (high power: $M = 2.25$, $SD = 0.75$, low power: $M = 2.56$, $SD = 0.74$), $B = -0.31$, $SE = 0.27$, $p = .261$, $Beta = -0.20$.

Naïve participants and participants familiar with the topic also responded differently to the posture conditions as a function of their time spent in the postures; the three-way

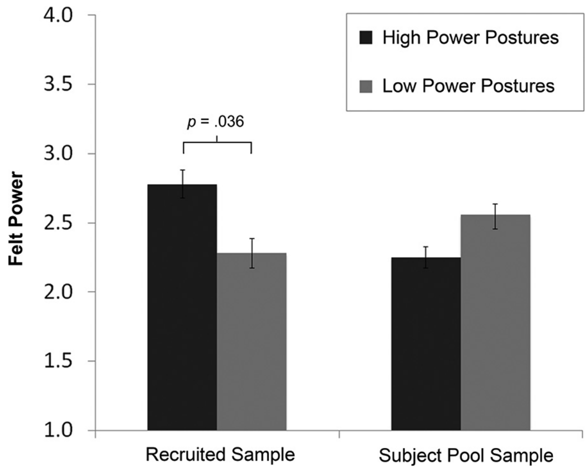

Figure 3. The recruited sample's (left) and the subject pool sample's (right) felt power in each posture condition. The recruited sample behaved according to predictions and reported feeling more powerful when enacting high-power postures compared to low-power postures. Error bars represent standard error.

interaction among familiarity, posture condition, and posture duration was significant, $B = -7.46$, $SE = 2.23$, $p = .001$, $Beta = -0.70$ (Figure 4). Naïve participants behavior conformed more to predictions; the more time they spent in the high-power postures the *more* powerful they reported feeling, $B = 1.96$, $SE = 0.91$, $p = .034$, $Beta = 0.44$. Time did not significantly impact felt power in the low-power posture condition, $B = -0.49$, $SE = 0.65$, $p = .449$, $Beta = -0.11$. Participants familiar with the topic showed the opposite pattern; the longer they maintained the high-power posture the *less* powerful they reported feeling, $B = -2.40$, $SE = 1.09$, $p = .031$, $Beta = -0.54$, and there was no effect in the low-power posture condition, $B = 2.62$, $SE = 1.59$, $p = .104$, $Beta = 0.58$.

Discussion

The present study had two primary goals. First, it investigated whether gender, both participant gender and model gender, influenced participants' duration spent in power-related postures. Second, it sought to replicate Carney and colleagues' (Carney et al., 2010) finding that enacting high-power postures leads to more risky behavior and more felt power, with additional considerations of time spent in the postures, sample, and familiarity with the topic.

Posture duration

Participants enacted the high-power postures longer than the low-power postures and, contrary to predictions, there was no evidence that male and female participants differed in this respect. Though unpredicted, the finding that participants generally enacted high-power postures longer than low-power postures is compatible with work on the positive psychological consequences of having power (Galinsky, Rucker, & Magee,

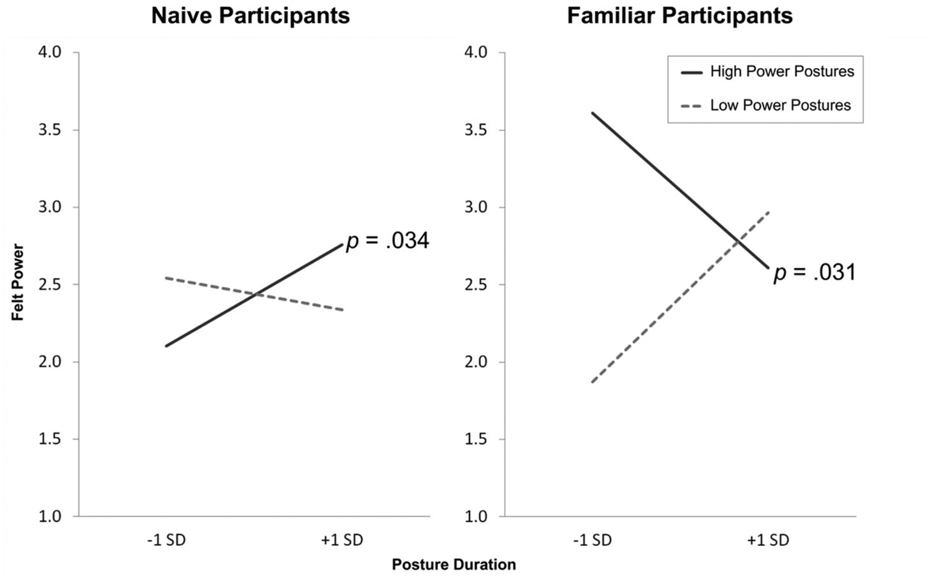

Figure 4. The relationship between time and felt power among naïve participants' (left) and participants familiar with the topic's (right). Naïve participants' behavior more closely matched predictions; the more time they spent in the high-power postures the more powerful they reported feeling.

2015). Feeling powerful activates approach tendencies (Keltner, Gruenfeld, & Anderson, 2003), leads to more positive affect (Wojciszke & Struzynska–Kujalowicz, 2007), improves self-esteem (Fast, Gruenfeld, Sivanathan, & Galinsky, 2009; Wojciszke & Struzynska–Kujalowicz, 2007), and increases optimism (Fast et al., 2009). Thus given these positive outcomes, participants in the present study choose to adopt the high-power postures longer than the low-power postures.

Further, we found that the particular combination of participant gender, posture condition, and model gender was influential as evidenced by the significant three-way interaction. While female participants' duration in the postures was unaffected by gender and posture combinations, male participants copying female models in low-power postures led to distinctly short durations. Specifically, male participants enacted low-power postures for less time when duplicating a female model in a low-power posture compared both to a male model in a low-power posture and compared to a female model in a high-power posture. Thus, male participants enacted low-power postures for less time than high-power postures, and this difference was exacerbated when the model portraying low power was a woman. It may be that low-power female models ratify the association between women and low-power postures. Alternatively, or perhaps in combination, male participants may have been held the stereotype-inconsistent low-power postures longer when copying a male model compared to a female model because a male model in a low-power posture erodes the association between women and low-power postures (Burgoon & Hale, 1988). Given that the planned contrast was only marginally significant, this interpretation should be treated with caution. Regardless, the overall pattern suggests that male participants' enactment of power postures was more responsive than female participants' enactment to associations between gender and power.

Research on the rigidity of male gender roles provides one explanation for the finding that male participants' time in the postures was sensitive to counter-stereotypic posture and gender combinations whereas female participants' was not. Work in anthropology (Gilmore, 1990), sociology (Willer, Rogalin, Conlon, & Wojnowicz, 2013), developmental psychology (Hemmer & Kleiber, 1981; Hilgenkamp & Livingston, 2002), and social psychology (Pleck, 1995; Vandello et al., 2008) all suggest that men's social status as men is more precarious than women's social status as women. For example, participants report that men's gender status is more tenuous than women's, more strongly endorsing items such as, "All boys do not grow up to become real men," and "Other people often question whether a man is a 'real man'," compared to analogous statements concerning women (Vandello et al., 2008). Additional studies find that male participants report feeling more anxious than female participants after having engaged in gender counter-stereotypic behavior, and male participants will subsequently engage in more gender stereotype-consistent behavior including aggressive thoughts, aggressive action, more support for dominance hierarchies, and a greater desire to advance in dominance hierarchies (Bosson, Vandello, Burnaford, Weaver, & Wasti, 2009; O'Neil, Good, & Holmes, 1995; Vandello et al., 2008; Willer et al., 2013). Thus, in the present study male participants' greater sensitivity to counter-stereotypic posture and gender combinations compared to female participants may be indicative of men's greater concern with violating gender norms.

Future research could investigate the processes underlying the effects of participant gender in enacting low-power versus high-power postures and the role of stereotypic associations. For instance, it is possible that men are more likely than women to attend to and recognize gender incongruity in power postures. Alternatively men and women may show similar awareness for gender incongruity but men may be more behaviorally reactive. Previous research suggests the latter explanation. Both male and female participants show evidence of a categorization difficulty concerning counter-stereotypic combinations of posture and target gender during *perception* (Bailey & Kelly, 2015; Bailey et al., unpublished manuscript). However, in the present study, only male participants' behavior seems to be affected by counter-stereotypic combinations during *enactment*. Additional research assessing both perceptual processing and enactment could more definitively identify the mechanisms behind the observed gender differences in the enactment of power postures.

Contrary to predictions, female participants' did not shy away from enacting the high-power postures, regardless of the model's gender. This result is consistent with the more mixed findings on gender differences in the enactment of power-relevant nonverbal displays, which sometimes find that female participants are just as likely as male participants to engage in high-power postures (de Lemus et al., 2012; Dovidio et al., 1988; Hall, 1998). Nonverbal displays have been identified as one behavior that women can do to signal high power without incurring the negative backlash elicited by other high-power behaviors (Brescoll & Uhlmann, 2008; Livingston, Rosette, & Washington, 2012; Williams & Tiedens, 2016). Given this, the current finding that female participants persist in maintaining high-power postures indicates that women may be able to use high-power postures to signal that they have power to others and to reap the positive psychological benefits of enacting power for themselves.

Replicating risky behavior and felt power

We replicated the direction but not the statistical significance of Carney and colleagues' (2010) which found that participants who enacted high-power postures were more likely to take a risk and reported higher felt power than those who enacted low-power postures. Further, the results concerning felt power highlighted the need for attention to moderators as only participants from the recruited sample and naïve participants showed the predicted effects. We first consider four methodological differences that may explain the failure to significantly replicate the effects of Carney and colleagues (Carney et al., 2010).

First, we used a modified version of the risk-taking measure in the subject pool sample. However, even among the recruited sample (n = 62), which used an identical measure, the present study did not significantly replicate Carney et al. (2010). Second, in the present study, participants physically copied a visual image rather than being placed in the posture by the experimenter. Although our method may have reduced compliance, it ensured that the experimenter who administered the risk-taking measure was blind to posture condition. We measured participants' compliance through the coding process and integrated it into our transformed time measure, still without producing a statistically significant replication.

Third, in the present study, we allowed time in the postures to vary rather than establishing a fixed duration. Participants maintained the postures for a comparable total duration (2 min and 30 s) to Carney and colleagues (2 min). When the analyses were conducted on only those participants who maintained the postures for at least a total of 2 min (n = 62), the replications did not reach significance. Possibly the use of four postures as opposed two postures is less effective as it lowers the average time spent in *each* posture. Achieving at least a minute in *each* posture may be critical in initiating the physiological changes thought to cause the behavioral effects. Finally, the present study used slightly different postures than Carney and colleagues (Carney et al., 2010). If this is the source of non-replication, Additional work is needed to clarify what specifically constitutes a high-power posture suitable for eliciting embodied findings. The postures used in the present study operationalized power postures as varying in expansiveness versus contractiveness, which has been vetted in prior work (e.g. Bailey & Kelly, 2015; Hall et al., 2005).

Familiarity with the topic also moderated the interaction between duration and posture condition on felt power. Naïve participants behaved according to predictions; the *more* time they spent in the high-power postures the *more* felt power they reported, and the relationship between time and felt power was in the opposite direction for low-power postures. However, the results concerning participants who were familiar with the hypotheses showed a very different pattern (see Figure 4). For these participants, those who held the posture for a brief duration (1 standard deviation below the mean) showed the anticipated effect: those in the high-power posture condition reported greater felt power than those in the low-power posture condition. This difference, which conforms to previous work on power poses with which participants were familiar (Carney et al., 2010), may reflect the general motivation of research participants to behave in ways supportive of a researcher's expectations (i.e. demand characteristics; Orne, 1962). However, this effect did not persist among participants who held the postures longer (1 standard deviation above the mean). It is possible that the greater effort exerted by participants while holding the postures for a longer time may have undermined their confidence in the anticipated effect of the postures (Schwarz, Sanna, Skurnik, & Yoon, 2007), thereby producing less felt power in the high-power posture condition as time in the posture increased. Although this interpretation of the different effects for participants familiar and unfamiliar with the topic is admittedly speculative, the moderating effect of familiarity highlights a potential challenge for social psychological research generally: as discoveries become widely disseminated, the phenomenon itself can change (Dovidio, 2016; Gergen, 1973).

Finally, sample moderated the effect of posture condition on felt power; only participants from the recruited sample reporting higher felt power in the high-power compared to the low-power posture condition. Park, Streamer, Huang, and Galinsky (2013) report that cultures differ in both what constitutes high-power nonverbal displays and the social appropriateness of such displays. The recruited sample was statistically significantly older, poorer, and more politically liberal than the subject pool sample. Though none of these factors could account for the effect of sample, it is possible that even within the US differing norms concerning power postures operate in these microcultures. If such different norms exist, they may have resulted in high-power postures leading to more felt power only in the recruited sample. It is not clear from the present work what demographic factors might have caused this difference.

Limitations and future directions

Methodologically, one limitation of the present work is that participants maintained the postures for relatively short durations. A curvilinear effect of posture duration on risky behavior and felt power was predicted only if participants' time in a given posture approached or exceeded 3 min as in Ranehill and colleagues (2015). However, the maximum time in a single posture was only 1 min and 30 s. Thus, a curvilinear effect may still exist with longer durations.

An additional limitation of this study is that it considered the effect of model gender on posture durations for White models only. Race intersects with gender in important ways; a phenomenon called gendered race suggests that feminine norms may be more pronounced for Asian models and masculine norms may be more pronounced for Black models (Galinsky, Hall, & Cuddy, 2013; Livingston et al., 2012). Thus, investigating the role of combinations of gender and race identities on participants' durations in the postures would be an important direction for future work. For example, male participants may be particularly hesitant to enact low-power postures for long following an Asian female model.

Conclusion

Prior work finds that enacting high-power postures has consequential outcomes; it both instills a sense in the self and conveys power to others (Cesario & McDonald, 2013; Carney et al., 2010, 2015; Schmid Mast & Hall, 2004). The present study did not replicate the effect of high-power postures in encouraging risky behavior or increasing felt power

(Carney et al., 2010). However, the findings concerning felt power highlight the importance of considering moderators and suggest that dissemination of findings may alter the phenomenon in counterintuitive ways.

Given that gender is relevant to power (e.g. Berger et al., 1972; Eagly & Karau, 2002), the present study also investigated the role of participant gender and model gender in influencing participants' persistence in enacting power postures. Consistent with theories about the rigidity of male gender roles, male participants' were more attentive to gender role violations, spending the least amount of time in low-power postures following a norm ratifying female model. Surprisingly, female participants showed no qualms about enacting high-power postures. Thus, although women in high-power postures are likely perceived as being counter-stereotypic (Bailey et al., unpublished manuscript), this does not impact women's own willingness to enact the postures, giving them access to the positive downstream consequences of enacting power (e.g. Fast et al., 2009).

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Disclosure statement

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Appendices

Appendix A1. Analyses using the alternative approach to calculating time

This measure of time was calculated from the start of any posture attempts to the end of any attempts for each participant on each posture trial, capturing any time in-between and not accounting for how well each attempt matched the posture templates. We report the key analyses concerning this time measure below. *An asterisk indicates a change in significance level from the transformed time measure reported in the Results section. No effects changed in direction.

Preregistered Analyses

Raw Posture Duration. Participants maintained the low-power postures (M = 33.46, SD = 13.54) for less time than the high-power postures (M = 40.47, SD = 15.51), $F(1, 90) = 5.27$, $p = .024$, $\eta_p^2 = .06$, and male and female participants did not differ in this respect, $F(1,90) = 0.39$, $p = .535$, $\eta_p^2 < .01$.

*Participant gender did not significantly interact with model gender and posture condition, $F(1,90) = 0.03$, $p = .862$, $\eta_p^2 < .01$. *Among male participants, both model gender and posture condition were important in determining time in the postures as the two marginally interacted, $F(1,43) = 2.86$, $p = .098$, $\eta_p^2 = .06$. The planned contrast for male participants in the low-power posture condition was marginally significant, $F(1,20) = 3.86$, $p = .063$, $\eta_p^2 = .16$, with male participants maintaining low-power postures for less time when copying a female model ($M = 32.72$, $SD = 12.04$) compared to a male model ($M = 35.81$, $SD = 12.08$). *Participants maintained the postures for less time when copying female models in the low-power posture condition ($M = 32.73$, $SD = 12.04$) compared to female models in the high-power posture condition ($M = 39.80$, $SD = 20.02$), $F(1,43) = 2.01$, $p = .163$, $\eta_p^2 = .05$. The complementary contrasts concerning the effect of model gender for high-power postures (female models: $M = 39.80$, $SD = 20.02$, male models: $M = 38.85$, $SD = 15.59$), $F(1,23) = 0.30$, $p = .585$, $\eta_p^2 = .01$, and the effect of condition for male models (low-power postures: $M = 35.80$, $SD = 12.08$, high-power postures: $M = 38.85$, $SD = 15.59$), $F(1,43) = 0.55$, $p = .473$, $\eta_p^2 = .01$, did not approach significance.

Among female participants, there was no evidence that model gender differentially affected how long female participants maintained the postures. Model gender and posture condition did not interact, $F(1,47) = 0.91$, $p = .345$, $\eta_p^2 = .02$, the planned contrast between female models ($M = 42.41$, $SD = 15.41$) and male models ($M = 40.95$, $SD = 14.38$) in the high-power posture condition did not approach significance, $F(1,21) = 0.31$, $p = .589$, $\eta_p^2 = .01$.

Risky behavior. There was no evidence for an interaction between posture condition and time, $B = -0.03$, $SE = 0.03$, $p = .258$, OR = 0.97. The curvilinear model did not provide additional insight compared to the linear model, $\chi^2(2, n = 94) = 2.23$, $p = .328$, $\phi_c = 0.11$.

Felt power There was no evidence for an interaction between posture condition and time, $B < -0.01$, $SE = 0.01$, $p = .728$, $Beta = -0.06$. The curvilinear model did not provide additional insight compared to the linear model, $F(2,88) = 1.52$, $p = .223$.

Exploratory Analyses

For posture duration, there was no evidence that sample or familiarity significantly moderated the interaction between participant gender and posture condition, sample: $F(1, 86) = 1.81$, $p = .182$, $\eta_p^2 = .02$, familiarity: $F(1, 86) = 0.11$, $p = .740$, $\eta_p^2 < .01$, or the interaction between participant gender, posture condition, and target gender, sample: $F(1, 86) = 0.51$, $p = .476$, $\eta_p^2 = .01$, familiarity: $F(1, 86) = 0.77$, $p = .381$, $\eta_p^2 = .01$.

For risky behavior, there was no evidence that sample or familiarity moderated the interaction between posture condition and posture duration on risky behavior, sample: $B = 0.07$, $SE = 0.07$, $p = .321$, $OR = 1.08$, familiarity: $B = 0.06$, $SE = 0.08$, $p = .460$, $OR = 1.06$.

For felt power, naïve participants and participants familiar with the topic also responded differently to the posture power conditions as a function of their mean time in the postures, $B = -0.10$, $SE = 0.03$, $p = .003$, $Beta = -0.69$. *Among naïve participants the more time they spent in the high-power postures the marginally *more* powerful they reported feeling, $B = 0.02$, $SE = 0.01$, $p = .058$, $Beta = 0.32$. Time did not significantly impact felt power in the low-power posture condition, $B < -0.01$, $SE = 0.01$, $p = .999$, $Beta < -0.01$. Participants familiar with the topic showed the opposite pattern; the longer they maintained the high-power posture the *less* powerful they reported feeling, $B = -0.03$, $SE = 0.02$, $p = .024$, $Beta = -0.55$, and there was no effect in the low-power posture condition, $B = 0.43$, $SE = 0.02$, $p = .077$, $Beta = 0.69$.

Appendix A2. Alternative statistics for risky behavior and felt power

Here, we report the effect of condition on risky behavior and felt power using the same analyses as Carney and colleagues (2010) to facilitate comparisons. Posture condition did not significantly affect the likelihood of making the risky choice, $\chi^2(1, n = 94) = 0.74$, $p = .391$, $\phi = 0.09$, or participants reported felt power, $F(1,93) = 1.48$, $p = .227$, $r = .03$. However, among participants in the recruited sample only, participants reported more felt power in the high-power posture condition compared to the low-power posture condition, $F(1,90) = 4.51$, $p = .036$, $r = .22$.

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Embodied power, testosterone, and overconfidence as a causal pathway to risk-taking

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ABSTRACT

Previous work has found that configuring participants into high-power versus low-power physical postures caused increases in subjective feelings of power, testosterone, and risk-taking as well as decreases in cortisol. We attempted to replicate and extend this pattern of findings by testing for the mediating role of testosterone and overconfidence in the relationship between power poses and risk-taking. We hypothesized that increases in testosterone in response to high-power poses would lead to increases in overconfidence, and that this indirect pathway would mediate the effect of power posing on risk-taking. We were unable to replicate the findings of the original study and subsequently found no evidence for our extended hypotheses. Overconfidence was unaffected by power posing and unrelated to testosterone, cortisol, and risk-taking. As our replication attempt was conducted in the Netherlands, we discuss the possibility that cultural differences may play a moderating role in determining the physiological and psychological effects of power posing.

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There is an inherent tension in decisions involving risk, with the allure of potential rewards offset against concerns of potential loss. The psychological scales on which these competing outcomes are balanced are sensitive to a myriad of factors, including personality (Dohmen et al., 2011; Weber, Blais, & Betz, 2002), gender (Byrnes, Miller, & Schafer, 1999; Ronay & Kim, 2006), hormones (Apicella, Dreber, & Mollerstrom, 2014), affect (Loewenstein, Weber, Hsee, & Welch, 2001), cognitive biases (Simon, Houghton, & Aquino, 2000), and social contexts (Ronay & Kim, 2006; Ronay & von Hippel, 2010a).

The experience of feeling powerful is one such factor reported to increase people's propensity for risk-taking (Anderson & Galinsky, 2006). Although moderated by individual differences in power motivations (Maner, Gailliot, Butz, & Peruche, 2007; Ronay & von Hippel, 2010b), the direct positive effect of power on risk-taking has been explained in terms of power's disinhibiting effects (Galinsky, Gruenfeld, & Magee, 2003) and its related association with the behavioral approach system (BAS; Anderson & Galinsky,

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2006). By virtue of activating this BAS, power leads people to allocate disproportionate attention to potential rewards, while obfuscating the costs of failure.

The power literature is dominated by experimental studies that have manipulated power by asking people to recall a time when they possessed (versus did not possess) power over another individual or individuals (Galinsky et al., 2003). In testament to the malleability of people's momentary sense of power, this manipulation has produced a host of behavioral effects ranging from corruption (Lammers, Galinsky, Dubois, & Rucker, 2015) to consumer behavior (Rucker & Galinsky, 2008)to creativity (Galinsky, Magee, Gruenfeld, Whitson, & Liljenquist, 2008). Even physical expansion versus contraction of the body has been found to have a positive influence on people's sense of psychological power and their willingness to take risks (Carney, Cuddy, & Yap, 2010; though see also Ranehill et al., 2015). Carney et al. (2010) reported that participants who were instructed to assume broad, expansive postures, versus contracted postures, were more willing to gamble an endowed $2 on a 50/50 chance of winning $4 via a die roll. The same paper reported that these "power poses" also increased participants' self-reported psychological power and circulating testosterone, while decreasing cortisol.

Nonetheless, existing research has yet to fully flesh out the bio-psychological causal pathway to risk-taking. As testosterone has been causally linked to both power (Booth, Granger, Mazur, & Kivlighan, 2006; Carney et al., 2010; Dabbs & Dabbs, 2000; Mazur & Booth, 1998) and risk-taking (Apicella, Carré, & Dreber, 2014; Coates & Herbert, 2008; Ronay & von Hippel, 2010), the relationship between embodied power and risk-taking may indeed be mediated by increases in testosterone. Alternatively, the relationship between embodied power and risk-taking may be jointly mediated by the interaction between testosterone and cortisol. Mehta and Josephs (2010) have proposed a "dual hormone hypothesis," whereby cortisol modulates the effect of testosterone on behavior, such as risk-taking (Mehta, Welker, Zilioli, & Carré, 2015). Because Carney et al. (2010) found high-power poses to both increase testosterone *and* decrease cortisol, the interaction between these effects may predict risk-taking, as has been previously shown (Mehta et al., 2015). Furthermore, although Ranehill et al. (2015) failed to replicate Carney et al.'s (2010) effects of power posing on hormonal changes, other manipulations related to power, such as winning a competition, have been shown to jointly impact testosterone and cortisol (e.g. see Hamilton, Carré, Mehta, Olmstead, & Whitaker, 2015). Thus, in the present research, we measured both testosterone and cortisol at two time points, as per Carney et al.'s (2010) original design and tested for both main and interactive effects.

Failed replication attempt and response by Carney et al.

Ranehill et al. (2015) attempted a conceptual replication of Carney et al.'s (2010) findings using a substantially larger sample (N = 200) – with 95% power to detect an effect size equal to that reported by Carney et al. – and a design in which the experimenter was blind to condition. Consistent with the original findings, results revealed a significant effect of power posing on self-reported feelings of power. However, power posing had no significant effect on testosterone or cortisol or any of the three behavioral tasks (risk-taking as per the original study plus risk-taking in the loss domain and willingness to compete).

Based on several methodological differences between these two studies, Carney, Cuddy, and Yap (2015) suggested that the null results reported by Ranehill et al. (2015) might not exclude the possibility that power posing indeed impacts risk-taking and hormone levels. For example, whereas Carney et al. (2010) used an elaborate cover story, Ranehill et al. (2015) informed participants that the study examined how physical position affects hormone levels and behavior. Additionally, the instruction method and the time of the poses varied across the studies. Ranehill et al. (2015) gave their instructions to the participants via a computer (to facilitate blind conditions) and the poses were held for 3 min each. Carney et al.'s (2010) experimenter configured the poses of the participants, which were held for 1 min each. Carney et al.'s (2010) experiment involved a social task (viewing and forming impressions of faces) during the postural manipulation, but Ranehill et al.'s (2015) experiment had participants constructing words as a substitute filler task. Carney et al. (2015) suggest that any one of these methodological differences could potentially account for the two studies' diverging results.

The proposed research

The goal of the current research was twofold. First, we intended to replicate Carney et al.'s (2010) main effects and to test the possibility that the relationship between embodied power and risk-taking is statistically mediated by increases in testosterone. As described below, we used Carney et al.'s (2010) reported effect sizes as a guide for estimating a sufficiently powered sample size. Further, we followed Carney et al.'s (2010) procedures more closely than Ranehill et al. (2015) in their replication attempt. Our second goal was to test the possibility that elevated levels of testosterone increase overconfidence, which in turn may facilitate risk-taking (see Figure 1).

We positioned overconfidence within our proposed model as there is empirical work linking people's sense of inflated confidence to two of Carney et al.'s (2010) three critical variables – power (Fast, Sivanathan, Mayer, & Galinsky, 2012) and risk-taking (Camerer & Lovallo, 1999; Campbell, Goodie, & Foster, 2004; Malmendier & Tate, 2008) – and a theoretical rationale for why we might expect a relationship with testosterone.

Power and overconfidence

Keltner et al.'s (2003) approach/inhibition theory argues that power activates the BAS, increasing people's sensitivity to the possibility of reward. Consistent with this perspective, power manipulations have been shown to increase attention to positive and

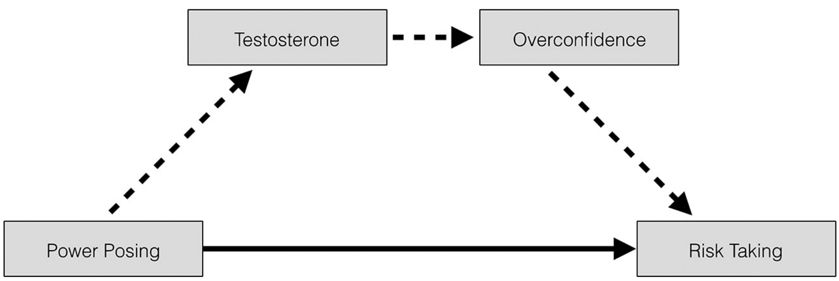

Figure 1. Proposed causal pathway from power to risk-taking via testosterone and overconfidence.

rewarding information (Anderson & Galinsky, 2006; Anderson & Berdahl, 2002), to enhance optimism (Anderson & Galinsky, 2006), to increase perceived control of outcomes (Fast, Gruenfeld, Sivanathan, & Galinsky, 2009), and to orient people toward action (Anderson & Berdahl, 2002; Fast et al., 2009; Galinsky et al., 2003; Magee, Galinsky, & Gruenfeld, 2007). Building from these findings, Fast and colleagues (2012) reported five experiments that used multiple instantiations of power – including manipulations via episodic recall tasks, role assignment, and extant work related power – where they found high power to be associated with overconfident decision-making. Based on these findings, we propose that exaggerated expressions of confidence will emerge as a function of the degree to which high-power poses facilitate a high-power mind-set.

Overconfidence and risk-taking

Risky decisions frequently involve balancing the expected probability of rewards against the costs of failure (e.g. Bernoulli, 1738; Daly & Wilson, 2001; Friedman & Savage, 1948; Mishra, 2014; Real & Caraco, 1986; Rubin & Paul, 1979; Winterhalder, Lu, & Tucker, 1999). In this way, accurate calibration of one's skills, knowledge, and abilities can be functional; confidence matched by ability both increases the likelihood of taking risks and the likelihood of those risks paying off. However, just as confidence tethered to ability increases risky choices, so too does confidence without supporting ability (Camerer & Lovallo, 1999; Campbell et al., 2004; Krueger & Dickson, 1994; Malmendier & Tate, 2008; Miller & Byrnes, 1997). In short, similar to the power findings outlined above, confidence increases the perceived likelihood of success and reduces the perceived likelihood of failure, thereby increasing the attractiveness of risk-taking, whether or not that confidence appropriately matches one's knowledge, skills, and abilities.

It is worth disentangling the effects of confidence/overconfidence with regard to (1) predicting risk-taking and (2) predicting associated outcomes. On the one hand, when the outcome of risk-taking is independent of one's level of knowledge, skill, or ability – such as when one is betting on the flip of a coin – confidence and *over*confidence should be indistinguishable in predicting the win or loss of the coin flip. However, if *over*confidence (though not confidence) reflects a generalized tendency toward self-deceptive self-enhancement (von Hippel & Trivers, 2011), which is accompanied by the perceived ability to shape transparently random outcomes (Langer, 1975), then we might expect *over*confidence, though not accurately calibrated confidence, to predict risk-taking in the form of betting on the flip of a coin, or the roll of a die. In other words, calibrated confidence should be sensitive to context to a greater extent than miscalibrated confidence.

Testosterone and overconfidence

Drawing from Johnson and Fowler (2011), we propose that overconfidence facilitates entry into competitions, as is necessary for the acquisition of two psychosocial variables strongly related to testosterone – status and dominance (Dabbs & Dabbs, 2000; Mazur & Booth, 1998). As overconfidence increases perceptions of the likelihood of success and diminishes the salience of potential failure (Simon et al., 2000), it provides a motivational state well matched to the competitive and risky pursuit of status and dominance. Indeed, Johnson et al. (2006) report a positive association between testosterone and overconfidence in participants' expectations of success during a simulated war game, though this effect did neither emerge within gender nor when gender was controlled for. Nonetheless, no study has examined whether overconfidence might act as an adaptive psychological mechanism that may be modulated in response to fluctuating levels of testosterone, in turn decreasing risk sensitivity and increasing risk-taking. Such was the goal of adding a measure of overconfidence to our attempted replication of Carney et al. (2010).

Method

Participants and overview of procedure

With the exception of overconfidence, which was measured both pre- and post-manipulation, as described below, and a brief question concerning participants' familiarity with the original study, all methodological details and analyses were an exact replication of the methodology described by Carney et al. (2010).[1] As data were collected in the Netherlands, the experimental materials were presented in Dutch.

Carney et al. (2010) report that the difference in testosterone change between power pose and non-power pose participants is medium in size, r = 0.34. Taking this as an estimate of the population effect size, we required 63 participants to have 80% power to detect an effect of power poses (high versus low) on testosterone change. However, given recent calls for replications to target sample sizes 2.5 times larger than the original study (Simonsohn, 2015), we aimed to enroll 110 participants. Enrollment began on 7 March 2016, and continued until 20 May 2016, at which time we had data from a total of 108 participants (M_{age} = 21.48, SD = 2.43). Although we made efforts to recruit an equal number of men and women, the sample was 59% female due to a majority-female subject pool and time constraints on data collection.

Participants were randomly assigned to the high-power pose (N = 53) or low-power-pose (N = 55) condition. Participants were told that the goal of the study was to investigate the science of physiological recordings, with the intention of investigating how placement of electrocardiography above or below the heart may influence heart rate estimates. Participants' bodies were posed by an experimenter into high-power or low-power poses. Each participant stayed in each of two poses for 1 min. Participants' risk-taking was measured with a gambling task. Feelings of power were measured via self-report. Saliva samples were taken immediately before and 17 min after the power poses to measure testosterone and cortisol. Overconfidence was measured before the power poses and after the risk-taking measures and subjective feelings of power questions.[2]

Power poses

Poses were taken directly from Carney et al. (2010). Poses varied on two nonverbal dimensions universally associated with power: expansiveness (i.e. taking up more space or less space) and openness (i.e. keeping limbs open or closed). To configure participants into the poses, the experimenter placed an electrocardiography lead onto the back of each participant's calf and the underside of their nondominant arm and explained, "To test accuracy of physiological responses as a function of sensor placement relative to your heart, you are being put into a certain physical position." The experimenter then manually configured participants' bodies into the poses by lightly touching their arms and legs. As needed, the experimenter provided verbal instructions (e.g. "Keep your feet above heart level by putting them on the desk in front of you."). The experimenter then left the room. Participants were videotaped to ensure that they correctly made and held the poses for 1 min each. While making and holding the poses, participants completed a filler task that consisted of viewing and forming impressions of nine faces.

Measures of overconfidence

Overconfidence was measured via participants' overestimation of their performance on a general knowledge questionnaire (see Appendix 1) (GKQ; Michaiolva, 2010). This overestimation-based operationalization is consistent with a majority of empirical work on overconfidence (e.g. Fischhoff, Slovic, & Lichtenstein, 1977; Kruger & Dunning, 1999; Kruger & Mueller, 2002; Larrick, Burson, & Soll, 2007; Moore & Healy, 2008). We used an adapted version of the previously validated GKQ (Michailova, 2010: Michailova & Katter, 2014), which consisted of the 18 items from the original measure (e.g. *How many days does a hen need to incubate an egg?)* plus 6 additional items. Participants were instructed to choose the correct answer from three alternatives and to provide a number between 33% and 100% indicating their confidence in the accuracy of that answer. The 24 items were split into two sets of 12 items, balancing hard, moderate, and easy items across the two sets. One set was completed at the beginning of the experiment, immediately following acquisition of informed consent, and the second set was completed immediately after the risk-taking task and subjective feelings of power questions. We computed overconfidence by regressing participants' confidence scores for each item onto their accuracy scores for that same item, and saving the standardized residual scores (Anderson, Brion, Moore, & Kennedy, 2012; Cronbach & Furby, 1970). Two of the general knowledge questions were correctly answered by all participants, so we excluded these two questions from analysis as our residual approach could not be implemented due to the absence of any variance on these items. The residual scores were then aggregated for the first and second blocks of overconfidence. Cronbach's alpha for T1 and T2 measures of overconfidence was 0.64 and 0.74, respectively. The two blocks correlated with each other at $r = 0.67$, $p < 0.01$.

Measures of risk-taking and powerful feelings

Following the power posing, participants were presented with a gambling task. They were endowed with €2 and told that they could either keep the money – the safe bet – or roll the die and risk losing the €2 for a potential payoff of €4 (with odds of winning fixed at 50/50, the gamble will be risky but rational). Participants were asked to indicate how "powerful" and "in charge" they felt on a scale of 1 (*not at all*)–4 (*a lot*).

Saliva collection

Testing was scheduled every workday between 12:00 and 17:00 to attenuate the influence of diurnal fluctuations in testosterone. Saliva samples were taken before the power pose manipulation (approximately 10 min after arrival; Time 1) and again 17 min after the power pose manipulation (Time 2).

Standard salivary hormone collection procedures were used (Dickerson & Kemeny, 2004; Schultheiss & Stanton, 2009). Participants were asked to abstain from eating, drinking, smoking, chewing gum, and brushing their teeth for at least 1 h prior to their scheduled session. After providing informed consent, participants were asked to rinse their mouths with water and chew a piece of sugar-free Stimorol gum for 3 min. to stimulate salivation; this procedure yields the least bias compared with passive drool procedures (Dabbs, 1991). Participants provided approximately 2 ml of saliva through a Salimetrics saliva collection aid into a sterile polypropylene microtubule. Samples were immediately frozen to avoid hormone degradation and to precipitate mucins. Samples were packed in dry ice and shipped for analysis to Dresden Lab Service, where they were assayed in duplicate for salivary cortisol and salivary testosterone using a highly sensitive enzyme immunoassay.

Hormone assays

Concentration of alpha-amylase in saliva was measured by an enzyme kinetic method: Saliva was processed on a Genesis RSP8/150 liquid handling system (Tecan, Crailsheim, Germany). First, saliva was diluted 1:625 with double-distilled water by the liquid handling system. About 20 ml of diluted saliva and standard were then transferred into standard transparent 96-well microplates (Roth, Karlsruhe, Germany). Standard was prepared from "Calibrator f.a.s." solution (Roche Diagnostics, Mannheim, Germany) with concentrations of 326, 163, 81.5, 40.75, 20.38, 10.19, and 5.01 U/l alpha-amylase, respectively, and bidest water as zero standard. After that, 80 ml of substrate reagent (*a*-amylase EPS Sys; Roche Diagnostics, Mannheim, Germany) were pipetted into each well using a multichannel pipette. The microplate containing sample and substrate was then warmed to 37°C by incubation in a water bath for 90 s. Immediately afterward, a first interference measurement was obtained
at a wavelength of 405 nm using a standard ELISA reader (Anthos Labtech HT2, Anthos, Krefeld, Germany). The plate was then incubated for another 5 min at 371°C in the water bath, before a second measurement at 405 nm was taken. Increases in absorbance were calculated for unknowns and standards. Increases of absorbance of diluted samples were transformed to alpha-amylase concentrations using a linear regression calculated for each microplate (Graphpad Prism 4.0c for MacOSX,
Graphpad Software, San Diego, CA).

Saliva samples were frozen and stored at −20°C until analysis. After thawing, salivettes were centrifuged at 3,000 rpm for 5 min, which resulted in
a clear supernatant of low viscosity. Salivary concentrations were measured using commercially available chemiluminescence immunoassay with high sensitivity (IBL International, Hamburg, Germany). The intra and interassay coefficients for cortisol were below 8%.

Familiarity check

As Carney et al.'s (2010) finding received substantial media attention, including a widely viewed TED talk delivered by coauthor Amy Cuddy, we checked for participant familiarity with the reported finding via a funnel debriefing. Participants were presented with a screenshot of Amy Cuddy delivering her TED talk and asked (1) if they had previously seen this person and, if so, where and (2) if they had seen this person deliver a TED talk and, if so, what the topic of the talk was (see Appendix 2). Eighteen participants reported familiarity with the TED talk. We included these participants in the primary analyses reported below. However, we also analyzed the data to test for moderation by familiarity with Amy Cuddy's talk and so report these results separately.

Planned analyses

As per Carney et al. (2010), one-way analysis of variance was used to test between condition differences in self-reported power, and risk-taking, as well as post manipulation testosterone (T2), controlling for baseline testosterone (T1), and post manipulation overconfidence (T2), controlling for baseline overconfidence (T1). To control for sex differences in testosterone, we used participant sex as a covariate in all analyses. A combination of regression analyses and bootstrapping (Preacher & Hayes, 2004) was used to test the hypothesized causal pathway from power poses to risk-taking, via testosterone and overconfidence.

Results

Feelings of power and gambling decisions

Participants who held high-power poses reported no stronger feelings of power (M = 2.12, SD = 0.77) compared with participants who held low-power poses (M = 1.95, SD = 0.79), $F(1,105) = 1.35$, $p = 0.25$, $d = 0.226$, $r = 0.113$. Logistic regression revealed that the proportion of participants to choose the risky gamble did not differ across the high-power condition (81.13%) and the low-power condition (89.09%), $z = 1.42$, $p = 0.23$, $\varphi = 0.038$

Testosterone and cortisol

High-power poses (M = 41.11, SD = 35.89) did not result in higher levels of T2 testosterone than low-power posers (M = 47.69, SD = 38.16), $F(1,102) = 0.39$, $p = 0.53$, $d = 0.121$, $r = 0.061$. High power poses (M = 2.07, SD = 1.53) also did not result in lower levels of cortisol compared with low-power poses (M = 2.49, SD = 1.72), $F(1,102) = 0.03$, $p = 0.86$, $d = 0.034$, $r = 0.017$. Logistic regression revealed that post-manipulation testosterone was unrelated to gambling decisions, $z = 0.002$, $p = 0.96$, $\varphi = 0.037$. The interaction between post-manipulation testosterone and cortisol was also unrelated to gambling decisions, $z = 0.02$, $p = 0.90$, $\varphi = 0.037$.

Overconfidence

High-power ($M = 0.14$, $SD = 0.53$) posers did not have higher post-manipulation levels of overconfidence than low-power posers ($M = -0.01$, $SD = 0.51$), $F(1,104) = 0.26$, $p = 0.61$, $d = 0.099$, $r = 0.05$. Post-manipulation testosterone was unrelated to post manipulation overconfidence, $b = 0.00$, 95%CI [–0.00, 0.00], $t(103) = 0.90$, $p = 0.37$, $d = 0.18$.

Indirect effects

Although we observed no main effect of condition on risk-taking, we tested for indirect effects via T2 testosterone and T2 overconfidence, controlling for gender, T1 overconfidence, and T1 testosterone. All confidence intervals overlapped with zero.

Familiarity with power poses

Feelings of power and gambling decisions

Familiarity with Amy Cuddy's TED talk had no main effect on feelings of subjective power, $F(1,103) = 0.03$, $p = 0.86$; nor did familiarity with the talk interact with condition to influence subjective feelings of power, $F(1,103) = 0.22$, $p = 0.64$. Logistic regression also revealed no main effect or interaction on gambling decisions (z's = 0.01, p's = 0.93 and 0.94).

Testosterone and cortisol

We reran our analysis of variance (ANOVA) to test for increases in testosterone in response to power posing, with familiarity included as both a potential main effect and a potential moderator of condition. Although participants familiar with the talk ($M = 36.65$, $SD = 31.96$) had lower testosterone than participants who were unfamiliar with the talk ($M = 46.02$, $SD = 37.94$), $F(1,100) = 3.90$, $p = 0.05$, $d = 0.38$, familiarity did not moderate the (null) effect of power posing on testosterone, $F(1,100) = 1.73$ $p = 0.19$, $d = 0.25$. Applying the same procedure to T2 cortisol revealed no main effect $F(1,100) = 1.40$, $p = 0.24$, $d = 0.23$, and no moderating effect of familiarity, $F(1,100) = 0.68$, $p = 0.41$, $d = 0.16$.

Overconfidence

We observed no main effect, $F(1,102) = 0.29$, $p = 0.59$, $d = 0.10$, and no moderating effect of familiarity, $F(1,102) = 0.10$, $p = 0.75$, $d = 0.06$, on T2 overconfidence. Post-manipulation testosterone remained unrelated to post-manipulation overconfidence after controlling for familiarity, $b = 0.00$, 95%CI [–0.00, 0.00], $t(103) = 0.80$, $p = 0.42$, $d = 0.16$.

Discussion

In contrast to Carney et al. (2010), we find no support for the hypothesis that power posing affects subjective feelings of power or risk-taking. Nor do we find any evidence that power posing changes testosterone, cortisol, or the additionally proposed interaction between testosterone and cortisol. We propose three explanations for the inconsistencies between our results and those reported by Carney et al. (2010). First, our results could reflect a false negative, despite our efforts to conduct a well-powered study. Second, Carney et al.'s (Carney et al., 2010) results could reflect a false positive, as might be suggested by our results and Ranehill et al.'s (Ranehill et al., 2015) results. Third, unknown factors could moderate the effects of power posing on risk-taking and hormone levels. Consider differences in sample characteristics across studies. Carney et al. (2010) sampled US MBA students, and we sampled Dutch psychology undergraduates (notably, Ranehill et al. (2015) sampled Swiss students). These populations could differ in many ways – perhaps in terms of baseline differences in power motivations across Dutch and US students, or differences between MBA students and psychology students – and such factors could moderate the effects of power posing. As Ranehill et al. (2015) suggest, it is as of yet unclear what conditions might be required for power posing to have the effects reported by Carney et al. (2010) – or if they have such effects under any conditions.

Conclusion

In response to Carney et al.'s (Carney et al., 2015) notes on potential confounds in Ranehill et al.'s (Ranehill et al., 2015) failed replication of Carney et al. (2010), our experimental design precisely followed the experimental design and analytic strategy of the original study. We used the same elaborate cover story, we had the experimenter configured the poses of the participants and we had participants hold the poses for one minute each, and we included the same "social task" during the postural manipulation (viewing and forming impressions of faces). Although Carney et al. (2015) highlighted that any one of these methodological differences could potentially account for the two studies' diverging results, we find no evidence to support this alternative explanation.

Notes

1. During preregistration, we stated that we would exactly follow the protocol of Carney et al. (2010), whose procedures were not fully described in the published manuscript. As to remain consistent with the original design, we too measured those. We did not analyze these variables and so do not report them here. All materials and procedures are available for download via Professor Carney's website: http://faculty.haas.berkeley.edu/dana_carney/PRS%20Materials%20-%20to%20replicate.zip
2. Although not ideal for testing our model, which positions overconfidence before risk-taking, we measured T2 overconfidence following the key DV's of Carney et al. (2010) to maintain the integrity of the original design.

Disclosure statement

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Appendices

Appendix 1

(General Knowledge Questionnaire for Assessing Overconfidence)

****

Below you will be presented with some general knowledge questions.

Imagine that you are taking part in a game, like "Trivial Pursuit" or "Who wants to be a Millionaire?," and you have to choose the correct answer from the three given alternatives.

(1) Please select ONLY ONE of the three given answers. Only one of them is correct.
(2) When you have made your choice and have selected your answer, we would like to know how sure/confident you are that your answer is correct.

Since there are three alternative answers and only one of them is correct you have a 33% chance of giving a correct answer. Therefore, 33% means that you are guessing and do not know the correct answer, and 100% corresponds to absolute certainty.

You can choose any number between 33% and 100% to indicate your confidence that your answer is correct. Enter your confidence for every answer after every test item.

*****

1. Which of the following is known for being an instant camera?		
Canon Camera	**Polaroid Camera**	Minolta Camera
How confident are you that your answer is correct?		
33–100%		
2. Where do flounders mostly live?		
in coral reefs	**dug into the ground**	in reeds
How confident are you that your answer is correct?		
33–100%		
3. What do roll mops mostly consist of?		
herring fillet	pork	salmon fillet
How confident are you that your answer is correct?		
33–100%		
4. Which country does the Nobel Prize winner in Literature Gabriel García Márquez come from?		
Colombia	Spain	Venezuela
How confident are you that your answer is correct?		
33–100%		
5. Which style movement does Anacreontics belong to?		
Rococo	Romanticism	Realism
How confident are you that your answer is correct?		
33–100%		
6. What is meant by horripilation?		
itch	**goose bumps**	muscle pain
How confident are you that your answer is correct?		
33–100%		
7. How many letters does the Russian alphabet consist of?		
40	**33**	26
How confident are you that your answer is correct?		
33–100%		
8. "Tosca" is an opera from?		
G. Puccini	G. Verdi	A. Vivaldi
How confident are you that your answer is correct?		
33–100%		
9. What is the name of the Greek Goddess of wisdom?		
Pallas Athena	Nike	Penelope
How confident are you that your answer is correct?		
33–100%		
10. What is the most abundant metal on Earth?		

(*Continued*)

(Continued).

Iron	**aluminum**	copper
How confident are you that your answer is correct?		
33–100%		
11. What is the word for a person who lacks knowledge?		
ignatius	**ignorant**	ideologue
How confident are you that your answer is correct?		
33–100%		
12. Who was the first to fly in an airship around the Eiffel tower?		
Santos-Dumont	Count Zeppelin	Saint-Exupery
How confident are you that your answer is correct?		
33–100%		
13. What is the snow shelter of Eskimos called?		
wigwam	**igloo**	tipi
How confident are you that your answer is correct?		
33–100%		
14. Which animal digs with its teeth?		
maned wolf	mole	bilby
How confident are you that your answer is correct?		
33–100%		
15. How many days does a hen's egg incubate for?		
21 days	28 days	14 days
How confident are you that your answer is correct?		
33–100%		
16. What is ascorbic acid?		
apple vinegar	**vitamin C**	vitamin B
How confident are you that your answer is correct?		
33–100%		
17. What is the middle color of the rainbow?		
Blue	yellow	**green**
How confident are you that your answer is correct?		
33–100%		
18. What is the whitish coating you sometimes see on chocolate called?		
bloom	glycerol	mold
How confident are you that your answer is correct?		
33–100%		
19. Which language does the concept "Fata Morgana" come from?		
Italian	Arabic	Swahili
How confident are you that your answer is correct?		
33–100%		
20. What is inflamed when one has gingivitis?		
toe tissue	eye tissue	**gum tissue**
How confident are you that your answer is correct?		
33–100%		
21. What does one call the vocal organ of birds?		
bellows	**syrinx**	sonorant
How confident are you that your answer is correct?		
33–100%		
22. Which of the following is a hot chili sauce?		
Tabasco	Curacao	Macao
How confident are you that your answer is correct?		
33–100%		
23. What is the fasting month in Islam called?		
Sharia	**Ramadan**	Imam
How confident are you that your answer is correct?		
33–100%		
24. Which enterprise does Bill Gates belong to?		
Intel	**Microsoft**	Dell
How confident are you that your answer is correct?		
33–100%		

Appendix 2

(Image from Cuddy's TED talk for Funnel Debriefing to Assess Familiarity with the Power Posing Effect)

Using a "funnel debriefing" format, participants will be asked:

(1) Have you seen this person before? YES/NO
(2) Where have you seen this person before? OPEN RESPONSE
(3) Have you seen this person deliver a TED talk? YES/NO
(4) What was the topic of the TED talk you saw this person deliver? OPEN RESPONSE

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Real and imagined power poses: is the physical experience necessary after all?

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ABSTRACT

Previous research investigated the effects of power poses at the behavioral, subjective, and neuroendocrine level. However, it is not clear whether the same effects would be obtained also by just imagining, rather than adopting, a power pose. We planned to investigate this question by asking 200 participants to either perform or imagine a constrictive or an expansive body posture during 2 min and then measure the effect on a gambling decision task and on felt power. We followed a sequential analysis procedure by running the first 100 participants in the performed posture condition in order to check the presence of the power posing effect. Because no effect of power poses on gambling decision or on felt power was found, we ran the remaining 100 participants also in the performed instead of the imagined condition. The results after running 200 participants confirmed that power poses did not affect gambling decision. However, participants felt more powerful after adopting an expansive pose compared to a constrictive pose. Exploratory analyses found that this effect was mainly driven by male participants. In addition, participants rated themselves as being more able to adopt the expansive body posture and they reported to put more effort in adopting the restrictive body posture. Overall, our results indicate that the effect of power poses on behavior might not be as widespread as previously thought of and suggest that moderators should be investigated carefully in future research.

KEYWORDS

Power pose; gambling decision; performed pose; imagined pose; felt power

In the last decades, a wealth of research has investigated how physical experience can shape psychological states, a concept known as embodiment (see Meier, Schnall, Schwarz, & Bargh, 2012 for a review). For instance, judgments about morality are influenced by the experience of cleanliness (Schnall, Benton, & Harvey, 2008) and trust in others is influenced by physical warmth (Williams & Bargh, 2008). The common frame of different examples of embodiment is that the body state is represented in our mind in a more abstract or metaphorical way and this representation influences cognition, behavior, affect, and thoughts.

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Whereas the literature on the effects of embodiment is rich, the research investigating the mediators and the moderators of this process is scarce. A question that has not received much attention by researchers so far concerns how the bodily information triggers specific psychological states. Can the physical experience be bypassed? In other words, does the sensorimotor information need to be real (i.e. physically performed) or can it be just imagined (i.e. mentally performed)? We will investigate this question by focusing on a particularly debated topic in the embodiment domain, namely the effects of power poses on subjective feelings and behavior.

Early evidence about the effect of power poses comes from Carney, Cuddy, and Yap (2010). In their study, participants adopting expansive poses during 2 min were more likely to take a risky gamble, felt more powerful, had increased levels of testosterone, and decreased levels of cortisol compared to participants adopting a constrictive pose. Since this pioneer study, the research focusing on power poses has proliferated. Power poses foster effects that are typically associated with having power or feeling powerful, such as dishonest behavior (Yap, Wazlawek, Lucas, Cuddy, & Carney, 2013) or increased performance in social or competitive tasks (Cuddy, Wilmuth, Yap, & Carney, 2015), to name just a few. Overall, researchers often proposed power poses as a panacea because of their beneficial effect in different types of tasks. Yet some studies have questioned the validity of the results stemming from power poses (Ranehill et al., 2015) and investigated the boundary conditions of the power pose effects (Cesario & McDonald, 2013). In the Ranehill et al. study, participants adopting a constrictive or an expansive pose did not show any difference at the behavioral and neuroendocrine level. Cesario and McDonald failed to replicate the findings by Carney et al. when the social component of the task was not made relevant and when the participants were assigned to a role that provided a more salient source of information to guide behavior than power poses (e.g. being frisked by the police). These findings stress the importance of the situation in which the expansive poses are adopted and suggest that more investigation is needed to understand under which conditions expansive poses become power poses and trigger the effects reported by Carney et al. In the present study, we address the novel question of whether the effect on behavior and subjective feelings can be triggered by simply imagined and not actually performed expansive poses.

Studies on mental imagery show that the mere imagination of body states or movements can prime specific concepts. Wilson and Gibbs (2007) found that both performing and imagining a movement (e.g. pushing) made comprehension of a related metaphor (e.g. pushing an argument) faster. In another study, children learned a text more deeply if they either manipulated or imagined themselves manipulating an object that was congruent with the story (Glenberg, Gutierrez, Levin, Japuntich, & Kaschak, 2004). Not only can mental imagery prime concepts, but it can also affect behavior and performance. Mere imagination of specific body movements used to complete a task can improve performance, for instance in music (Keller, 2012) and sport (Murphy, 1990). A possible explanation of these findings is that performing and imagining an action partially share the same neural substrates. As an example, sensorimotor activations found when imagining self-body rotations are similar to those found in physically performed rotations (Creem et al., 2001). Taken together, mental imagery of body movements has similar behavioral and neural effects as actually performing the same movements. Therefore, we hypothesize that the imagination of adopting an expansive or a constrictive pose has the same effects on behavior and subjective feelings as physically adopting the same pose. According to Barsalou (1999), embodiment is based on a neural reactivation of a sensorimotor experience that can prime concepts. We argue that this reactivation does not need to have a physical input, but that it can be generated in a top-down way. If that would be the case, the importance of actually striking expansive poses before a task to improve performance, as suggested by Carney et al. (2010), should be reconsidered. Indeed, if the effect of imagined and performed expansive poses on behavior is equivalent, then such expansive poses should be considered similar to other methods typically used by researchers to prime power or activate the concept of power, as reporting an autobiographical event related to power (Galinsky, Gruenfeld, & Magee, 2003; Schmid & Schmid Mast, 2013) or completing a word search task (Chen, Lee-Chai, & Bargh, 2001; Study 1).

In the present study, we ask participants to either adopt or imagine adopting an expansive or a constrictive body posture. Subsequently, we involve them in a gambling decision (Carney et al., 2010) and measure their felt power. We hypothesize that participants adopting an expansive body posture are more likely to take a risky gamble and feel more powerful than participants adopting a constrictive body posture both in the imagined and the performed conditions. Because we question whether the physical experience is a necessary condition to trigger an effect at the psychological level, the importance of the present study does not only concern research on power poses but can be extended to the whole domain of embodiment.

Method

Participants

We recruited 233 participants through a participant pool at the University of Lausanne and at the Ecole Polytechnique Fédérale de Lausanne (EPFL) in Switzerland. Participants were either bachelor or master students majoring in different domains and were fluent in French. Twenty-five participants were discarded from the subsequent analyses because they were not able to keep the assigned position for 2 min (see the criteria below) and two were excluded because they failed to indicate the position of the red dots. One participant was discarded because of a technical failure of the computer showing the facial stimuli. Five participants were not taken into account because they reported having heard about the power pose manipulation before (e.g. from TED talks) and could guess the purpose of the study. The exclusion of 33 participants from subsequent analyses resulted in a total of 200 participants (101 females and 99 males; M_{age} = 20.83, SD_{age} = 1.97). Participants were remunerated according to their decision and the outcome of the gambling task (1, 10, or 20 CHF).

Experimental design

We planned to use a 2 x 2 design, with the variables body posture (expansive vs. constrictive) and prime type (performed vs. imagined) as independent factors and the count of people who gambled their money as dependent measure. Based on the results reported in Carney et al. (2010), we used G*Power (Faul, Erdfelder, Lang, & Buchner,

2007) to calculate Cohen's $w = .030$. Using this effect size, an a priori power of 0.95 and an $\alpha = 0.025$ (Bonferroni correction for the two planned comparisons), we calculated that 165 participants were the required sample size. We decided to collect data from 200 participants to increase the reliability of the results (50 participants per cell).

However, because the study by Carney et al. (2010) was underpowered (20 and 22 participants respectively in the constrictive and expansive body posture conditions) and the effect size reported in that study is most likely biased, as shown by Ranehill et al. (2015), we decided to perform a sequential analysis (Lakens & Evers, 2014). This means running the performed condition first (both in the expansive and constrictive body posture) and investigating whether the finding reported by Carney et al. can be replicated through a chi-square test and only if the original effect is replicated, running the imagined condition. Therefore, after having run 100 participants in the performed condition, we checked whether the original power posing effect on risk taking emerged. This was not the case, which is why we ran the remaining 100 participants also in the performed condition. In other words, we never ran the imagined body posture condition because we were not able to replicate the original power posing effect.

In sum, participants were randomly assigned to either the constrictive (101 participants) or expansive (99 participants) body posture, with roughly the same number of male and female participants in both conditions (50 women in the constrictive body posture and 51 in the expansive body posture condition).

Procedure

Upon arrival, participants were welcomed and requested to sign an informed consent form. Participants were handled by one of the three experimenters who were blind to the aim of the study and the condition the participants were assigned to. Participants were told that the study consisted of two parts, the first focusing on body postures and first impressions and the second on gambling. In the first part, the experimenter left the room after giving general instructions about the task to the participant. Because the experimenters were blind to the experimental condition, they did not configure participants as in other power pose studies (Carney et al., 2010) and were not aware of the pose that participants adopted. All visual stimuli were presented through a projector on a wall of the experimental room. Upon the exit of the experimenter from the room, a cue presented for 10 s indicating to the participants whether they had to perform or to imagine the body pose indicated subsequently (the imagined cue condition was never presented because of the outcome of the sequential analysis). A 3 x 2 m image was projected on the wall (Figure 1). The image showed a grid of eight adult faces with a neutral expression that enhanced the social relevance of the task, as was the case with other studies on power posing (Carney et al. 2010; Cesario & McDonald, 2013). At the center of the image was a picture of a person adopting the body position that the participant had to use (reference pose). The image of the reference pose was presented only during the initial 20 s (preparation phase) but participants had to maintain the body pose during the entire 2 min. The expansive and constrictive body posture models have been used previously in power pose studies (Yap et al., 2013) and are depicted in Figure 2.

Figure 1. The image that was displayed to participants during the power pose manipulation. At the center of the image and only during the initial 20 s, a model displayed the pose to adopt.

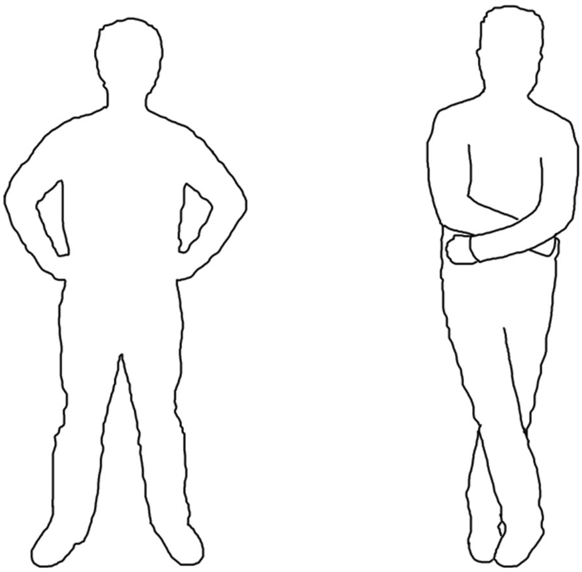

Figure 2. An illustration of the expansive (on the left) and contractive (on the right) body posture shown to participants.

Participants were videotaped in order for the researchers to be able to control their actual body position. To ensure that participants focused their attention on the projected faces, a red dot was superimposed twice for 2 s on the overall image (after 1 min and at the end of the 2 min). The dot was randomly presented (through Eprime) either at the top (at 150 cm from the floor) or at the bottom (at 50 cm from the floor) of the image and participants were asked to verbally report immediately after each dot appeared, whether the dot was above or below the actual physical position of their hands.

At the end of the 2 min, the experimenter entered the room (still blind to the experimental condition the participant was in) and instructed the participant about the gambling task. The participant was given 10 CHF (roughly equivalent to 10 US$) and, as has been done in other studies (Cesario & McDonald, 2013), had the option of keeping it without gambling or else of choosing three numbers from 1 to 6 and rolling a die once with a 50% probability of losing 9 CHF (if they did not roll the chosen number) and a 50% probability of having a payoff of 20 CHF (if they rolled the chosen number). The experimenter acted out the gambling possibilities by putting on the table the 10 CHF bill that participants could keep, showing them the second bill they could take if they won the gamble, and the 1 CHF coin they would receive if they lose (the exact same procedure as used in Carney et al., according to a personal communication from Dana Carney, 20 February 2016).

Following the gambling task, participants were asked to fill in a short online questionnaire using Qualtrics. First, they rated on a scale from 1 (*not at all*) to 4 (*a lot*) how powerful, dominant, submissive, powerless, and in charge they felt, together with seven distractor items (e.g. discouraged, happy). These four items were combined to create a composite variable (M = 2.75, SD = 0.50, Cronbach's *alpha* = .59). Because the alpha was lower than .70, which is a commonly accepted value, we decided to discard the composite measure and used instead just the item "powerful" for our analysis (M = 2.12, SD = 0.90). The same single item was used by Ranehill et al. (2015) to measure felt power.

Moreover, participants rated their ability to hold the pose (M = 4.26, SD = 0.67) on a scale from 1 (*I was not able to hold the pose easily*) to 5 (*I was able to hold the pose easily*) and they indicated how much effort they put into holding the power pose (M = 2.28, SD = 1.18) on a scale from 1 (*I did not put in a lot of effort*) to 5 (*I put in a lot of effort*). At the end of the experimental procedure, participants were asked to guess the aim of the study and to report whether they are familiar with the power pose studies (i.e. either they participated in a previous study or heard about other studies, for instance from TED talks). As mentioned earlier, five participants were excluded because they were familiar with the power pose studies and could therefore guess the aim of the study. Finally, participants were remunerated according to the outcome of the gambling task and were thanked.

A research assistant, blind to the aim of the study, watched the videotapes of the participants during posing (expansive or constrictive) and was instructed to code for significant and relatively long (i.e. longer than 5 s) changes from the posture they were instructed to take. Examples of significant changes are leaning backward in the expansive posture condition and uncrossing the legs in the constrictive posture condition. In addition, the research assistant checked whether participants were able to correctly indicate the position of the red dot during the power posing task. These manipulation checks resulted in the exclusion of 27 participants from subsequent analyses, as described above.

Results

Pre-registered analysis: gambling decision and felt power

We investigated whether body posture had an influence on the gambling decision using a chi-square test. Body posture did not have a significant effect on the gambling decision, χ^2 (1, N = 200) = 0.25, p = .620, ϕ = .04. Indeed, 67.7% of participants in the expansive and 64.4% of participants in the constrictive body posture decided to gamble their money.

Moreover, we investigated the effect of body posture on felt power using a t-test. Participants adopting an expansive body posture (M = 2.24, SD = 0.93) felt significantly more powerful than participant in the constrictive body posture (M = 1.99, SD = 0.85), t(195.99) = 2.00, p = .047, 95% CI [0.004, 0.501], Cohen's d = 0.28.

Exploratory analysis

We investigated whether participant sex might influence the results reported above concerning gambling decision and felt power. We performed a log linear analysis with sex (male = 1, female = 2), body posture, and gambling decision as main factors. The interaction between sex, body posture, and gambling decision did not have a significant influence on gambling frequency, parameter *estimate* = -.04, Z = 0.52, p = .603. The interaction between sex and gambling was significant, parameter *estimate* = .17, Z = 2.23, p = .026. Overall, more male participants decided to gamble (74%) compared to female participants (58%).

In terms of felt power, we conducted a two-way ANOVA with body posture and participant sex as factors. We found a significant main effect of sex, F(1, 196) = 8.29, p = .004, 95% CI [−0.60, −0.11], *partial* η^2 = .041. Male participants (M = 2.29, SD = 0.09) felt more powerful than female participants (M = 1.94, SD = 0.08). The effect of posture was significant as well, F(1, 196) = 4.27, p = .040, 95% CI [0.01, 0.50], *partial* η^2 = .021 (see above for the means). Moreover, the interaction between sex and body posture was marginally significant, F(1, 196) = 3.08, p = .081, *partial* η^2 = .015. Using a post hoc contrast, we found that male participants adopting an expansive (M = −2.53, SD = −0.14) body posture felt significantly more powerful than male participants adopting a constrictive (M = −2.06, SD = −0.12) body posture, p = .008, 95% CI [0.13, 0.82]. We found no significant effect for female participants adopting expansive (M = 1.96, SD = 0.11) and constrictive (M = 1.92, SD = 0.12) body postures, p = .825, 95% CI [−0.30, 0.38].

Moreover, we investigated the ability and the effort participants put into adopting an expansive vs. constrictive body posture through two -way ANOVA's using sex and body posture as factors. In terms of ability, participants reported being more able to adopt the expansive (M = 4.46, SD = 0.60) compared to the constrictive (M = 4.05, SD = 0.67) body posture, F(1,196) = 21.39, p < .001, 95% CI [0.24, 0.59], *partial* η^2 = 0.10. Participant sex, F(1,196) = 1.12, p = .291, 95% CI [−0.27, 0.08], *partial* η^2 = 0.01, and the interaction between sex and body posture, F(1,196) < 1, p = .957, 95% CI for females [0.17, 0.67], 95% CI for males [0.16, 0.66], *partial* η^2 < 0.001, were not significant.

In terms of effort, participants reported putting more effort into adopting the constrictive (M = 2.54, SD = 0.12) compared to the expansive (M = 2.00, SD = 0.22) body posture, F(1,196) = 11.74, p < .001, 95% CI [−0.87, −0.23], *partial* η^2 = 0.06. There was no significant sex difference, F(1,196) < 1, p = .760, 95% CI [−0.27, 0.37], *partial* η^2 < 0.001. The interaction between sex and body posture was significant, F(1,196) = 7.41, p = .007,

Table 1. Correlations between gambling decision, felt power, ability and effort.

	Overall			Females			Males		
	1	2	3	1	2	3	1	2	3
1. Gambling decision	–			–			–		
2. Felt power	−.04	–		−.11	–		−.03	–	
3. Ability	−.01	.18*	–	−.04	.21*	–	−.01	.13	–
4. Effort	−.09	.04	−.21**	.16	.21*	−.14	−.01	−.12	−.31**

* $p < .05$, ** $p < .01$. Gambling decision was dummy coded (0 = no gambling, 1 = gambling).

partial $\eta^2 = 0.04$. Men reported putting more effort into adopting the constrictive ($M = 2.74$, $SD = 1.19$) compared to the expansive ($M = 1.76$, $SD = 0.88$) body posture, $p < .001$, 95% CI [−1.43, −0.54]. For women, there was no significant difference in effort between the constrictive ($M = 2.35$, $SD = 1.13$) and the expansive ($M = 2.24$, $SD = 1.29$) body posture, $p = .617$, 95% CI [−0.56, 0.33].

We analyzed the overall correlations between gambling decision, felt power, ability and effort and the same correlations separately for men and women. Results are reported in Table 1. Ability and effort are overall inversely correlated and particularly so in male participants.

Discussion

We planned to investigate the effect of performed and imagined body postures on gambling decision. We decided to first try to replicate the original effect found by Carney et al. (2010) for performed body postures and then potentially run the imagined body posture condition (sequential analysis). After running 100 participants, we could not replicate the effect found by Carney et al. for performed body posture on gambling decision. We then opted for running another 100 participants in the same condition in order to investigate with more statistical power the potential effect of body posture on gambling decision. We found that even after running 200 participants, body posture still had no significant effect on gambling decision. Moreover, the effect size for the gambling decision is small ($\phi = .04$) according to the classification by Cohen (1988). These results are important because they call for more attention on the topic of the effect of power poses. Whereas several studies published in the last years reported a significant effect of power poses at the behavioral, neuroendocrine, and emotional level (Carney et al., 2010; Bohns & Wiltermuth, 2012; Huang, Galinsky, Gruenfeld, & Guillory, 2011; Park, Streamer, Huang, & Galinsky, 2013), recently some studies have questioned whether a real effect of power poses on behavior and hormones really exists (Ranehill et al., 2015) or have focused on the moderators of the effect of power poses (Cesario & McDonald, 2013).

In agreement with other studies on power poses (Carney et al., 2010; Fischer, Fischer, Englich, Aydin, & Frey, 2011; Park et al., 2013; Ranehill et al., 2015), we found that participants adopting an expansive posture felt more powerful than those adopting a constrictive posture. However, feeling more powerful did not in turn impact behavior (gambling decision), which is in line with the findings of Ranehill et al. (2015) study on both males and females. One possibility is that the effect of power poses is not strong enough to influence behavior, although it might influence affect (Welker, Oberleitner, Cain, & Carré, 2013) and felt power.

Our results in terms of gambling decision match those of Ranehill et al. (2015). Carney, Cuddy, and Yap (2015) pointed out that the differences in findings between their original study and that of Ranehill et al. might be due to the differences in the experimental procedure. Given that the behavioral outcome of our study is in line with that of Ranehill et al., the common aspects in the procedure of these two studies, which differ from that of Carney et al. (2010), might have originated the null finding. First, in both studies, the experimenters were not aware of which experimental condition the participants were assigned to. This is an important difference compared to Carney et al. study, because the experimenter's awareness of the experimental condition might have biased participants' gambling decision through verbal and nonverbal cues of the experimenter. In past research, similar criticism has been applied to studies using priming by showing that the effect of priming disappears when the experimenter is blind to the experimental conditions the participants are assigned to (Doyen, Klein, Pichon, & Cleeremans, 2012). Future research might explicitly investigate this question by comparing conditions in which the experimenter is aware vs. unaware. Furthermore, in both Ranehill et al. and our study, the experimenter did not configure participants' poses manually. It is possible that physically touching participants to show the right position they have to adopt, as it was done in Carney et al. study, increases the social component of the power posing task, which is a moderator of the effect of power poses (Cesario & McDonald, 2013). Increasing the social component might provide a framework to the participants to interpret the body posture as a cue of social power. Finally, in Ranehill et al. and our studies, participants were recruited in Switzerland. Carney et al. (2015) argued that culture might be a moderator of the effect of power pose. However, we chose body postures that seemed to be in agreement with the norms of the Swiss culture. In particular, we avoided those postures that seemed to be perceived differently in different cultures, such as putting the feet on a desk (Park et al., 2013).

Carney et al. (2015) hypothesized that participant sex could be a moderator of the effects of power poses. Indeed, in our study, we found that participant sex affected felt power but not gambling decision. Men felt more powerful after having adopted an expansive as compared to a constrictive body posture but the same pattern was not found in women. To our knowledge, other studies on power poses did not report sex differences at the behavioral, neuroendocrine, or emotional level. It is possible that adopting an expansive posture was more effective in male participants because it might come more naturally to them given that men show more expansive postures than women in general (Knapp & Hall, 2014) and that expansive postures are related to being more dominant (Hall, Coats, & LeBeau, 2005). This might have triggered feeling more powerful in men only when adopting an expansive posture. Moreover, men showing the expansive posture are congruent with the stereotypical role of men as more dominant (Feingold, 1998). Our results show that men put less effort in adopting the expansive body posture which might be an indication that this posture comes more easily to them than to women. In line with the idea that some postures are more easily adopted by men because they correspond to the male stereotype, Schubert and Koole (2009) found that making a fist (i.e. a sign of dominance) made male but not female participants associate themselves more strongly with power.

Participants reported being less able and putting more effort into the posing when adopting the constrictive body posture compared to the expansive body posture. It might be that this difference is responsible for the absence of behavioral effects of body postures. Indeed, putting more effort in adopting the constrictive body posture might have made participants focus less on their posture and this in turn might have decreased the effect of adopting it.

We measured felt power after the gambling decision, in line with the procedure adopted by other studies on power posing (Carney et al., 2010; Ranehill et al., 2015). Whereas this procedure is useful in order to avoid making the concept of power salient for participants, it is possible that the outcome of the gambling decision had more influence on felt power than the body posture. One possibility for future studies might be to measure felt power before the gambling decision by using an implicit measure.

In sum, in the present study, we could not replicate the original effect of power poses on gambling decision found by Carney et al. (2010). This result stresses the importance of more research to understand the boundary conditions under which power poses have an effect on people's behavior. In the last few years, research on power poses has attracted the attention of the scientific community and the media. Even though power poses might influence behavior in specific situations, our results suggest that the effect does not seem to be as widespread and easy to obtain as it was previously thought.

Disclosure statement

No potential conflict of interest was reported by the authors.

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Benefits of power posing: effects on dominance and social sensitivity

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ABSTRACT
The current study attempted to replicate results by Cuddy, Wilmuth, Yap, and Carney (2015) who showed that adopting high-power poses prior to a demanding job interview improved dominant behavior and overall hireability judgments. We extended this approach by adding social sensitivity as a second important dimension of social competences. We tested the following hypotheses: (1) Power posing increases behavioral dominance indicators (2) Power posing strengthens behavioral indicators of social sensitivity. We also attempted to replicate results by Cuddy and colleagues who demonstrated that the effects of the power-posing manipulation on hireability judgments were mediated by behavioral dominance indicators. Additionally, we hypothesized that hireability judgments are independently predicted by indicators of dominance and social sensitivity. Results failed to replicate the findings by Cuddy and colleagues (2015). Power posing had no significant main effects on behavioral indicators of dominance and social sensitivity. As expected, hireability judgments were independently predicted by dominance and social sensitivity.

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Introduction

Acting self-confidently is highly beneficial for leading businesses (Van Zant & Moore, 2013), dating partners (Cunningham, 1989), or during job interviews (Isaacson, Isaacson & Kennedy, 2004). Self-confidence is one side of the medal when performing to one's full potential in work life and beyond. The other side is social sensitivity which can be defined as an empathic ability to correctly understand others' feelings and thoughts and adapt well in social situations (Bender, Walia, Kambhampaty, Nygard, & Nygard, 2012). Socially sensitive individuals have happier relationships (Cohen, Schulz, Weiss, & Waldinger, 2012), are happier in their jobs (Yirik & Ören, 2014), and show better overall job performance (e.g. Cox, 2013; Sadri, 2011). The current study will reexamine the potential of power poses to increase dominance and self-confidence and extend this approach by adding social sensitivity as a second important dimension of social competences.

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Cuddy, Wilmuth, Yap, and Carney (2015) observed participants in a demanding job interview context and let them adopt either self-confident (high-power poses) or self-insecure body postures (low-power poses) shortly before a videotaped self-presentation. Participants in the high-power poses condition showed better performance during their self-presentations in the stressful interview and reported stronger feelings of power than participants in the low-power poses condition. Effects of high-power poses may be explained physiologically by an increase of blood serum levels of testosterone and a decrease of cortisol levels (Carney, Cuddy, & Yap, 2010). A moderate rise of testosterone may be associated with stronger performance during stressful situations and prior research has linked higher dominance to high testosterone levels, especially in individuals with low cortisol levels (Mehta & Josephs, 2010). Other researchers found that increased testosterone levels are also associated with improved achievement motivation and high assertiveness (Yildirim & Derksen, 2012). Individuals with both high testosterone and low cortisol levels are found to be more likely to occupy high-status positions in social hierarchies (Sherman, Lerner, Josephs, Renshon, & Gross, 2015). Importantly, high levels of testosterone were not only associated with self-assertive and dominant behaviors. Individuals with increased testosterone levels also show fairer behavioral interactions and more fair play (Eisenegger, 2010). Thus, testosterone levels may be linked with social competences in a broader and more general way. The full spectrum of social competences is gaining more and more importance for psychology as well as management researchers and practitioners (Weisinger, 1998). Social competences are not only an important predictor of job success (Wong & Law, 2002) but also play a crucial role in job selection procedures (Hunter & Hunter, 1984). In many modern assessment centers, social competences are a central aspect of hiring criteria.

Social competence may be defined as the capability to interact successfully in social situations and can be conceptualized as divided into the two independent dimensions of dominance and social sensitivity (e.g. Thorndike, 1920). While Cuddy and colleagues (2015) primarily focused on the dominance dimension of social competences, we want to extend this approach and also include the sensitivity dimension in our analyses. Dominance and social sensitivity can be conceptualized as personality traits (Greenspan & Granfield, 1992) but also as situation-specific state variables (Petrides, Furnham, & Mavroveli, 2007). While dominant persons are often judged as higher in social competences (Anderson & Kilduff, 2009), several studies suggested that the dominance dimension of social competences is not the only predictor of effective leadership. Effective leadership was associated with a combination of both aspects of social competences (Santora, 2007), and group intelligence was shown to be closely dependent on the social sensitivity of the members of a group (Wooley, 2011). Socially sensitive leaders are able to create a more satisfying job environment (e.g. Burden, 2015; Chiniara & Bentein, 2016). Successful leaders often act in a socially sensitive way (Hewertson, 2012). Leaders who are rated higher in social sensitivity by their coworkers are also judged as performing better by their bosses (Sadri, 2011). People who are assigned to high-power leadership positions increase their levels of social sensitivity (Schmid Mast, Jonas, & Hall, 2009). Empathic accuracy in face to face interactions, the ability to judge the emotions of a stranger, and trait measures of prosocial behavior are greater in individuals who occupy higher-power positions in their company (Côté et al., 2011). These findings suggest that high levels of social sensitivity can be strongly beneficial not only to the work environment but also expand to subjective well-being. The main target of the current research was to explore whether power poses have positive effects not only on the dominance dimension of social competences but also on the social sensitivity dimension. While dominant behaviors can be strengthened by high-power poses, our current research was also interested in exploring situational contexts that foster or impede socially sensitive behaviors.

While power poses foster dominance behavior, they may also have adverse effects on social sensitivity. At least in self-report questionnaires, dominance was linked to negative consequences such as higher levels of aggression. A longitudinal study using self-ratings showed negative cross-sectional and longitudinal relationships between dominance and empathy (Sidanius, Kteily, Sheehy-Skeffington, Ho, Sibley, & Duriez, 2013). Similarly, there are possible trade offs between the impression of being assertive and competent versus socially warm but incompetent (Cuddy, Fiske, & Glick, 2008). Especially dominant females may be judged as less socially competent as compared with equally dominant men (Brescoll, 2012). However, taking into consideration that power and leadership can be also closely linked to social sensitivity (Schmid Mast et al., 2009), we expect positive outcomes of power poses not only on indicators of behavioral dominance but also on social sensitivity.

Hypotheses

In our study, we tested the following hypotheses. Hypotheses 1, 3, and 5 represent a direct replication of Cuddy and colleagues (2015). The other hypotheses refer to an extension in our approach including socially sensitive behaviors.

A) Hypotheses concerning the manipulation of power poses prior to the job interview.

(1) High- versus low-power posing increases behavioral dominance indicators during the job interview (replication of the results by Cuddy et al., 2015).
(2) High- versus low-power posing increases behavioral indicators of social sensitivity during the job interview.
(3) High- versus low-power posing increases hireability judgments during the job interview (replication of Cuddy et al., 2015).

B) Hypotheses concerning the prediction of hireability judgments (regression analyses).

(4) We expect hireability judgments to be independently predicted by behavioral indicators of dominance and sensitivity.
(5) We expect effects of the power-posing manipulation on hireability judgments to be mediated by behavioral dominance indicators (replication of Cuddy et al., 2015).

Methods

Participants

For testing our core hypotheses (Hypotheses 1–3; see above), we conducted one-tailed independent sample *t*-tests with behavioral dominance indicators, behavioral social sensitivity indicators, and hireability judgements as the dependent variables and power pose (high versus low) as the independent variable. In Cuddy et al.'s (2015) study, the manipulation of power poses showed a medium to large effect (d = .68) on hireability judgments. According to G*Power (www.gpower.hhu.de), the required total sample size was calculated as N = 56 for d = .68, α = .05, 1-β = .80. Following suggestions by Simonsohn (2015) recommending the use of a 2.5-fold amplified sample in replication studies, we pursued a sample size of N = 200 participants. We collected data for a total of 212 participants, because 5 participants requested to erase their videos after their interviews and 7 participants did not hold the high- or low-power pose for the entire 5 min. This resulted in the intended total sample size of 200 participants. While 133 of the participants were female (66 in the low and 67 in the high-power pose condition), 67 were male (33 in the low and 34 in the high-power pose condition. We recruited participants between the age of 18 and 51 (M = 24.46, SD = 4.78) who had at least a high school degree. Most of the participants were students of various Berlin universities. They received € 15 for their participation.

Behavioral codings

The two coders were condition-blind to all behavioral ratings. Before the ratings, they watched four anchor videos of Cuddy et al.'s (2015) study in order to be familiarized with prototypically high and low behavioral indicators of dominance and social sensitivity. For the final rating, both raters were instructed to watch the full length of each video interview and give their ratings after the entire 5 min on a paper pencil sheet. Interrater reliability was satisfactory for all ratings (i.e. Cronbach's alpha at least α = .70).

To investigate behavioral dominance indicators, we used the original 7-point scale ratings by Cuddy et al. (2015) and translated them into German using backward translation. Overall performance was measured by the question "Overall, how good was the interview?" and was answered on a scale from 1 = awful to 7 = amazing. Hireability was assessed by "Should this participant be hired for the job?" to be answered as 1 = no, 2 = maybe, or 3 = yes. Following Cuddy et al. (2015), behavioral dominance indicators (see Table 1) were grouped into the two factors of verbal content (internal consistency α = .97) and nonverbal presence (internal consistency α = .96). In this study, we were also interested in emotional and relational aspects of social sensitivity. To assess this dimension (see Table 2), we used the items of the Interpersonal Reactivity Index (IRI) by Davis (1983). The items were adapted from the subscale "perspective taking" which best represents social sensitivity as defined by Bender et al. (2012). Additionally, we adapted items from the Trait Emotional Intelligence Questionnaire (TEIQue) by Petrides and Furnham (2001). Internal consistency of the eight items was satisfactory (α = .94).

Table 1. Behavioral ratings of dominance/ratings from Cuddy et al. (2015).

1)	Expansiveness: How expansive was the speaker's body?
2)	Overall performance: Overall, how good was the interview?
3)	Hireability: Should this person be hired for the job?
Nonverbal presence	
4)	Enthusiastic: How enthusiastic was the speaker?
5)	Captivating: How well did the speaker capture your attention?
6)	Confident: How confident was the speaker?
7)	Awkwardness (reverse scored): How awkward was the speaker?
Verbal content	
8)	Structured: How well organized and structured was the speech?
9)	Straightforward: How straightforward was the speech?
10)	Intelligent: How smart and intelligent was the speech?
11)	Qualified: How impressive were the qualifications that the speaker mentioned in the speech?

Items were rated on a scale from 1 (not at all) to 7 (very much). This table is from Cuddy et al. (2015) by copyright 2015 of the American Psychological Association. Adapted with permission.

Table 2. Behavioral ratings of social sensitivity.

1)	Empathy: How empathic is the candidate?
2)	When speaking, did the speaker give examples of her/his empathy?
3)	Was the speaker socially sensitive?
4)	Did the speaker demonstrate team work abilities?
5)	Was the speaker trustworthy?
6)	Was the speaker clear about her/his own and other people's feelings?
7)	Did the speaker mention the importance of social relationships?
8)	Was the speaker capable of taking someone else's perspective?

Items were rated on a scale from 1 (not at all) to 7 (very much).

Procedures

Closely following the procedures by Cuddy et al. (2015), participants were randomly assigned to either the high-power (expansive and open) or the low-power pose (contractive and closed) condition. At the beginning, the participants were told that the cover story of the experiment was "Physical motion and performance." For that purpose, the participants were asked to give a baseline saliva samples. To foster credibility of the cover story, participants were asked to fill in a questionnaire about consumed food and liquids or completed exercises during that day.

Participants of Group A were then asked to adopt a high power (expansive and open) and participants of Group B to adopt a low-power pose (contractive and closed) for 1 min, respectively, closely following Cuddy et al. (2015). High-power posers stood up straight, put their hands to theirs hips, pointing them out to the sides and feet shoulder width apart. In contrast, low-power posers put their feet together and entwined themselves with their arms. All instructions were directly taken from Cuddy et al. (2015):

> **High-power pose condition**.
> This study is about physical motion and performance. There is a physical position we'd like you to try out. If you could stand up and sort of stand with your two feet apart and hands on your hips like this [experimenter demonstrated for participant]. Get comfortable in this pose for a minute while I go set something up. Just get comfortable in this physical position and I will be back in 1 min [If needed, experimenter adjusted the participant's posture by lightly touching arms and legs].

> **Low-power pose condition**.
> This study is about physical motion and performance. There is a physical position we'd like you to try out. If you could stand up and sort of stand with your feet together and crossed over and your arms and hands wrapped around your torso like this [experimenter demonstrated for participant]. Get comfortable in this pose for a minute while I go set something up. Just get comfortable in this physical position and I will be back in 1 min [If needed, experimenter adjusted the participant's posture by lightly touching arms and legs].

Directly after the initial 1-minute power poses manipulation, participants were asked to imagine that they were about to be interviewed for their dream job and had to prepare for a videotaped self-presentation in front of two experts. These procedures were taken from the Trier Social Stress Test (Kirschbaum, Pirke, & Hellhammer, 1993). Participants were asked to maintain the body posture during 5 min of preparation, altogether they held the posture for 6 min. During their preparation time participants were videotaped, to verify that they actually maintained the posture. The participants were aware of this fact. The instruction was:

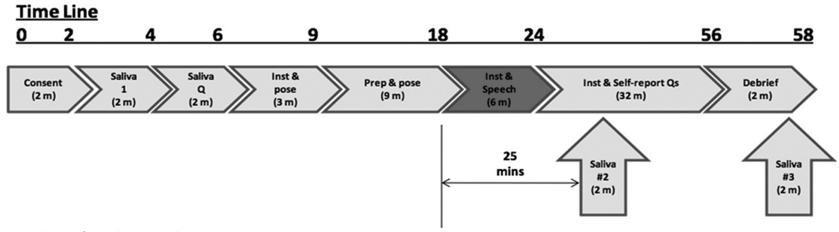
> Now what we are going to do is to have you prepare a speech. Imagine that you are about to interview for your dream job. We'd like you to stay in this position and think about what you will say. You will have 5 min to prepare then you will deliver your speech for 5 min to two evaluators. The other experimenter and I will evaluate your performance on the speech task. We will be evaluating your nonverbal behavior and what you say and how you say it. Remember, you really want this job. You should be honest and straightforward and talk about your experiences, strengths, and why you should be chosen for this job. You should keep this physical position while you are preparing the speech. To prepare, just think through what you want to say, and you may practice. I am going to turn on this video camera while you prepare. The camera is there so that we can later verify that you maintained this physical position. Remember, you are preparing for 5 min; then you will deliver a 5-min speech to two evaluators. Do you have any questions? I am turning on the video camera now and I will leave the room while you prepare. I will be back in 5 min.

Self-presentations were given in front of a video camera and in front of two experts who did not give any verbal or nonverbal feedback. In order to increase credibility of the cover story ("Physical Motion and Performance"), participants were asked to give salivary samples directly after preparing for their self-presentations.

> You can now stand however you like. I am [Experimenter #1's name] and this is [Experimenter #2's name]. We are both experienced evaluators. We will be evaluating how you perform on your speech on a number of different dimensions. We will be observing your nonverbal behavior and listening to what you say and how you say it. We will be taking some notes while you are giving your 5-min speech. The camera is rolling and you may begin whenever you are ready. Please begin by stating what your ideal job is.

Directly after their presentations, participants completed a five-item measure of self-reported feelings of power, answering how dominant, in control, in charge, powerful and like a leader participants felt on a 5-point scale (1 = not at all, 5 = a lot, $\alpha = .883$). These procedures were identical to the study by Cuddy et al. (2015). A complete test instruction can be found in Table 3. Finally, just before debriefing participants they were asked, whether s/he had any knowledge of the paper by Cuddy et al.'s (2015) or whether s/he had seen the Ted talk by Cuddy on these results.

Table 3. Briefing of the experimenters.



*Order of tasks (~1 h; N = 200)
*10 Euro per hour for each subject
*Two experimenters at a time (need two evaluators)
*Prepare Saliva materials and other materials (consent, experimenter log)
*Note on door indicating to DO NOT DISTURB and WAIT HERE (be seated)
*Evaluators make notes on clipboards; no expression on faces/no nodding.
*Randomly assign to: condition (BIG vs. SMALL),

(1) Consent (2 min)
(2) Saliva #1 – baseline (2 min)
(3) Saliva questionnaire (2 min)
(4) Instructions and pose "physical motion and performance" (BIG or SMALL; (3 min)
(5) Speech task instructions and power pose while prepping Trier (videotape; 9 min)
(6) Trier task instructions [1 min] and speech with two evaluators (videotape; 5 min)
(7) Saliva #2 exactly 25 min after end of Trier PREP
(8) 30 min of self-report
(9) Debrief
(10) Saliva #3 at very end after debrief

Consent [2 min]
"Welcome to our study called *physical motion and performance*. First fill out this consent form. Today we will be taking saliva samples but on the consent form it says you will also be hooked up to physiological recording equipment which you will not be doing today."

Saliva #1 (put sticker on microtubule subject # and time #1) [2 min]
"The first thing we are going to do is take a baseline saliva sample. From this saliva, we can measure different things going on in your body. I can tell you all about it at the end of the study. What I want you to do is swallow until your mouth is dry. Now take this piece of sugarfree gum and chew it for a minute or so and pool as much saliva as you can in your mouth and then drool through this sterile straw into this sterile microtubule"

Saliva Questionnaire [2 min]
"Now we need you to fill out this questionnaire- it asks you about anything you have had to eat or drink or any exercise you have had in the past couple hours. Let me know when you have finished the questionnaire."

BIG: Instructions "physical motion and performance" [3 min]
"This study is about physical motion and performance. There is a physical position we'd like you to try out. If you could stand up and sort of stand with your two feet apart and hands on your hips like this [do not show photo but demonstrate for participant]. Get comfortable in this pose for a minute while I go set something up. Just get comfortable in this physical position and I will be back in one minute [adjust person if needed by demonstrating; close door and leave person in pose]."

SMALL: Instructions "physical motion and performance" [3 min]
"This study is about physical motion and performance. There is a physical position we'd like you to try out. If you could stand up and sort of stand with your feet together and crossed over and your arms and hands wrapped around your torso like this [do not show photo but demonstrate for participant]. Get comfortable in this pose for a minute while I go set something up. Just get comfortable in this physical position and I will be back in one minute [adjust person if needed by demonstrating; close door and leave person in pose]."

Speech Task Instructions and Speech Task Prep (videotape preparation) [9 min]
"Great. Now what we are going to do is to have you prepare a speech. Imagine that you are about to interview for your dream job. We'd like you to stay in this position and think about what you will say. You will have 5 min to prepare then you will deliver your speech for 5 min to 2 evaluators. Me and the other experimenter will evaluate your performance on the speech task. We will be evaluating your nonverbal behavior and what you say and how you say it. Remember, you really want this job. You should be honest and straightforward and talk about your experiences, strengths, and why YOU should be chosen for this job. You should keep this physical position while you are preparing the speech. To prepare just think through what you want to say and you may practice. I am going to turn on this video camera while you prepare. The camera is so that we can later verify that you maintained this physical position. Remember, you are preparing for 5 min then you will deliver a 5 min speech to 2 evaluators. Do you have any questions? I am turning on the video camera now and I will leave the room while you prepare. I will be back in 5 min." [make sure video can see participant's full body and face].

(*Continued*)

Table 3. (Continued).

Trier task with two evaluators (look down 10x; videotape) [6 min + 2 min instruct + 30 min of self-report]
"Ok. You can now stand however you like. I am X and this is Y. We are both experienced evaluators. We will be evaluating how you perform on your speech on a number of different dimensions. We will be observing your nonverbal behavior, listening to what you say, and how you say it. We will be taking some notes while you are giving your 5-minute speech. The camera is rolling [check to make sure still can see full body and face], you may begin whenever you are ready [start watching time]. Please begin by stating what your ideal job is."
"Ok, you may now sit down and complete some measures on the computer. There are a number of different questionnaires and tasks for you to complete. This will take you about 30 min. We will come in at some point in about 20 min and take a second saliva sample. We are turning off the video camera now." [make sure correct sub # is on Media Lab and start computer]
Saliva #2: 25 min **after END of Trier PREP (sticker on microtubule sub # and time #2) [2 min]**
"Hi- I am just going to interrupt you for a minute to take a second saliva sample. Again, we need you to swallow until your mouth is dry. Now take this piece of Sugarfree Gum and chew it for a minute or so and pool as much saliva as you can in your mouth and then drool through this sterile straw into this sterile microtubule. Ok, I am going to leave you again so that you can continue with your questionnaires. I will come back at the very end about 4 min before you are supposed to leave [make sure they start again]"
Before we are finished, we have one further question for you. Please answer on this sheet:
Have you seen/heard of the TED talk by Dr. Amy Cuddy "Your Body Language Shapes who you are" about Power Posing?
(Picture of Ted talk will by Dr. Amy Cuddy will be shown as a reminder)
Debrief [2 min]
"Ok, thank you so much for participating in our experiment. Today we are interested in the relation between physical motion and performance. Our hypothesis is that some kinds of physical motions help performance on stressful tasks like the speech task. We asked you to pose in different physical positions to explore the relation between these positions and how well you did on the speech task. We are also interested in how your body responds to stress so we will be analyzing your saliva for the hormone cortisol which is a stress hormone. Do you have any questions? Thank you so much for your participation today."
Saliva #3: at very end of the experiment (sticker on microtubule sub # and time #3) [2 min]
"Just before you walk out the door we need to get one final saliva sample. Here is a tube and a straw and some gum. Ok, thanks so much!"
Statements
We confirm that we will share the raw data and laboratory log from our study. Additionally we confirm, that we will agree to CRSP publishing a short summary of the preregistered study under section Withdrawn Registrations, if we later withdraw our paper, after having received an IPA.

Deviation from preregistered protocol

At the end of the experiment, before participants were asked whether they had any knowledge of the study by Cuddy and colleagues (2015), participants filled in a short ten item Big Five questionnaire (Rammstedt, Kemper, Céline, Klein, & Kovaleva, 2013). Because this measure had not been reported in the preregistered procedures, we do not present any results for the Big Five measures in the current study.

Results

Preregistered analyses

We first tested whether the power-posing manipulation showed any effects on participants' self-reported feelings of power after delivering their self-presentations. Similarly to Cuddy et al. (2015), participants of the high-power group (M = 3.00, SD = 0.82) reported higher feelings of power than participants of the low-power group (M = 2.73, SD = 0.90), $t(198)$ = 2.25, p = .01, d = .318, one-sided.

When testing effects on behavioral dominance indicators following Hypothesis 1, we found no significant effects of the power poses manipulation. Overall quality of participants' presentations was not judged better for the high (M = 4.23, SD = 1.34) as compared with the low (M = 4.17, SD = 1.47) power posers, $t(198) = .28$, $p = .39$, $d = .04$, one-sided. Similarly, nonverbal presence was not judged differently between high (M = 4.13, SD = 1.37) and low (M = 4.28, SD = 1.40) power posers, $t(198) = -.74$, $p = .23$, $d = -.11$, one-sided. The same was true for verbal content (M = 3.82, SD = 1.45 versus M = 3.93, SD = 1.45), $t(198) = -.53$, $p = .30$, $d = -.08$, one-sided. Thus, Hypothesis 1 was not confirmed by our results.

When testing effects on behavioral social sensitivity indicators following Hypothesis 2, we also found no significant differences between high (M = 3.80, SD = 1.36) and low (M = 4.06, SD = 1.43) power posers, $t(198) = -1.29$, $p = .10$, $d = -.18$, one-sided. Thus, Hypothesis 2 was also not confirmed by the current results. The same was true when testing for Hypothesis 3. Hireability of participants was not judged differently between high (M = 2.25, SD = .75) and low (M = 2.32, SD = .73) power posers, $t(198) = -.63$, $p = .36$, $d = -.09$, one-sided.

When testing for Hypothesis 4, hireability judgements were independently predicted by nonverbal presence ($\beta = .48$, $t = 6.28$, $p < .001$), verbal content ($\beta = .29$, $t = 3.60$, $p < .001$), and social sensitivity ($\beta = .13$, $t = 2.48$, $p = .01$) judgments, $R^2 = .67$, $p < .001$. Because the power-posing manipulation did not show any effects on hireability judgments, we had to drop analyses for Hypothesis 5 that aimed to explore whether these effects were mediated by behavioral dominance indicators.

Exploratory analyses

Additional analyses explored whether participants' sex or their knowledge of the Ted talk about the study by Cuddy and colleagues (2015) showed any significant main or interaction effects with the power poses manipulation on the dependent variables. All analyses resulted in nonsignificant effects. Specifically, sex did not show a significant main or interaction effect on participants' self-reported feelings of power after delivering their self-presentations, $F(1,196) = 2.37$, $p = .37$, $\eta^2 = .70$ and $F(1,196) = .52$, $p = .47$, $\eta^2 = .00$. Sex also had no significant main or interaction effect on the overall quality of participants' presentations, $F(1,196) = .82$, $p = .53$, $\eta^2 = .45$ and $F(1,196) = .34$, $p = .56$, $\eta^2 = .00$. The same was true for effects on nonverbal presence, $F(1,196) = .08$, $p = .82$, $\eta^2 = .08$ and $F(1,196) = .57$, $p = .45$, $\eta^2 = .00$, verbal content, $F(1,196) = .51$, $p = .60$, $\eta^2 = .34$ and $F(1,196) = 2.46$, $p = .12$, $\eta^2 = .01$, behavioral social sensitivity indicators, $F(1,196) = 1.68$, $p = .42$, $\eta^2 = .63$ and $F(1,196) = 1.76$, $p = .19$, $\eta^2 = .01$, and hireability judgments, $F(1,196) = .01$, $p = .95$, $\eta^2 = .01$ and $F(1,196) = .80$, $p = .37$, $\eta^2 = .00$.

When participants were asked after their presentations whether they had previously seen the Ted talk about the study by Cuddy and colleagues (2015), 156 participants indicated that they were unaware of the Ted talk, 24 reported they had seen the talk, and 20 did not provide any information. Participants' knowledge of the Ted talk did not show any significant main or interaction effects with the power poses manipulation on the dependent variables. Specifically, knowledge of the Ted talk did not show a significant main or interaction effect on participants' self-reported feelings of power after delivering their self-presentations, $F(1,176) = .85$, $p = .53$, $\eta^2 = .46$ and $F(1,176) = .73$, $p = .40$, $\eta^2 = .00$. Knowledge of the Ted talk also had no significant main or interaction effect on the overall quality of participants' presentations, $F(1,176) = 1.80$, $p = .41$, $\eta^2 = .64$ and $F(1,176) = .14$, $p = .71$, $\eta^2 = .00$. The same was true for effects on nonverbal presence, $F(1,176) = 17.98$, $p = .15$, $\eta^2 = .95$ and $F(1,176) = .01$, $p = .94$, $\eta^2 = .00$, verbal content, $F(1,176) = .15$, $p = .77$, $\eta^2 = .13$ and $F(1,176) = .65$, $p = .42$, $\eta^2 = .00$, behavioral social sensitivity indicators, $F(1,176) = .70$, $p = .56$, $\eta^2 = .41$ and $F(1,176) = .76$, $p = .39$, $\eta^2 = .00$, and hireability judgments, $F(1,176) = 3.40$, $p = .32$, $\eta^2 = .77$ and $F(1,176) = .13$, $p = .72$, $\eta^2 = .00$

Discussion

This study aimed to replicate and extend results by Cuddy and colleagues (2015) and investigated whether high- versus low-power posing could help to improve job interview performance with respect to both behavioral dominance and social sensitivity indicators. Findings showed that the power-posing manipulation was successful in increasing participants' feelings of power, while it did not show significant effects on their observer-rated dominance and social sensitivity. Thus, the current study failed to replicate significant main effects of power posing on behavioral dominance indicators that were found by Cuddy and colleagues. In the current study, overall hireability of participants was independently predicted by behavioral indicators of dominance and social sensitivity. Again, this result corroborates the view of dominance and social sensitivity as independent aspects of social competences.

An important strength of the current study is represented by the fairly large sample size of 200 participants and the use of two coders who both independently rated the complete 200 participants. Possible reasons for not replicating the main effects of power posing found by Cuddy and colleagues (2015) could be searched in cultural differences in the way people are socialized in Germany and the USA (Kieser, 1994). It seems plausible that dominant and assertive self-presentation is evaluated more positively in the USA, while participants in Germany refrain from presenting themselves too self-confidently. German participants may try to make a more humble impression and thus profit less from power-posing manipulations.

Overall, the current study replicated behavioral dominance as a strong predictor of hireability judgments. Additionally, social sensitivity judgments added incremental validity for the prediction of hireability over and above behavioral dominance judgments. However, the current results failed to replicate significant main effects of power posing on behavioral dominance indicators that were found by Cuddy and colleagues (2015). Participants' sex had no influence on their observer-rated dominance and social sensitivity and also did not interact with the power poses manipulation. Future studies should search for other variables that may moderate effects of power poses on social dominance and sensitivity. Motivation to present oneself in a more humble and modest way as compared with a more dominant and self-assertive way could be one potential moderator. It may be explored whether especially participants who want to make a dominant impression profit from power poses manipulations whereas power posing has no or even negative effects on participants who want to present themselves more humbly. According to the current results power posing had no positive effects whatsoever on observer judgements of social competences. Future studies should identify the personal and situational variables that trigger positive versus negative consequences of power posing on social competences.

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ARTICLE

Power vs. persuasion: can open body postures embody openness to persuasion?

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ABSTRACT

In the current study, we sought to replicate the finding that adopting an open/expansive body posture increases subjective feelings of power, while also investigating how these body postures influence the processing of persuasive messages. Two hundred participants were randomly assigned to adopt either an open or a closed body posture while reading either a strong or a weak persuasive message regarding junk food taxation. Afterwards, we measured participants' attitudes toward junk food, subjective feelings of power, thought confidence, and openness. Results failed to replicate the previously found effect of body posture on subjective feelings of power. Compared to weak messages, strong messages led to more persuasion, higher subjective power, more thought confidence, and more openness. However, body posture did not affect these outcomes. Overall, these findings challenge the idea of a direct, causal relationship between open body postures and power, by showing that power posing effects are not maintained under certain conditions.

Arms up in the air, head tilted back, palms open. Recent research has identified this body posture as a "power posture" – a posture not only indicating but also inducing powerful feelings (Carney, Cuddy, & Yap, 2010). But the same posture can be observed among attendees of an evangelical church or a motivational speech, where it may signal and induce openness, be it to the Holy Spirit or to new ideas.

These two possible states – power and openness – may be inconsistent with each other. In fact, power induced through role-playing before receiving a persuasive message can increase confidence in one's own thoughts, make people rely more on their existing opinions, and decrease the amount of elaboration of persuasive messages (Brinol, Petty, Valle, Rucker, & Becerra, 2007). However, we contend that, depending on the situation, open body postures may mean something else than power. Specifically, when placed in the role of the recipient of a persuasive message, adopting open body postures may activate a different meaning of openness – that of openness to persuasion. This activation would determine individuals to show opposite effects: be more open to others' messages and, as a result, increase elaboration of persuasive arguments. In sum,

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in the current study we investigate whether, under certain conditions, open body postures may embody openness to other's ideas rather than power.

Embodied power

Open body postures can lead people to feel more powerful subjectively (Carney et al., 2010; Park, Streamer, Huang, & Galinsky, 2013; Ranehill et al., 2015). This increase in subjective feelings of power was accompanied by other power-related outcomes such as increased accessibility of power-related words (Huang, Galinsky, Gruenfeld, & Guillory, 2011; Park et al., 2013), increased risk taking (Carney et al., 2010), and enhanced performance during a self-presentation task (Cuddy, Wilmuth, Yap, & Carney, 2015). However, the evidence for the effect of power postures is mixed (e.g. using a high-powered sample, Ranehill et al. did not replicate the Carney et al. effects on risk taking) and many of the existing studies are underpowered.

Besides methodological concerns such as sample size, in the current study we also seek to address the issue of the casual process proposed in the literature on "power postures." So far, the relationship between open body postures and increased power feelings is seen as a direct, causal relationship, as emphasized by Adam and Galinsky (2012), who suggest that the symbolic meaning of the body posture is automatically embodied because it is a direct *result* of the physical experience. Similarly, Cuddy and colleagues cite William James's (1950) theories of emotion and ideomotor action, according to which a physiological reaction *causes* the experience of a certain emotion. In the same vein, it is believed that open body postures *cause* feelings of power through a direct link. This assumption has become so ingrained in the current discourse on body postures that open body postures are now known and equated with power postures.

However, we have greatly advanced in the understanding of emotion since James. For example, the two-factor theory of emotion suggested that the same physiological arousal can ultimately cause different emotions. One classic example comes from Schachter and Singer (1962), in which the same epinephrine injection caused participants to feel angry in one condition and euphoric in another condition, depending on the emotional state of the confederate. Although Schachter's theory was criticized and modified over the years (Cotton, 1981; LeDoux, 1995), the important idea is that later data challenged James's idea of a direct, causal relationship between physical experiences and distinct emotions. Similarly, we must contend that open body postures can ultimately induce other psychological states than those related to power.

Relatedly, recent research on open body postures has shown that the effect of expansive postures on power feelings can be moderated by contextual factors. For example, in Brinol, Petty, and Wagner (2009), the direction of thoughts moderated the effects of open postures on attitudes toward the self, such that open postures led to more positive thoughts about the self only when participants wrote about their best qualities and not about their weaknesses.

In another study, open body postures led to more risk taking compared to closed body postures only when the context was social (there were faces presented), and the effect disappeared in a nonsocial context (no faces), suggesting that the effects of open body postures are moderated by action possibilities (Cesario & McDonald, 2013). Moreover, when participants imagined being in a submissive role (e.g. being frisked by the police) while maintaining an open body posture, they behaved in a disempowered way (Cesario & McDonald, 2013).

Overall, research suggests that, depending on contextual factors, the effect of expansive body postures on power-related feelings can be weaker, stronger, or even reversed. However, although there is evidence that the meaning of the body posture can be interpreted as lower power, there are no studies to our knowledge that investigated non-power-related interpretations of open body postures. In other words, what are *other* psychological states that may be associated with open body postures? In the current study, we investigate whether one of those outcomes is openness to persuasion, given contextual cues that do not prime power, but rather the state of being open to other people's arguments.

Embodied persuasion

There are numerous studies that show that persuasion can be embodied, such that certain positions or movements of the face or body can increase the likelihood to change one's attitude in response to a persuasive message. For example, people were more persuaded when they received persuasive messages while reclining comfortably rather than standing (Petty, Wells, Heesacker, Brock, & Cacioppo, 1983), when nodding rather than shaking their head (Brinol & Petty, 2003), or when they had their arm extended rather than flexed (Cacioppo, Priester, & Berntson, 1993).

According to Brinol and Petty (2008), body states and postures influence persuasion either through evaluative conditioning (e.g. head nodding is associated with a positive reaction which, in turn, becomes associated with the persuasive message) or through increasing or reducing cognitive resources (e.g. standing takes more resources than sitting comfortably, which cognitively loads participants exposed to persuasive messages). However, although there is evidence that persuasion can be embodied, it seems imperative to revisit the issue of embodied persuasion in the light of the "power poses" literature, which equals open body postures with power without further investigating other states. Indeed, none of the existing embodied persuasion studies investigated open vs. closed body postures and whether they would embody openness to persuasion.

Open body postures: power and/or persuasion?

In the current study, we propose that the context or the implicit role cues in which the person adopts an open body posture may influence what is being embodied. If the person is asked to give a persuasive speech, make risky decisions, or imagine being a business owner who has to make decisions, such implicit task cues may activate power because participants are put in a situation in which they can influence outcomes or other people. However, if the person is placed in a situation in which they are *exposed to* persuasive messages, such implicit task cues may activate openness to persuasion. The meaning of the body posture may be interpreted differently, depending on the situation: open = power when the person is in a situation in which they have to take risks (Carney et al., 2010; Huang et al., 2011), persuade others (Cuddy et al., 2015), or role-play a business owner (Fischer, Fischer, Englich, Aydin, & Frey, 2011), but open = openness to new ideas when the person is in a situation in which they are exposed to a persuasive message. In the latter case, consistent with principles of evaluative conditioning, the openness cue from the body would become associated with the message and translate into more openness to new ideas. Being open to new ideas would determine people to engage in careful, effortful processing when evaluating persuasive messages, and as such change their attitude when the persuasive message is strong, but not when the message is weak. Given that the open body postures would activate the "openness to new ideas" meaning and not the "power" meaning, we would expect these postures *not* to lead to increased powerful feelings and confidence in one's thoughts, a finding inconsistent with the current view on power postures (Carney et al., Cuddy et al.).

Conversely, if there were an exclusive causal relationship between open body postures and power as proposed by the power posing literature, then we would expect open postures to lead to more powerful subjective feelings. In turn, feeling powerful would lead to having more confidence in one's own thoughts, and in terms of persuasion we should see effects similar to studies 2 and 5 in Brinol and colleagues (2007), whereby power reduced persuasion to strong arguments because power validates a person's own initial beliefs.

It should be noted that Fischer and colleagues (2011) suggested that power postures (e.g. making a fist, open body postures) could increase confirmatory information processing, such that individuals favored information that was consistent with their point of view. This finding may seem inconsistent with our proposal that open body postures may increase openness to new ideas. However, we contend that the confirmatory information processing was obtained because participants were, at the same time, asked to take the role of a high-power person (owner of a business who has to make decisions), thus offering contextual cues which activate the concept of power rather than openness. As such, we believe the confirmatory information processing was a result of a power manipulation, which included posture but also high-power role-playing. Still, to minimize this concern, we will initially ensure that participants have a fairly neutral attitude regarding the topic of interest.

The current study and hypotheses

In the current study, we seek to investigate whether open body postures can embody openness to persuasion under certain conditions. We will initially neutralize participants' attitude toward introducing a junk food tax by asking participants to generate an equal number of arguments supporting and opposing junk food taxation and then measure their attitude toward it, which we expect to be relatively neutral. Afterwards, we will ask participants to adopt either an open or a closed body posture, using the same postures, instructions, and cover story as Carney and colleagues (2010). While adopting these postures, participants will be exposed to a persuasive message favoring the introduction of a tax on junk food. In order to enhance the extent to which these findings seek a closer replication of Carney and colleagues, we will present human faces along with the persuasive messages. We will additionally manipulate the quality of the argument by presenting either strong or weak persuasive messages. We take this approach in order to investigate the extent to which body posture may lead to persuasion through a thoughtful, elaborate processing of the message (in which case persuasion would be enhanced by strong messages) or through a simple thoughtless process (in which case persuasion would be enhanced by weak messages). We will then assess participants' attitude change in line with the persuasive message, their subjective power, their openness and confidence in their own thoughts.

We propose two competing hypotheses. If open body postures exclusively embody power, then we should see effects similar to the power effects on persuasion found in Brinol et al. (2007, studies 2 and 5), such that closed body posture (low power) would lead to more favorable attitudes toward junk food taxation, especially when the message contains strong arguments. Open body postures would lead to less favorable attitudes, regardless of the quality of the argument. This result would be expected because power would increase confidence in one's own thoughts, so the quality of the argument should not matter. As such, we also propose that open body postures would increase confidence in one's own thoughts and would decrease openness. Importantly, consistent with Carney and colleagues, we would also expect that open body postures increase feelings of subjective power.

Conversely, if power embodies openness to persuasion, we should see a different pattern. First, if openness of body signals openness to new ideas, we should see that participants are more deeply processing messages in open vs. closed postures and they would be, as such, especially influenced by a strong message. So we predict that participants would be most persuaded when being exposed to a strong message while holding an open posture. If the mechanism is indeed increased openness to ideas, we should also see that participants holding open body postures report being more open compared to those holding closed body postures. Inconsistent with Carney and colleagues (2010) and the literature on power and persuasion (Brinol et al., 2007; Fischer et al., 2011), open body postures should not lead to an increase in subjective power feelings and confidence in own thoughts.

This research is important as it will establish whether there is an exclusive, causal relationship between open body postures and powerful feelings or whether the meaning of open body postures can be interpreted differently depending on the implicit role cues present in the situation.

Method

Participants and design

Two hundred participants (111 female, mean age 22.43 years old) were recruited on the Rutgers University-Camden campus and were paid $10 for their participation. Participants were only excluded if they had severe difficulties in speaking, reading, or understanding English. Participants were randomly assigned to one of four conditions in a 2 (body posture: open or closed) $\times$ 2 (persuasive message: strong vs. weak argument) between-participants design.

The sample size was determined based on a power analysis conducted with G*Power software (Faul, Erdfelder, Lang, & Buchner, 2007), using the Wilcox–Mann–Whitney test with an alpha level of .05. The estimated effect size used in the analysis was .50 and was obtained by averaging four effect sizes: $d = .91$ (Carney and colleagues, 2010), $d = .48$, $d = .28$ (Huang et al., 2011), and $d = .34$ (Ranehill et al., 2015). We used the effect sizes on subjective power feelings as this is the main finding we are attempting to replicate. The a priori power (1 – β) was set to .95. The resulting total *N* was 184, which we rounded up to 200.

Procedure

Participants were run by one of two female experimenters who were blind to the hypotheses of the study. After signing informed consent, participants were informed that the study investigated how people evaluate certain messages related to current societal issues. To begin with, participants underwent a "neutralization" procedure – we asked them to generate three arguments supporting and three arguments opposing the junk food taxation in order to ensure that participants have a relatively neutral attitude toward junk food taxation at the beginning of the experiment. Using a MediaLab program, we first displayed a brief explanation of junk food taxation ("*Junk food is food that has low nutritional value, typically high in sugar and produced in the form of packaged snacks needing little or no preparation. Junk food taxation would involve adding an extra tax on the sale of junk foods*") and then prompted participants to alternate between generating arguments supporting and opposing junk food taxation. To check for the success of our "neutralization" strategy (or to be able to control for participants' initial opinion in further analyses if the neutralization was not successful), we then measured participants' pre-attitude toward junk food taxation.

Afterwards, participants were asked to adopt either an open or a closed body posture depending on the condition. While maintaining the assigned body posture, participants were exposed to either a strong or a weak message supporting junk food taxation, depending on the randomly assigned condition. We then measured participants' feelings of power, attitude toward junk food taxation, subjective power feelings, openness, and thought confidence. Finally, participants were debriefed. We also asked them whether they were familiar with the power posture literature (from classes or the Amy Cuddy Ted Talk).

Cover story

Participants were told "*you will participate in a study which investigates how people evaluate certain messages related to global issues. Before you begin, we would like to pretest some ideas for a future study we will be conducting. For this, we are asking your help in generating arguments for and against the introduction of a tax on junk food.*" After generating the arguments and measuring their pre-attitude toward junk food taxation, we told participants "*You are now ready to participate in the main study in which we will ask you to evaluate one message related to the issue of junk food taxation. Your task is to read this message and later evaluate it. As side project we are currently conducting in our lab, we are also testing the accuracy of physiological responses as a function of sensor placement. Because of this, we are asking you to hold a certain physical position.*"

Body posture manipulation

We used the two standing positions from Carney et al. (2010). Open body posture included arms away from body, palms open, feet apart, whereas the closed body posture included arms crossed across the chest, palms against the body, feet crossed at the ankles. We did not use the forward lean in the Carney and colleagues standing open position because it may introduce a confound. More specifically, we aimed for the body posture openness to be the only factor being manipulated across the open and closed postures (e.g. participants in the open body posture condition should not be physically closer to the screen displaying the persuasive message compared to those in the closed body posture condition). Pictures similar to Carney and colleagues were used as models. A screen displaying the persuasive message was placed at eye level in front of the participants. We also presented faces along with the persuasive message.

Persuasive messages

Both the strong and weak messages are included in the Appendix.

Measures

Pre-attitude about junk food taxation

Following the neutralization procedure and before being exposed to the persuasive message, participants were asked to rate junk food taxation on three 9-point (1–9) semantic differential scales (*against – in favor, unfavorable – favorable, bad – good*). The items were averaged to create a composite pre-attitude index (M = 5.23, SD = 2.07, Cronbach's α = .92), with higher numbers denoting more positive attitudes toward junk food taxation.

Post-attitude about junk food taxation

Following the persuasive message, participants were asked to rate junk food taxation on three 9-point (1–9) semantic differential scales (*against – in favor, unfavorable – favorable, bad – good*). The items were averaged to create a composite post-attitude index (M = 6.08, SD = 2.14, Cronbach's α = .93), with higher numbers denoting more positive attitudes toward junk food taxation.

Sense of power

Consistent with Carney et al. (2010) and Ranehill et al. (2015), participants indicated how "powerful" and "in charge" they felt on a scale from 1 (*not at all*) to 4 (*a lot*). The items were averaged to create a composite subjective power index (M = 2.51, SD = 0.89, Cronbach's α = .83), with higher numbers denoting higher sense of power.

Thought direction and confidence

We asked participants to list the thoughts that went through their mind when exposed to the message about junk food taxation. After listing their thoughts, we asked participants to rate on nine-point scales how favorable the thought was (1 = *not at all favorable*, 9 = *extremely favorable*), how confident they were in that thought (1 = *not at all confident*, 9 = *extremely confident*), and how valid they believed the thought to be (1 = *not at all valid*, 9 = *extremely valid*). The last two items were averaged to create a composite thought confidence index (M = 7.71, SD = 1.23, Cronbach's α = .94), with higher numbers denoting more confidence in own thoughts.

Openness

Participants rated four statements on a scale from 1 (*strongly disagree*) to 9 (*strongly agree*) as they relate to their state while reading the persuasive message. The items are "*I feel open to new ideas," "I welcome thinking about new ideas," "I like to entertain different points of view," and" I am curious about many different things*." The items were averaged to create a composite openness index (M = 6.42, SD = 1.56, Cronbach's α = .85), with higher numbers denoting more openness.

Results

Preliminary analyses

As a preliminary analysis, we investigated whether the pre-attitude toward junk food taxation was indeed neutral by computing a one-sample t-test against the value of 5 – the midpoint of the scale. The mean attitude toward junk food taxation before the experimental manipulation was M = 5.23, SD = 2.07, which was not significantly different from 5, $t(199) = 1.60$, p = .112, *Cohen's d* = .11, 95% CI [–0.05, 0.52]. Given the lack of significance and the small effect size, this variable was not used as a covariate in future analyses.

Pre-registered analyses

Persuasion

To test our main research question regarding the effects of body posture on persuasion, we conducted a 2 (body posture: open or closed) × 2 (persuasive message: strong vs. weak) between-participants analysis of variance (ANOVAs) on the post-attitude toward junk food taxation. Results showed a main effect of message strength, $F(1, 196) = 10.72$, p = .001, *Cohen's d* = .46, 95% CI [0.39, 1.55], such that strong messages (M = 6.57, SD = 2.24) led to more persuasion compared to weak messages (M = 5.60, SD = 1.94). This result shows that our manipulation of message strength was successful and replicated previous research. The main effect of body posture on attitudes toward junk food was not significant, $F(1, 196) = 1.67$, p = .197, *Cohen's d* = .18, 95% CI [–0.20, 0.97], although an inspection of the means suggested a trend for open body postures to lead to more persuasion (M = 6.27, SD = 2.07) compared to closed body postures (M = 5.89, SD = 2.21). The interaction between posture and message strength was not significant, $F(1, 196) = .05$, p = .83, η^2 = .0001.

Subjective feelings of power

In order to investigate whether Carney and colleagues' effects of body postures on subjective power replicate, we conducted a 2 (body posture: open or closed) × 2 (persuasive message: strong vs. weak) between-participants ANOVAs on subjective power. Results did not replicate Carney and colleagues' findings, such that there was no main effect of body posture on subjective power, $F(1, 196) = 1.19$, p = .28, *Cohen's d* = .15, 95% CI [–0.10, 0.36]. In other words, participants who adopted open body postures did not feel more powerful (M = 2.57, SD = 0.79) compared to participants who adopted closed body postures (M = 2.44, SD = 0.98). The interaction between posture and message strength was not significant either, $F(1, 196) = .45$, $p = .50$, $\eta^2 = .002$. However, further validating our message strength manipulation, there was a main effect of message strength on subjective power feelings, $F(1, 196) = 22.81$, $p = .0001$, *Cohen's* $d = .68$, 95% CI [0.33, 0.80], with participants feeling more powerful after being exposed to a strong message ($M = 2.79$, $SD = 0.79$) compared to a weak message ($M = 2.22$, $SD = 0.89$).

Thought confidence analyses

We also conducted a 2 (body posture: open or closed) × 2 (persuasive message: strong vs. weak) between-participants ANOVA on the thought confidence score. There was a main effect of message strength on confidence, $F(1, 195) = 5.68$, $p = .02$, *Cohen's* $d = .35$, 95% CI [0.07, 0.75], with participants feeling more confident in their thoughts regarding junk food taxation after being exposed to a strong message ($M = 7.92$, $SD = 1.06$) compared to a weak message ($M = 7.50$, $SD = 1.35$). However, the main effect of body posture on thought confidence was not significant, $F(1, 195) = 0.003$, $p = .96$, *Cohen's* $d = .008$, 95% CI [−0.35, 0.33], with no difference in thought confidence between participants who adopted an open body posture ($M = 7.71$, $SD = 1.31$) compared to a closed body posture ($M = 7.72$, $SD = 1.14$). The interaction between body posture and message strength was not significant either, $F(1, 195) = 1.11$, $p = .29$, $\eta^2 = .006$.

Openness

Finally, we conducted a 2 (body posture: open or closed) × 2 (persuasive message: strong vs. weak) between-participants ANOVA on the openness score. Results showed a similar pattern, with a main effect of message strength on openness, $F(1, 196) = 18.64$, $p = .0001$, *Cohen's* $d = .61$, 95% CI [0.50, 1.34], such that participants exposed to a strong message felt significantly more open to arguments ($M = 6.87$, $SD = 1.49$) compared to those exposed to weak messages ($M = 5.96$, $SD = 1.50$). The main effect of posture on openness was not significant, $F(1, 196) = 0.58$, $p = .44$, *Cohen's* $d = .11$, 95% CI [−0.58, 0.26], with participants not differing in their openness feelings in an open ($M = 6.33$, $SD = 1.53$) compared to a closed body posture ($M = 6.50$, $SD = 1.59$). Furthermore, the interaction between body posture and message strength was not significant, $F(1, 196) = 0.23$, $p = .63$, $\eta^2 = .001$.

Discussion

In the current study, we proposed two competing hypotheses. According to the first hypothesis, open body postures would increase feelings of subjective power, thus supporting the idea of an exclusive, causal relationship between open body postures and power (Adam & Galinsky, 2012; Carney et al., 2010). As a competing hypothesis, we proposed that open body postures might embody openness to persuasive arguments, thus supporting the idea that the implicit role cues present in the situation may change the interpretation of the body posture.

The results of the current study failed to show effects of body postures either on subjective power feelings or attitude change following a persuasive message. First, using a high-powered sample size, we did not replicate the Carney et al. (2010) effects of posture on power, such that participants who adopted open body postures did not feel more powerful compared to those who adopted closed body postures. Second, the competing hypothesis regarding persuasion was not supported either. Although the direction of the means suggested that open body postures might lead to more positive attitudes following a persuasive message, this difference was not significant. The predicted interactions with the strength of the persuasive message were not supported either.

It should also be added that our findings did not suggest that body posture influenced cognitive processes. Across all dependent variables, we found an effect of persuasive message strength, but this main effect was not moderated by body posture. This finding suggests that all participants, regardless of their body posture, were engaging in thoughtful processing, hence being more persuaded, confident, and open when hearing a strong compared to a weak message. This finding does not support previous work (e.g. Huang et al., 2011), suggesting that open body postures influence power-related cognitions.

There are several reasons why we may not have replicated the results of Carney and colleagues (2010). First, consistent with our theoretical reasoning, we did not replicate the effects of body posture on power because the cues within the situation were not consistent with the power role – participants were being persuaded rather than persuading others (Cuddy et al., 2015) or asked to take risky decisions (Carney et al., 2010). As such, this finding challenges the idea of a direct causal relationship between body posture and power. Second, there may be several methodological differences between our study and the Carney and colleagues work. For example, faces were presented simultaneously along with the persuasion message. In terms of postures, we only used the standing open and closed postures. Moreover, the open standing posture did not include the forward lean as we considered this to be a potential confound – it would be unclear whether effects on subjective power would be accounted for by the openness of the posture or the forward lean which suggests approach behavior. Indeed, increased power has been associated with approach-related tendencies (Keltner, Gruenfeld, & Anderson, 2003).

It is also worth exploring the reasons why we did not find effects of body posture on persuasion. First, it may be that the message should be delivered through audio rather than writing because reading a complex message and holding a certain posture for several minutes may increase physical discomfort, which may negatively affect persuasion capacity (Brinol & Petty, 2008). Using a persuasive message presented through audio may also be advised for future research because the effects of posture on persuasion may be more likely to occur within a perceived social interaction with another person rather than by reading a written message. Future research should investigate the effects of posture on attitude change and processing in more realistic social interactions, which are more likely to activate implicit cues that influence the interpretation of the body posture.

Conclusions

Using a high-powered sample we were not able to replicate the previous findings on power postures. We did not find support for a direct, causal relationship between open body postures and subjective power, which suggests caution in interpreting the result of Carney and colleagues (2010), as well as subsequent talks and popular books. It is possible that power effects were not replicated because they only occur under specific conditions (e.g. participants being placed in an implicit power role). The current study advances our understanding of such a situation in which power posing is not successful in inducing power feelings.

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Appendix

Pro-junk food tax (strong persuasive message)

In favor of junk food taxation

Some states in the United States are considering legislation on the taxing of junk food. When taking many factors into consideration, this program seems likely to bring about a number of good things.

According to some proponents of this legislation, taxing junk food will provide money for many government-based initiatives. For example, they estimate that a 1-cent tax per 12-ounce soft drink could generate about $1.5 billion annually which could be spent on promoting physical activity and nutrition education. In addition, a penny tax per pound of candy would raise about $70 million. Large amounts of money like this could be used to fund a number of healthy lifestyle programs and to subsidize health insurance for people suffering from obesity.

In addition to the economic benefits, placing a tax on junk food will encourage healthy eating. According to Stanford Professor Keith Brown, a major reason people eat junk food is because it is cheap and convenient. Dr. Brown says that so much cheap junk food creates a "toxic environment" of sweetened food. This junk food is more calorically dense than healthy food, so people are much more likely to gain weight. Taxing junk food could make people choose healthier alternatives because the junk food would no longer be cheaper in the long run. Moreover, Dr. Brown proposes to tax junk food to make unhealthy food more expensive and to use the funds from the tax to decrease the costs of healthy food by 70%. By taking the pressure off of individuals to choose between food quality and food value, people will feel more positive toward buying and eating healthier food.

By promoting healthy eating habits, this taxation would also have an indirect impact on the nation's obesity problem (and medical conditions related to obesity). The *Journal of the American Medical Association* reports that in 2001, 44.3 million Americans were obese and the number of Americans with diabetes increased 61% since 1990. A report from the *Journal of Food Analysis* found that Americans receive nearly one-third of their calories from junk food. These facts are even more alarming when one realizes that diseases like diabetes cost millions of dollars annually in health care and lost productivity. In a 1992 study that assessed the direct costs of treating diabetes in the United States, the American Diabetes Association found that the estimated total expenditure for 1 year was $45.2 billion. Because eating large amounts of junk food is associated with being obese and is related to a higher risk for costly diseases like diabetes, junk food is a major contributor to the current obesity problem.

Pro-junk food tax (weak persuasive message)

In favor of junk food taxation

Some states in the United States are considering legislation on the taxing of junk food. When taking many factors into consideration, this program seems likely to bring about a number of good things.

According to some proponents of this legislation, taxing junk food will provide money for some government-based initiatives. For example, they estimate that a 1-cent tax per 12-ounce soft drink could generate a small amount of money annually which could be spent on a number of different things. In addition, a penny tax per pound of candy could create a small increase in funds as well. Amounts of money like this could be used to partially fund programs for a small number of citizens.

In addition to the economic benefits, placing a tax on junk food might encourage healthy eating. According to college student Keith Brown, a major reason people eat junk food is because it is cheap and convenient. Brown says that so much cheap junk food creates a "toxic environment" of sweetened food. This junk food is somewhat more calorically dense than healthy food, so people are more likely to gain weight. Taxing junk food could make people choose healthier alternatives because the junk food would no longer be much cheaper in the long run. Brown proposes to tax junk food in order to negatively affect junk food producers and, in turn, decrease the large amount of junk food that has become too readily available for consumers. By taking the pressure off of individuals to choose between food quality and food value, people will feel more positive toward buying and eating healthier food.

By promoting healthy eating habits, this taxation may also have an indirect impact on the nation's obesity problem (and medical conditions related to obesity). Another college student reports that in 2001, 15.3 million Americans were obese and the number of Americans with joint pain increased 2% since 1990. A report from a local newspaper found that Americans receive nearly one-twelfth of their calories from junk food. These facts are even more alarming when one realizes that conditions such as joint pain cost thousands of dollars annually in health care and lost productivity. In a 1992 survey that assessed the indirect costs of treating sufferers of joint pain, one health clinic found that the estimated total expenditure for 1 year was $100,000. Because eating large amounts of junk food is associated with being obese and is related to a higher risk for costly medical conditions like joint pain, junk food is a major contributor to the current obesity problem.

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Does that pose become you? Testing the effect of body postures on self-concept

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ABSTRACT
Self-concept expansion predicts a range of adaptive outcomes. An intriguing possible cause of self-concept expansion is the posing of one's body expansively, that is, "power posing." In Study 1 ($N = 65$), we found that body expansion had an effect, of moderate magnitude ($d = 0.58$), on self-concept size in college women as measured by the Twenty Statements Test. Participants who were randomly assigned to hold expanded poses (vs. contracted) – under the guise of a cover story about holding different body positions to test the accuracy of wireless electrodes – wrote significantly more self-statements than those who assumed contracted positions. In pre-registered Study 2 we tested whether this finding was replicable and extended this research by aiming to characterize the process by which it occurred. One hundred and twenty-eight women students were randomly assigned to hold either expanded or contracted postures. They completed surveys measuring two general classes of potential mediators ("broaden-and-build" and "narrow-and-disrupt"), body self-objectification as a moderator, and four indices of self-concept size. Posture was not found to affect self-concept size, nor was it moderated by self-objectification. Though there was no effect on self-expansion, in exploratory analyses, assigned posture affected one of the broaden-and-build measures: psychological flexibility. Results of Study 2 could indicate that a mere two minutes of holding an expanded versus contracted body posture is not enough to induce changes in self-concept size; lack of main effects could in addition be due to a range of unmeasured confounders and/or the fragile and transient nature of the effect.

KEYWORDS
Posture; nonverbal; self-concept; psychological flexibility; self-objectification

Imagine sitting in a job interview having just been asked, "So what else can you tell me about yourself?" – and realizing you have nothing more to say. What if you could easily expand your range of self-descriptors, thus improving your chances for the next opportunity? Beyond shaping how we experience our own body, here we test whether the

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subjective experience of holding an expansive corporeal form, even briefly, can actually expand the accessibility of one's meaningful, core psychological self.

The array of beliefs describing oneself is self-concept. Self-concept contains descriptors that may include and are not limited to traits, goals, roles, relationships, and situations (McConnell & Strain, 2007; Showers & Zeigler-Hill, 2012) and differs across people, situations, and time (Markus & Kunda, 1986; McConnell, 2011; Richman, Slotter, Gardner, & DeWall, 2015; Showers & Zeigler-Hill, 2012). The structure of one's self-concept predicts a range of adaptive outcomes (Oyserman, Elmore, & Smith, 2012). Self-concept expansion, in particular, has been shown to heighten persistence on cognitive and physical tasks (Mattingly & Lewandowski, 2013a), increase self-efficacy (Mattingly & Lewandowski, 2013b), and predict job satisfaction and commitment (McIntyre, Mattingly, Lewandowski, & Simpson, 2014). Self-concept can expand or contract as a function of romantic and other interpersonal relationships and roles (Aron, Paris, & Aron, 1995; McIntyre et al., 2014; Slotter, Gardner, & Finkel, 2010; Showers & Zeigler-Hill, 2012), as well as intra-psychic drivers such as novelty or interest (Mattingly & Lewandowski, 2014).

An intriguing other possible cause of self-concept expansion may be the posing of one's body expansively, colloquially known as, "power posing." Carney, Cuddy, and Yap (2010) found that holding one's body briefly in expanded or contracted postures changed not only one's bodily self, but also one's conceptual self, related to self-reported states such as feeling powerful. These findings are congruent with psychological theories of embodiment, which focus on how "higher level processing is grounded in the organism's sensory and motor experiences" (Winkielman, Niedenthal, Wielgosz, Eelen, & Kavanagh, 2015, p. 151). However, structure of self-concept (e.g. size, other content) was neither central to Carney et al. (2010) nor measured as a consequence of body expansion in that or in subsequent studies (Cuddy, Wilmuth, Yap, & Carney, 2015; Fischer, Fischer, Englich, Aydin, & Frey, 2011; Huang, Galinsky, Gruenfeld, & Guillory, 2011; Park, Streamer, Huang, & Galinsky, 2013).

Findings from studies to replicate body expansion effects have not been entirely consistent. As Carney, Cuddy, and Yap (2015) note in a recent review of expansive versus contractive nonverbal displays, a few key moderators that might account for the inconsistencies across studies include participant awareness of the hypothesis, length of time holding the poses, involvement of social tasks during the manipulation, and experimenter bias. Thus, we do not expect that merely adopting an expansive or contractive pose necessarily will change self-concept. This is because expanded postures are imbued with different meanings depending on the social context, roles, and power of actors involved (Tiedens & Fragale, 2003). For example, even while adopting the very same expansive pose (or at least imagining so; Cesario & McDonald, 2013), being frisked by the police as a crime suspect and running a board meeting as an executive are two distinctly psychological experiences. Similarly, being socially excluded (vs. included) can attenuate the effects of expansive postures (Welker, Oberleitner, Cain, & Carré, 2013).

Still, under particular circumstances (e.g. neutral or nonthreatening to the posture holder) striking an expanded posture may be becoming – in the sense that it enhances a person's *presence*, as Cuddy argues in her book by the same title (Cuddy, 2015). But what if, more literally, our postures *become us* – insofar as expanded physical postures expand the size and meaningful content of self-concept. If so, how might this happen? A person holding an open stance and head held high literally has a better view of the world than a closed-armed, head-hung counterpart. The theory of broaden-and-build predicts that positive emotions can lead to increased perceptual awareness, a change that allows individuals to build their cognitive resources and think in more flexible, abstract, and approach-oriented terms (Fredrickson, 2001). Each of these changes can increase the ability to bring novel skills and ideas into one's self-concept, rendering the self correspondingly more complex.

Consistent with this theory, studies replicating Carney et al.'s (2010) power posing experiment to date have found that expansive poses increase confidence-related thoughts (Briñol, Petty, & Wagner, 2009), boost mood (Nair, Sagar, Sollers, Consedine, & Broadbent, 2015), abstract thinking (Huang et al., 2011), and performance and nonverbal presence alike (Cuddy et al., 2015). It is worth noting that a growing literature suggests that self-structure is important to well-being (see Mattingly & Lewandowski, 2013a, 2013b; Mattingly, Lewandowski, & McIntyre, 2014) and that there are benefits of self-complexity (Gresky, Ten Eyck, Lord, & McIntyre, 2005; Linville, 1985, 1987; Rafaeli-Mor & Steinberg, 2002). However, other literature clarifies that it may not be self-complexity as such that is linked to adaptive outcomes but more specifically the personally meaningful and authentic characteristics (see Ryan, LaGuardia, & Rawsthorne, 2005; Schlegel, Hicks, Arndt, & King, 2009) and psychological flexibility (Kashdan & Rottenberg, 2010) accompanying phenomena such as (conceptual) self-expansion.

Conversely, do contractive postures result in a type of myopia that not only physically impedes one's view but also contracts self-concept? We argue that the flip side of "broaden-and-build" is "narrow-and-disrupt" processes that may be at work. Contractive postures are strong indicators of low social status (Martens, Tracy, & Shariff, 2012; Tiedens & Fragale, 2003). Seeing oneself as low social status has been shown to narrow and distort cognitive processing via ruminative coping (Jackson, Twenge, Souza, Chiang, & Goodman, 2011) which in turn impairs a host of self-concept shaping processes: problem solving, instrumental behavior, and social support (Lyubomirsky, Layous, Chancellor, & Nelson, 2015). Additionally, internalizing low social status (akin to integrating it into self-concept) thwarts the fulfillment of basic psychological needs (Jackson, Richman, LaBelle, Lempereur, & Twenge, 2014), fundamentally compromising self-flourishing and expansion.

Importantly, trait self-objectification may be a key moderator, amplifying the harmful effects of contractive poses. Studies demonstrate that situations inducing self-objectification cause nonverbal withdrawal in social situations (Saguy, Quinn, Dovidio, & Pratto, 2010) and compromise higher-order thinking for females in particular (Fredrickson, Roberts, Noll, Quinn, & Twenge, 1998; Quinn, Kallen, Twenge, & Fredrickson, 2006), though growing research is also demonstrating a range of negative outcomes for men (for review see Moradi & Huang, 2008). Because objectification of female bodies is pervasive, accompanying self-objectification profoundly affects myriad negative psychological outcomes (Moradi & Huang, 2008) – to the point of being likened to "psychological cliterodectomy" especially for females living in Westernized countries (Grabe, 2013), and is thus a crucial variable to consider in the phenomenology of embodiment.

Current investigation

There are little data so far about whether the expansion of the body is experienced as an expansion of self. Here we seek to test several boundary conditions of power posing. Specifically, we test an outcome novel to this literature: whether power posing can affect self-concept. Because gender can affect how body postures are subjectively experienced from the inside (e.g. internal proprioceptive feedback; Roberts & Arefi-Afshar, 2007), we will conduct our examination in an all-female sample (Allen, Gervais, & Smith, 2013 is among the few with all-female samples in this literature) and test trait self-objectification as a moderator.

Beyond these extensions, we otherwise plan to closely replicate the manipulation used by Carney et al. (2010). Specifically, we will retain the same cover story (testing physiological sensors), similar affective context (no intentional induction of strong emotions, such as the Trier Social Stress Test; cf. Nair et al., 2015), amount of time the poses are held (2 min total per condition, i.e. 2 poses for 1 min each), experimenter presence during posing (not in the room, but videotaping), distractor task during the manipulation (paying attention to faces), and general population (drawn from campus settings, meaning they are status-primed by virtue of being in higher education).

Experimental aims and hypotheses

Our primary aim is to test if size of postures (expanded v. contracted) cause differences in size and content of self-concept. Given that self-expansion has been shown to result from increased positively valenced self-concept content (Mattingly et al., 2014; McIntyre et al., 2014), we hypothesize that expanded postures will activate more meaningful self-concept content and yield relatively greater positive self-concept size. If our main hypothesis is confirmed, we will explore potential mechanisms accounting for this link. We hypothesize a dual-process model, such that expanded postures increase positive self-concept size and meaning via a range of broaden-and-build processes. Furthermore, contracted postures should activate self-contraction, which is the loss of positive self-concept content (Mattingly et al., 2014; McIntyre et al., 2014). Thus we also predict that a complementary host of "narrow-and-disrupt" processes reduces positive self-concept size. See Appendix 1 for constructs and predictions about which condition will have higher scores for each variable. Additionally, we hypothesize that higher trait self-objectification will attenuate the benefits seen in the expanded postures condition and amplify the decrements seen in the contracted postures condition. Finally, we will test for potential confounding by verbal response style (Mattingly & Lewandowski, 2014).

Study 1

Participants and design

This non-registered pilot study included 65 participants as part of a larger investigation examining the effects of posture on a variety of outcome measures. Participants were female, between 18–27 years old, and not taking hormonal birth control or hormonal supplements. Approval to conduct the research was obtained from the Institutional

Review Board of the college campus at which the participants were recruited. Students participated in the study in exchange for $10 or credit in a psychology course. Student of color organizations were included in recruitment efforts toward generating a racially/ethnically diverse participant pool. The racial/ethnic distribution of the sample was approximately 42% White, 40% Asian heritage, 12% Black, 5% mixed heritage, 2% Latina.

This was a between-groups experiment in which we manipulated one independent variable with two levels. Participants were placed in either expanded (head up, arms away from the body) or contracted (head down, limbs close to the body) poses (2 poses for 1 min each) identical to those used in Carney et al. (2010). Participants were then asked to complete a range of measures including one about self-concept, which is our dependent variable of interest. (Measures not part of the current investigation are not included in this article; data not shown.)

Procedure

We scheduled individual laboratory sessions. After giving informed consent, participants were told they would be helping the laboratory test new wireless heart rate monitors, specifically investigating whether the monitors worked adequately when the body was placed in different positions (cover story adapted from Carney et al., 2010). The main experimenter placed wireless leads on both calves and the inner arm of the participant's nondominant hand and verbally instructed the participant into the first pose (either contractive or expansive), which was a seated position.

As the main experimenter was leaving the room, a video camera was set up by a second experimenter, who then left so the participant was alone during the bulk of the posing time. The purpose of the video recording was as a manipulation check to ensure the participant held the pose correctly, and this was explained to the participant during the session. While assuming the pose, participants completed a task requiring them to view a series of faces showing different emotions. Faces were adapted from materials used in Carney et al. (2010). For each of the two posture holds per condition, nine different faces in succession appeared on the screen over the course of 1 min. Images were set to automatically advance after ~5–7 s intervals timed in a Power Point presentation viewed on a laptop sitting on a desk in front of the participant. After the first pose was held for a minute (timed by the experimenters), the experimenter re-entered the room and verbally instructed participants into the second, standing, position. The experimenter then left and participants again completed the faces filler task, with a different set of nine faces but otherwise same as before, while holding the pose for 1 min. At the end of the second posture, the experimenter instructed participants to come out of the pose and complete the measures, which included the self-concept measure.

Materials

We assessed self-concept size using the Twenty Statements Test (TST; Kuhn & McPartland, 1954). The TST asks participants to respond by filling up to 20 blank lines with their answers to the following prompt modified for our study to read:

> In the twenty blanks below please make twenty different statements about yourself that complete the sentence "I am _________." Complete the statements as if you were describing yourself to yourself, not to somebody else. Write your answers in the order they occur to you. Don't worry about logic or "importance." It's okay if you don't fill them all in.

Responses were later coded for analysis by noting the number of statements each participant completed, which served as the dependent variable. Study materials, including raw data, can be found in Appendix 3 and at https://osf.io/g85ep/?view_only=a4bc9c796ae347b08c4188251cebfe85.

Results and discussion

We used an independent samples *t*-test to compare self-concept size for expanded and contracted posers. There was a significant difference in the number of statements made between the expanded (n = 32, M = 18.2, SD = 3.25) and contracted (n = 33, M = 16.0, SD = 4.10) posing conditions, $t(63) = -2.31$, $p = .024$, 95% CI[−3.97, −0.29]. The magnitude of the difference between the means revealed a medium effect size (Cohen's d = 0.58).

We next used a one-way between-groups analysis of covariance to determine if the postural effect on self-concept size would persist beyond demographic information. Age, class year, and highest education of each parent were entered as covariates in the analysis. With their inclusion, the difference between the number of statements made by those placed in expanded (n = 32, M = 18.0, SE = 0.66) versus contracted (n = 32, M = 16.2, SE = 0.65) positions was attenuated but remained, $F(1, 59) = 3.99$, $p = .050$, $\eta^2_{\text{partial}} = .063$. The magnitude of the difference remained a medium effect size (Cohen's d = 0.50).

Our results from Study 1 indicate that "power posing" has an effect, of moderate magnitude, on self-concept size. Participants placed in the contractive postures wrote significantly fewer self-statements than those who assumed expansive positions. Study 1 provides proof-of-principle that "expanded postures expand the self" to the extent that participants who briefly held expanded (vs. contracted) poses reported more self-concept descriptors.

In Study 2 we will test whether this finding is replicable. We will again use the same laboratory experimental paradigm. Furthermore, we extend Study 1 by adding additional measures of self-concept, including one that relies on a checklist rather than spontaneous generation of content; two nonverbal measures of self-concept size; and beyond self-concept size, assessments emphasizing authentic self-concept. If the effect of posture on self-concept is replicated, we will conduct two sets of exploratory analyses to clarify the nature of this association. Specifically, we will test if trait self-objectification modifies the effects of posture on self-concept. Finally, we will explore potential mediators of a posture–self-concept link. We suspect this link, if it indeed is a true association, is mediated by both "broaden-and-build" processes activated by holding expansive postures and "narrow-and-disrupt" processes activated by assuming constrictive ones. To rule out whether the main finding regarding posture–predicting self-concept size, should it emerge, is confounded by verbal response set we will include a corresponding indicator to be tested as a potential covariate.

Study 2

Participants

We plan to recruit 128 students, identifying as female, from college campuses in Western Massachusetts, United States. Participants will be pre-screened to ensure they do not have any injuries to their extremities and will therefore be able to correctly hold the two poses. Participants will receive either a $10 gift card or course credit for their participation in the study. Recruitment will include student of color organizations to ensure a racially/ethnically diverse subject pool.

Video recordings of the postures will be examined immediately after the laboratory session. Participants will be excluded if they fail to hold one or both of the poses correctly. In the data collection phase, recruited participants will be replaced only if they are excluded for such a technical error. In the pilot study, compliance to the instructions for posing was excellent; no participants were deemed in the video review to have failed properly holding the poses.

Procedure

We will recruit participants via paper, email flyer, and snowball sampling for participation in a 60-min experimental study on postures. After expressing interest via email to be part of the study, potential participants wishing to continue will give consent and complete a brief online survey confirming that they fit all of the study criteria and measuring trait self-objectification (instrument explained later). This completed initial survey will prompt scheduling of an individual session to complete the in-laboratory experiment at a later date. Once in the laboratory, participants will be told they are aiding laboratory staff to test new wireless physiological monitors, specifically whether the heart rate monitors work effectively when the body is placed in different positions (cover story adapted from Carney et al., 2010). After obtaining informed consent for the laboratory portion, we will randomly assign participants to either the expansive or contracted poses condition. The main experimenter will be naïve to condition but will inform the participant of the brief slideshow of female and male faces portraying varying emotions as described in Study 1, telling participants that identifying emotions can affect heart rate and will thus indicate if the sensors are working properly. The experimenter will then exit the room. A lab technician will enter and place four electrode pads on the participant – on the inside of both calves and on the inside of both arms.

Participants will follow verbal instructions of the same length for each condition from the lab technician on how to assume the first position and will be asked to hold the pose for 1 min.

> *Expansive Pose 1*: "Please stay seated, and put your feet crossed, on the table with toes above heart level. It's ok for knees to bend. Put your hands behind your head. Interlace fingers, elbows moving in line with your ears, so the sensor is above heart level. Tilt your head slightly up but make sure you can still see the computer screen comfortably. Are you ready?"
>
> *Contractive Pose 1*: "Please stay seated and put your knees together and feet together on the ground. Fold your hands with your non-dominant hand over the other one, and place them

> in your lap, so the sensor is right about at hip level. Tilt your head slightly down; make sure you can still see the computer screen comfortably. Are you ready?"

During this time, the lab technician will direct the participant's attention to the computer screen and then exit the room. After the minute, the lab technician will re-enter the room and give instructions for assuming the second pose.

> *Expansive Pose 2*: "Please stand up facing the table. Take a step forward, with your dominant foot in front. Place your finger tips on the table, a little wider than shoulder-width apart, and your fingers slightly spread for support. Again, tilt your head slightly up, but make sure you can still see the computer screen comfortably. Are you ready?"
> *Contractive Pose 2*: "Please stand up facing the table. Cross your legs; it does not matter which leg is in front of the other. Place your non-dominant hand on the opposite arm and your dominant hand on the opposite side of your torso. Again, tilt your head slightly down, but make sure you can still see the computer screen comfortably. Are you ready?"

As before, participants will hold this pose for 1 min while completing the faces filler task. After completing the manipulation, the participant will be asked to complete questionnaires. Upon finishing the measures, participants will be thoroughly debriefed, thanked, and compensated for their time.

Measures

All measures will be adapted so stems prompt participants to describe their thoughts and feelings "in the moment." Self-concept measures and mediators will be block randomized to minimize order effects.

Self-concept size

Participants will complete a TST (Kuhn & McPartland, 1954) in which they will be given 20 lines to answer the question "Who am I?" Instructions will read,

> In the blanks below please write answers to the simple question 'Who am I?' Answer as if you were giving the answers to yourself – not someone else. Write your answers in the order they occur to you. It's ok if you don't fill them all in. Describe your true, authentic, deepest self. WHO AM I?

More lines completed will indicate greater size of self-concept. Participants will also complete the Self-Concept Size Checklist (Mattingly & Lewandowski, 2014), which asks respondents to indicate via an extensive checklist of words those they see as self-descriptive. Examples of words used include anxious, blunt, and polite. Higher scores indicate larger self-concept, though to further capture size of the domain capturing specifically the most meaningful and authentic self-descriptors, we will modify the instructions to read

> You will now view a list of traits that describe different kinds of people. Think about each of these traits carefully and let us know which of these traits best describes your true, authentic, deepest self. If a trait describes the 'truest you' please circle it. If a trait does not, leave it uncircled.

As a third measure of self-concept, participants will be given a compass and asked to practice drawing a circle once. Then, again using the compass, participants will draw a circle representing their sense of authentic self, to represent "all of those things that make up who you are as a person" (adapted from Mattingly & Lewandowski, 2013b). The diameter of the circle in centimeter will indicate self-concept size. Last, a mind-mapping task, adapted from Buzan and Abbott (2005), will be used to evaluate self-concept size. For this activity participants will be asked to depict their "self" through nodes and branches. Subjects will be instructed to

> Start in the center of the paper with a word or image that describes your true, authentic, deepest self. Use lines to connect your central word or image to other qualities, roles, or traits that describe your true, authentic, deepest self. You can choose to include as many or as few branches as you like. We'll give you a few minutes to do this.

Higher numbers of branches will indicate larger self-concept, specifically those true, authentic facets of the self that we aim to capture with all of our self-concept measures.

Broaden-and-build processes

Participants will complete a 21-item basic psychological needs satisfaction scale (Gagné, 2003; Johnston & Finney, 2010) containing items measuring autonomy, competence, and relatedness. Sample items include "I feel like I am free to decide for myself how to live my life" (autonomy), "Most days I feel a sense of accomplishment from what I do" (competence), and "I really like the people I interact with" (relatedness). Participants will respond to each item with a scale from 1 (not at all true) to 7 (very true), with higher scores indicating higher psychological needs fulfillment, which we hypothesize frees up correspondingly more attentional resources. Cronbach's alpha for the 21-item measure was high in a comparable sample (.95, Jackson et al., 2014).

Participants will complete a question asking how powerful and in charge they feel at this moment (Carney et al., 2010). We will also use a modified Subjective Social Status scale (Adler, Epel, Castellazzo, & Ickovics, 2000) to measure how participants view themselves in relation to their community and to the United States as a whole. Participants will be presented with a 10-runged ladder and prompted to select the rung that most closely fits their perceived status. Directions at the top of the page will make explicit the directionality of the ladder (the top of the ladder represents those with the highest standing and the bottom of the ladder those with the lowest standing). Participants will also be asked "How much do you think it's your own doing that you are at the rung you selected?" as a measure of internalization of status, with less internalization an indicator of greater broaden-and-build capacity, as will be greater perceived power and social status.

Participants will complete the Willingness to Communicate scale (McCroskey, 1992) as a measure of how likely they are to initiate communication, which is an interpersonal way of building one's resources. The measure directs participants to indicate what percentage of the time they would choose to communicate in 20 given situations, and adapted for this study, participants will be asked to imagine if they were in these situations given how they are currently feeling. Sample situations include "Talk with a physician" and "Talk in a large meeting of acquaintances." Cronbach's alpha is reportedly high (.92, McCroskey, 1992).

A 20-item Positive Affect Negative Affect Scale (PANAS; Watson, Clark, & Tellegen, 1988) will direct participants to rate from 1 (very slightly or not at all) to 5 (extremely) different emotions depending on how they feel "at the present moment." Sample positive feelings include "interested" and "excited." Reliabilities for the positive affect subscale have been reported to range from .86 to .90 (Watson et al., 1988).

Participants will also complete a six-item Subjective Vitality scale (Ryan & Frederick, 1997), capturing how alive and alert participants feel. They will rate statements based on how they feel in the moment on a scale from 1 (not at all true) to 7 (very true). Sample statements include "I feel alive and vital" and "I am looking forward to each new day." This scale demonstrates good reliability, reported as .84 in Bostic, Rubio, and Hood (2000).

To capture psychological flexibility, the shortened Committed Action Questionnaire (CAQ-8; McCracken, Chilcot, & Norton, 2015) measures committed action as part of the process of flexible persistence in goal-directed behavior. Participants will be asked to rate four negatively and four positively phrased items on a scale from 0 (never true) to 6 (always true). Examples of statements include "I can remain committed to my goals even when there are times that I fail to reach them" (positively phrased) and "If I feel distressed or discouraged, I let my commitments slide" (negatively phrased). The reliability of the scale is reported as high, Cronbach's alpha = .91 (McCracken et al., 2015).

Participants will also complete the Five Facet Mindfulness Questionnaire (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006) which assesses five elements of mindfulness: observing, describing, acting with awareness, nonjudging of inner experience, and nonreactivity to inner experience. With this questionnaire, participants will rate statements such as "When I'm walking, I deliberately notice the sensations of my body moving" (observing) and "When I do things, my mind wanders off and I'm easily distracted" (awareness) on a scale from 1 (never or very rarely true) to 5 (very often or always true). Items have been adapted to encourage participants to imagine these scenarios in the present moment. This scale has shown good internal consistency (Cronbach's alpha .89; Baer et al., 2006).

Narrow-and-disrupt processes

The PANAS described earlier includes a negative affect subscale, which will be used here. Sample negative feelings include "distressed" and "upset." Reliabilities for the negative affect subscales have been reported to range from .84 to .87 (Watson et al., 1988).

We will use the 10-item Ruminative Responses Scale, which asks participants to rate how they react when they are upset (Treynor, Gonzalez, & Nolen-Hoeksema, 2003). The scale has been validated to reflect two factors: reflection and brooding. Sample items include "Go someplace alone to think about your feelings" (reflection) and "Think about a recent situation, wishing it had gone better" (brooding). Responses will be rated on a 4-point scale ranging from 1 (never or almost never) to 4 (always or almost always). The prompt will be modified so that participants were instructed to answer how it applies to them "right now" and for the purposes of this study, we will use the brooding subscale (not reflection) to indicate narrow-and-disrupt processes. The alpha in a community-based adult sample for this subscale was .77 (Treynor et al., 2003).

Participants will be presented with the Self-Objectification Questionnaire (Noll & Fredrickson, 1998) to assess the relative importance of appearance-related (objectified) and competence-related (nonobjectified) attributes. The measure will ask participants to rank order 10 different body attributes from 0 (least impact) to 9 (greatest impact) based on how important each is to self-body concept. Examples of appearance-related attributes include "physical attractiveness" and "firm/sculpted muscles," whereas examples of competence-related attributes include "health" and "physical fitness level." Self-objectification scores will be calculated by subtracting the sum of the ranked competence items. Positive scores represent primary focus on body appearance, whereas negative scores indicate primary focus on body functionality.

As a final narrow-and-disrupt indicator, we will code the TST above for the number of first-person words used by each participant (Tausczik & Pennebaker, 2010), which have been shown to be used more by people in slumped versus upright postures (Nair et al., 2015). More first-person words indicate greater psychological narrowing-and-disrupting.

Demographics

For the purposes of describing the sample, we will collect demographic information from each participant including academic institution, parent education, age, year in school or role on campus, grade point average (GPA) if applicable, race/ethnicity, gender identity, and sexual orientation.

Potential covariates

Verbal response set will be measured for use as a potential covariate, using the Kitchen Tools Checklist (Mattingly & Lewandowski, 2014), which asks respondents to circle all items they believe belong in a well-stocked kitchen (e.g. nutcracker, whisk). The number of items parallels that of the Self-Concept Checklist. The rationale underlying this measure is that some people may check more items not reflecting more self-content, but simply reflecting the tendency to verbalize. Following Mattingly and Lewandowski (2014), we will examine whether the effects of manipulated posture on self-concept size persisted beyond the number of items endorsed on the Kitchen Tools Checklist. Finally, prior exposure to the power posing paradigm could interfere with the strength of the cover story as well as participant responses. Thus we will ask participants if they are familiar with the concept of power posing and if they have seen Amy Cuddy's TED talk on power posing. For manipulation checks, see Appendix 2, Analysis Pipeline, item 1.

Results and discussion

Pre-registered analyses

One hundred and thirty-three female students participated in Study 2. Two participants missing the video data to enable their postures to be verified were excluded from analyses (Table 1). Data were deemed incomplete if participants completed less than 75% of scale items; three participants were excluded for this reason due to clerical error at the time of data collection. These exclusions yielded an analytic sample of 128 participants.

We queried participants on a range of demographic indices. At the time of data collection, most participants (95.3%) were drawn from a college in Western Massachusetts, and they included both undergraduate and graduate students; the

Table 1. Descriptives – manipulation checks, demographics, and potential covariates by experimental condition.

		Posture					
		Contracted			*Expanded*		
Construct	Variable	*n*	%	*M* (SD)	*n*	%	*M* (SD)
Manipulation checks	Postures – Video review	68			65		
	Acceptable	66	97.1	–	65	100	–
	Unacceptable	0	0	–	0	0	–
	No verification	2	2.9	–	0	0	–
	Postures – Self-report	65			63		
	Contracted	57	87.7	–	24	38.1	–
	Expanded	8	12.3	–	39	61.9	–
Demographics	Parent education	65			63		
	Less than a college degree	16	24.6	–	13	20.6	–
	College degree	18	27.7	–	17	27.0	–
	Some graduate school	31	47.7	–	33	52.4	–
	Age	65		20.7 (1.85)	63		20.8 (1.94)
	Year in school	65	–		63		
	First year/Sophomore	17	26.2	–	16	25.4	–
	Junior/senior/graduate	48	73.8	–	47	74.6	–
	GPA	61		3.57 (.28)	63		3.61 (.35)
	Race/ethnicity	65			63		
	White	27	41.5	–	25	39.7	–
	Multiracial	16	24.6	–	16	25.4	–
	Asian/Latina/Black	22	33.8	–	22	34.9	–
	Sexual orientation	65			63		
	Completely Heterosexual	15	23.1	–	16	25.4	–
	Mostly heterosexual	13	20.0	–	30	47.6	–
	Other	37	56.9	–	17	27.0	–
Potential covariates							
Verbal response set	Kitchen Tools Checklist	65		38.9 (11.5)	63		35.6 (12.8)
Exposure to power posing	Know about power posing?	65			63		
	No knowledge of it	51	78.5	–	48	76.2	–
	Some knowledge of it	14	21.5	–	15	23.8	–
Experimenter bias	Experimenter + tech pair	65			63		
	Seasoned + Seasoned	32	49.2	–	29	46.0	–
	Any newbie in the pair	33	50.8	–	34	54.0	–

Postures – Video review is from the data set before any exclusions, $N = 133$. All else are from the main analytic dataset, $N = 128$. Each multinominal variable under consideration as a covariate (i.e. parent education, year in school, race/ethnicity, sexual orientation variable, previous knowledge about power posing, and experimenter + tech pair) was recoded to retain the largest number of categories while ensuring that each cell stratified by posture condition would have at least 10 or more participants.

remaining participants attended five other colleges or universities in the near vicinity and across the southern New England region of the United States. Table 2 presents remaining demographic indices stratified by randomly assigned posture (contracted vs. expanded). On average the sample was composed of college juniors but ranged from first- to ninth-year student in higher education (college or graduate studies), SD = 1.63. Before coming to the laboratory session, all participants indicated that they were female in response to an initial closed-ended screening question (yes/no?); at the end of the session, when asked to write in their gender identity, 96.9% identified as a female, and 3.1% identified as something else (e.g., androgynous, non-binary female-aligned). For analytic purposes so that multinominal categorical variables had at least 10 participants per cell in analyses stratified by experimental condition (see Table 1), we combined categories when possible while also maximizing the number of categories. Otherwise – as with the case of educational institution and gender identity, when one category held 95% or more of participants – we omitted the variable for consideration as a covariate.

Table 2. Associations of manipulation check, demographics, and potential covariates with self-concept size variables.

	Self-concept size variables											
	Twenty Statements Test			Self-Concept Checklist			Drawn circle			Mind map		
	r	*F*	*p*	*r*	*F*	*p*	*r*	*F*	*p*	*r*	*F*	*p*
Manipulation check												
Postures – Self-report	–	0.12	.73	–	0.20	.66	–	0.64	.43		0.010	.92
Demographics												
Parent education	–	0.66	.52	–	1.42	.25	–	0.99	.38	–	0.41	.66
Age	−.002	–	.99	−.11	–	.23	−.030	–	.74	−.085	–	.34
Year in school		0.000	.99		.083	.77		0.061	.81		0.88	.35
GPA	.091	–	.31	−.017	–	.85	.13	–	.17	−.056	–	.54
Race/ethnicity	–	1.12	.33	–	0.37	.69	–	0.18	.84	–	2.82	.063
Sexual orientation	–	0.47	.63	–	0.69	.50	–	0.38	.69	–	**3.16**	**.046**
Potential covariates												
Verbal response set	−.053		.56	.17		.051	.085		.34	−.040		.65
Exposure to power posing	–	1.85	.18	–	2.99	.086	–	2.7	.11	–	0.012	.91
Experimenter bias	–	0.86	.36	–	0.083	.77	–	0.013	.91	–	0.92	.34

Each association was analyzed using data from 128 participants, except for those with GPA, which had 123 participants with data. Correlations were run to examine the association between continuous demographic variables (age, GPA) and each self-concept size variable, respectively. ANOVAs were used when independent variables were categorical. Parameter in **bold** denotes a test where statistical significance was $p < .05$.

We collected pilot data to validate Carney et al. (2010) initial findings (p. 1364) that there were no differences between the poses by condition on comfort, difficulty, or pain. Before doing this, we ensured that the desk and the chair heights minimized difficulty, effort, and pain and more generally, paying attention to the directions we offered participants so the poses were comparable on these dimensions. We collected data within participants (with order of contracted vs. expanded poses randomized) which allowed participants to be their own controls. Paired-samples *t*-tests showed no differences in contracted versus expanded postures on comfort ($t[9] = 0.18$, $p = .87$), difficulty ($t[9] = -0.83$, $p = .43$), or pain ($t[9] = -0.30$, $p = .77$), respectively.

We explored demographic variables as possible covariates by conducting bivariate analyses of each demographic variable with each self-concept size variable (Table 2). For the self-concept variables, we calculated a Pearson's correlation coefficient with each continuous demographic variable (age and GPA), and analysis of variance (ANOVA) with each categorical demographic variable (highest parent education, year in school, race/ethnicity, and sexual orientation). There was not homogeneity of variance across the dependent measures to warrant multivariate analysis of variance. None of the demographic variables were associated (all *p*s > .05) with any of the self-concept variables, except for sexual orientation which was associated with the mind map measure of self-concept (final model: assigned posture, $F(2, 125) = 3.16$, $p = .046$, $\eta^2_{\text{partial}} = .048$); thus it was retained as a covariate.

Additionally, verbal response set, self-reported pose, exposure to power posing, and experimenter bias were studied to see if they were associated with self-concept size (Table 2). As before, a Pearson correlation coefficient with each self-concept size variable was calculated for the continuous variable (Kitchen Tools Checklist); with the remaining variables – which were all categorical (self-reported pose, exposure to power posing, and the experimenter + tech pair) – ANOVA was performed. All associations exceeded $p > .05$. None of these variables were retained as covariates.

The four self-concept size measures were then examined for outliers, defined as values greater or lesser than three SDs from the mean. Outliers were found for two variables: the number of statements completed on the TST and the number of branches present in the mind maps. The values in the bottom and top 5% for each of these measures were subject to a 90th percentile Winsorization, which required changing these extreme values to the 5th and 95th percentile values, respectively (Aguinis, Gottfredson, & Joo, 2013, p. 279). Each of the mediator and moderator variables were also checked for outliers, resulting in a similar 90th percentile Winsorization of the PANAS negative affect subscale. After this process, the self-concept variables were checked for violations of normal skew and kurtosis (absolute values greater than 2; Osborne, 2002). All of them were normally distributed.

To finally begin the direct tests of our pre-registered hypotheses, we tested the hypothesis that participants who were assigned to hold expanded (vs. contracted) bodily postures would have higher self-concept size. Data for the four self-concept size measures were standardized to *z*-scores and then summed to create a composite self-concept size score. An independent samples *t*-test revealed no significant differences between contracted and expanded postures on this composite, $t(126) = -0.59$, $p = .56$, 95% CI[–0.29, 0.15]); including sexual orientation as a covariate did not change the findings (final model: assigned posture, $F(1, 125) = 0.77$, $p = .38$, $\eta^2_{\text{partial}} = .006$).

The last of our pre-registered analyses was to explore whether self-objectification moderated the link between posture and self-concept size change. Even though we did not observe our hypothesized main effect of posture, testing the effect modification was warranted; for example, in the case of a cross-over interaction there could be no main effects but a significant interaction. Hierarchical linear regression was used to explore the association between pre-manipulation trait self-objectification (collected via an internet survey that was part of scheduling participants for the in-person laboratory session) and self-concept size. Variables were entered in three steps: assigned posture, adding mean-centered trait self-objectification, and finally including an interaction term of the two. In neither the preliminary nor final models did any of the parameters predict the composite self-concept size variable (final models: assigned posture ($B = 0.063$, SE = 0.11, $\beta = 0.051$, 95% CI [–0.16, 0.28], $p = .57$); trait self-objectification ($B = 0.001$, SE = 0.006, $\beta = 0.023$, 95% CI [–0.011, 0.013], $p = .85$); and interaction term of assigned posture by self-objectification ($B = 0.003$, SE = 0.009, $\beta = 0.049$, 95% CI [–0.014, 0.020], $p = .70$).

Unregistered exploratory post-hoc analyses

Data from Study 2 did not support the idea that self-concept size expands as one holds an expanded posture. As there were no effects to mediate, we did not pursue examining the "broaden-and-build" and "narrow-and-disrupt" processes as mediators as proposed in the pre-registration. While these processes might not be mediators of self-concept expansion they could still be outcomes of body posture in their own right.

To explore this idea, in unregistered analyses we examined the "broaden-and-build" and "narrow-and-disrupt" variables as potential outcomes of postural expansion. We found that psychological flexibility was higher among participants holding expansive

Table 3. Main variables of interest by experimental condition.

		Posture			
		Contracted		*Expanded*	
Construct	Variable	*n*	*Mean* (SD)	*n*	*Mean* (SD)
Main dependent					
Self-concept size	Twenty Statements Test	65	14.8 (4.0)	63	15.3 (4.4)
	Self-Concept Checklist	65	47.5 (17.8)	63	49.8 (15.8)
	Drawn circle	65	14.1 (5.1)	63	13.7 (3.9)
	Mind map	65	15.9 (8.2)	63	16.8 (9.4)
	Composite (of above)	65	−0.032 (.617)	63	0.033 (.64)
Potential moderator	Trait self-objectification	65	4.8 (13.3)	63	5.7 (13.1)
Potential mediators					
Broaden-and-build	PANAS – Positive	65	2.49 (.84)	63	2.67 (.76)
	Vitality	65	4.27 (1.22)	63	4.49 (1.15)
	Basic psychological needs	65	5.20 (.70)	63	5.22 (.75)
	Mindfulness	65	3.23 (.47)	63	3.25 (.43)
	Psychological flexibility	65	4.34 (.86)	63	4.62 (.76)
	Willingness to communicate	65	60.6 (17.0)	63	62.0 (17.6)
	Perceived power	65	2.68 (.69)	63	2.71 (.71)
	Subjective Social Status (SSS)	64	5.61(1.81)	63	5.98 (1.56)
	SSS Internalization (reverse)	64	3.98 (.95)	63	3.90 (1.08)
	Composite broaden-and-build	65	−0.55 (5.97)	63	0.57 (4.95)
Narrow-and-Disrupt	PANAS – Negative	65	1.42 (.39)	63	1.41 (.39)
	Perspective: first person	65	4.29 (6.64)	63	6.71 (7.47)
	Ruminative coping	65	2.46 (.54)	63	2.48 (.57)
	Composite narrow-and-disrupt	65	−0.17 (1.99)	63	0.17 (2.10)

Each variable was analyzed with 128 cases, except for subjective social status and subjective social status internalization, which had 123. SSS (Subjective Social Status) Internalization was reverse-coded.

poses (Table 3, consistent with what we expected, $t(126) = -1.99$, 95% CI[−0.57, −0.002], $p = .048$). There were no additional effects of postural condition on any of the broaden-and-build or narrow-and-disrupt processes. Though theoretically consistent with our hypothesis generally, it is but one of an array of analyses exploring a more particular broaden-and-build mechanism. As an unregistered analysis it should be interpreted with care, but it may offer a clue toward future research about which broaden-and-build mechanisms to study downstream of expanded body postures.

Additionally, and not surprisingly, self-reported pose post-manipulation was associated with assigned pose. Perhaps more surprising was the fact that a notable subset of participants (25%!) demonstrated a mismatch between the poses they were assigned to hold – verified by video to have been performed correctly – and their perceptions of the poses (contracted vs. expanded) that they were assigned. The mismatch was statistically significant as shown by differences in perceived pose as a function of assigned pose, $\chi^2 = 33.9$, $p < .001$.

To examine if expanded postures affect psychological flexibility as a function of perceived condition, we ran moderator analyses set up similarly to those previously described for self-objectification. Variables were entered in three steps: assigned posture; perceived posture, endorsed after the experiment was complete (contracted vs. expanded); and finally an interaction term of the two. Perceived posture (final model: $B = 0.63$, SE = 0.30, $\beta = 0.37$, 95% CI [0.034, 1.23], $p = .038$) – specifically seeing oneself as having engaged in an expansive pose even when having been assigned and verified to have done a contracted one – independently predicted psychological flexibility above and beyond the effects of assigned posture; in the multivariate model assigned posture did not predict the outcome (final model: assigned posture, $B = 0.20$, SE = 0.20, $\beta = 0.12$, 95% CI [–0.19, 0.59], $p = .31$), neither did their interaction (final model: perceived pose by assigned posture interaction, $B = -0.37$, SE = 0.37, $\beta = -0.21$, 95% CI [−1.09, 0.36], $p = .32$). Finally, we went back to Study 1 to see if there too was notable mismatch of the same type. Similar to Study 2, in Study 1, 20% of participants assigned to hold a contracted or expansive posture while post-manipulation endorsing instead that they actually held the other pose. The difference was also statistically significant as verified by χ^2 analyses = 25.2, $p < .001$.

General discussion

Taken together, we did not find compelling support that randomly assigned posture affects self-concept size. Though Study 1 ($N = 65$) demonstrated an effect of assigned posture on the number of TST responses generated, we did not replicate that finding in a better-powered Study 2 ($N = 128$) nor did it generalize to a self-concept size composite measure composed of three rather different self-concept size measures: a checklist, a compass drawing symbolic of the self, and the number of branches generate on a mind map describing oneself.

While previous research has shown effects of posture on outcomes including hormones (Carney et al., 2010), mood (Nair et al., 2015), and performance and presence (Cuddy et al., 2015), our key hypothesis that "expanded postures expand the self" remains unconfirmed. It may be that a one-time manipulation or the nature of our particular manipulation is not as potent as other factors that lead to self-expansion, such as falling in love (Aron et al., 1995), or that the effect, if it exists, is simply not robust enough to persist more than a few minutes in a laboratory session.

Post-hoc analyses suggested that instead of affecting self-concept, type of posture affected one particular psychological mechanism that broadens and builds psychological resources – psychological flexibility – and not a wider array of broaden-and-build indicators. There was no effect on the narrow-and-disrupt measures while controlling for assigned pose. Additionally, in post-hoc analyses, we found a mismatch between assigned and self-reported pose, suggesting that the postures assigned as expansive and contractive were not always phenomenologically so for participants. Because the effect on psychological flexibility was driven by perceived posture beyond the effects of assigned posture, this mismatch might indicate that the effects are due to a placebo, at least in part. In exploratory analyses, we went back to Study 1 and found a similar pattern of mismatch between assigned pose and participants' perceptions of that pose, so this mismatch could be worth further consideration. Other researchers (Cesario & McDonald, 2013) have highlighted the importance of context in shaping the effects of body posture, noting that identical postures show divergent effects when participants are asked to interpret them in different ways.

The strengths of the current investigation include being an experimental procedure with a standardized protocol: experimenters were trained carefully and followed very specific scripts to ensure high uniformity across participants within each condition. The highly scripted protocol also helped minimize experimenter bias. We recruited a strong team of multiple experimenters to avoid burnout or boredom from running multiple long sessions and carefully kept them naïve to condition before and during the protocol. By including numerous self measures in Study 2, we attempted to assess self-concept size from multiple perspectives, thus gaining a more complex picture of this construct than Study 1 could provide. Another strength of the experiment was the pilot testing done on the poses used during Study 2. By ensuring that the expanded and contracted conditions did not differ on comfort, difficulty, or pain, we could with some confidence rule out the effect of these variables on the manipulation. Additionally, by using an all-female sample we were able to minimize the confounding effects of gender as well as study a group for whom effects of power posing as such have not been explicitly and extensively characterized. Self-objectification has a rich base of theory and empirical findings over the past couple decades; as a field focused on the body, it is sensible to bring it into more explicit conversation with research on postures and status, and vice versa.

In many ways our efforts to adequately replicate Carney's 2010 methodology were successful. Our cover story matched the one used in the original Carney et al. study and allowed us to assess the effects of the posing without participant knowledge of the connection between the manipulation and the survey measures. We retained a similar, neutral affective context; kept our poses to 1 min each; used a similar distracter filler task during the posing; and had the experimenter leave the room during this time to minimize attention drawn to the manipulation. Additionally, we used a similar population of participants drawing from the local student community.

Despite our goal to exactly replicate our pilot study, there were a number of initially small changes that ultimately could have affected our ability to observe the same effects. In our interest to test potential mediators we may have undermined our ability to detect an effect of posture on self-concept. There were eight mediator measures preceding the four self-concept size measures; on average the mediators took about 9 min to complete, ranging from about 4 to 17 min. Moreover, three of the four self-concept measures required a fair amount of effort given that they were open-ended. The time between the manipulation and the self-concept size measures was relatively long; perhaps the effect of posture is more transitory than we anticipated.

Additionally, the TST was altered in Study 2. Participants were given only 5 min, instead of the original 8 in the pilot study to complete the task. As well, about 40% of our Study 2 sample had heard of "power posing" a phrase that was nowhere nearly as common in popular discourse during Study 1 data collection, which was in early 2011. Because of the current pervasiveness of the concept, as well as the popularity of Amy Cuddy's 2012 TED talk on power posing, there could be a history effect at work lessening the potency of the cover story. Regarding self-objectification, we measured the trait form as a moderator for Study 2; however, it may be that if there were effects, state self-objectification would be the more salient body-consciousness indicator with situational body focus (as in a power posing manipulation), or some combination thereof (Quinn et al., 2006).

Future investigations could try for a more direct replication of our pilot study, cutting down the time of the experimental session and looking solely at the direct effects of posing on our four self-concept size measures. A larger sample size would be beneficial. The small sample size in the pilot study, from which our original effect size was derived, might have served as an inadequate indicator of the sample size needed to see an effect in Study 2. A closer look at our data in Study 2 suggests high variability in our dependent measures, especially the composites. This is perhaps not surprising, given that they are composites, but in our efforts to block randomize the order of survey administration (within the eight mediator measures and four self-concept size measures) to minimize order effects, we may have inadvertently introduced a high degree of variance that washed out experimental effects, especially ones that are delicate and transient. For example, if someone reads an extensive checklist before generating original content for a TST or a mind map about oneself, the experience of the open-ended questions is vastly different from someone who reads it afterwards. Theoretically, randomization would balance out the order effects, but in the process of a long survey protocol following the manipulation, experimental effects if there were any may too have been attenuated. Last, an experimenter protocol with no cover story would reduce variability in how participants interpret their body positions, which could lead to an easier detection of the main effect.

Although our study did not result in the differences in self-concept size that we hypothesized, there is still much to examine in the realm of postural expansion. Our findings on differences in psychological flexibility point to some other process being altered by manipulating body position. From this study we cannot say that holding a pose becomes you. Future research can determine whether by expanding one's body a person can become more psychologically flexible – perhaps even more helpful in acing a job interview than having an expanded self-concept.

Disclosure statement

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Appendix 1. Constructs, variables, and predictions about which condition will yield higher means

		Posture with higher mean	
Construct	Variable	*Contracted*	*Expanded*
Main dependent			
Positive self-concept	Twenty Statements Test		X
	Self-Concept Checklist		X
	Drawn circle representing the self		X
	Mind map of self		X
Potential moderator	Trait self-objectification		
Manipulation checks	Postures – Video review		
	Postures – Self-report		
Potential covariates			
Verbal response set	Kitchen Tools Checklist		
Manipulation check	Postures – Self-report		
Exposure to power posing	Know of or seen Cuddy TED talk?		
Experimenter bias	Experimenter + tech pair		
Potential mediators			
Broaden-and-build	PANAS – Positive		X
	Vitality		X
	Basic psychological needs fulfillment		X
	Mindfulness		X
	Psychological flexibility		X
	Willingness to communicate		X
	Perceived power		X
	Subjective Social Status (SSS)		X
	SSS Internationalization (reverse)		X
Narrow-and-disrupt	PANAS – Negative	X	
	Perspective: 1st person words in TST	X	
	Ruminative coping	X	
Demographics	Parent education		
	Age		
	Year in school or role on campus		
	GPA if applicable		
	Race/ethnicity		
	Gender identity		
	Sexual orientation		

SSS Internalization will be reverse-coded.

Appendix 2. Analysis pipeline

Once we complete data collection, analyses will be conducted in the following order:

(1) *Exclude* necessary participants, based on
   (a) Manipulation check failures (exclude entire participant data)
      (i) Two types of manipulation checks will be done.
         (1) Only the first one – the video–camera-based check of participants assuming the correct poses – will be used to exclude participants who fail to produce the correct poses.
         (2) As part of a second manipulation check we will query participants about which pose they thought they were holding. Responses to this question will yield a self-report manipulation check variable that will be used as a potential covariate.

(b) Incomplete data
    (i) Scales calculated by averaging scores of items must include at least 75% of the items to be considered complete; they otherwise will be deemed missing.

(2) *Examine* demographics and other potential covariates
  (a) Determine if any demographic variables or other potential covariates (Appendix 1) are associated $p < .05$ with self-concept size outcomes. If so, retain as covariates.

(3) *Examine* dependent variables
  (a) Determine if there are outliers, operationalized as values 3 SDs from the mean. If so, we will perform a 90th percentile Winsorization, transforming all data below the 5th percentile to the 5th percentile, and transforming all data above the 95th percentile to the 95th percentile (Aguinis et al., 2013, p. 279).
  (b) Determine if the data are normally distributed, operationalized as skew and/or kurtosis < 2.0. If not, perform natural log transformation to improve normality (Osborne, 2002).

(4) *Testing* our hypotheses.
  (a) We will start by testing the direct effect of posture on self-concept using *t*-tests:
    (i) To examine if there is a significant direct effect of posture on positive self-concept size, we will perform an independent samples *t*-test.
      (1) The independent variable will be posture (expansive vs. contracted).
      (2) The dependent variable will be a single composite of the four measures of positive self-concept as listed in Appendix 1, derived by standardizing and then averaging the scores.
  (b) In addition, we will re-run this test using ANCOVA to determine those variables as covariates that were significantly associated with positive self-concept size (see 2 earlier). Based on the pilot study we do not expect to be using more than two covariates to be tested in this manner.
  (c) Power calculations
    (i) Based on preliminary data derived from our pilot study, we expect a medium effect size (d = 0.50). Using the software tool g-power (Faul, Erdfelder, Lang, & Buchner, 2007) and aiming for a power of 0.80 with an alpha of 0.05, we computed a required N of 128 for a two-group design, that is, n = 64 participants per group.

(5) *Testing* whether the association of posture and self-concept is moderated by self-objectification using hierarchical linear regression
  (a) To test whether trait self-objectification moderates the association between pose and positive self-concept size, we will compute hierarchical linear regression analyses entering posture (expanded vs. contracted), trait self-objectification, and the interaction term of status and objectification as predictors of the DV indicating positive self-concept size.
  (b) Power calculations:
    (i) Using conservative estimates of small-to-medium effect sizes (f^2 = 0.25) for the two predictors posture and self-objectification, and a small additional

effect of the interaction of posture and self-objectification (R^2 change of 10% equaling an f^2 = 0.11), the required sample size computed using g-power (Faul et al., 2007) is N = 90.

(6) *Exploring* whether the association of posture and positive self-concept size is mediated by any of the proposed mediators:
   (a) Potential mediators: broaden-and-build processes, and narrow-and-disrupt. See also Appendix 1 for a complete list of variables.
      (i) Variables from each of these two classes of mediators will be factor analyzed to create an indicator for each
         (1) Do either of these, individually or in tandem, mediate the posture-positive self-concept size link?
         (2) Power calculations: There is less consensus on a priori power calculations for mediation models. We decided to first estimate the required sample size for a simple mediation model based on a medium effect following Thoemmes, MacKinnon, and Reiser (2010). Their results show that for such a mediation model, a sample of N = 92 would be sufficient to detect medium effects.
         (3) For mediation models in which we will be able to test simultaneously each class of proposed mediator, and in addition control for covariates (if applicable), we will use bootstrapping using the PROCESS macro in SPSS (Preacher & Hayes, 2008).

Our final determination of required sample size is that, based on the required N of 128 calculated for testing the direct effect of posture on self-concept size with an alpha of 0.05 and a power of 0.80 (see aforementioned points), we plan to have 128 participants with viable data in our sample.

Appendix 3. SPSS data set guide

All nonpilot in-person data collection took place after the In Principle Acceptance was received and occurred from 4 April 2016 to 12 July 2016.

A raw data set with all participants (before quality check exclusions) is available, as well as a data set with the final 128 participants and all variables. A third smaller data set is also available which includes the final 128 participants and only the variables used in the analyses outlined by the pre-registered analysis pipeline.

Syntax files with annotations are also available, corresponding to the pre-registered analysis pipeline as well as any additional analyses.

Entries from the TST were de-identified to maintain participant confidentiality. All X's are in place of first or last names.

Key variables

IV:
  Posture (assigned posture of participants, expanded vs. contracted)
DVs:
  IamNum (number of statements completed on the TST)
  IamNumT (Winsorized number of statements completed on the TST)
SCSCTotal (total number of traits circled on the self-concept size checklist)
SDCA (diameter in cm of self-concept circle drawn with compass)

SDMM (number of branches counted in mind map)
SDMMT (Winsorized number of branches counted in mind map)
Demographics:
dAgeT (age of participant)
dSchYrT (year in school)
dGPAT (GPA on a 4.0 scale)
dParEdT (highest parent education level)
dRaceT (categorical race/ethnicity variable)
dSOT (categorical sexual orientation variable)
dGenT (categorical gender variable)
    dColT (categorical school attended variable)
Potential covariates:
KTCTotal (total number of items circles on the Kitchen Tools Checklist)
SRPose (self-reported pose)
PPExpoT (exposure to power posing)
ETBiasT (experimenter bias)
Potential moderator:
SOQTotal (summed self-objectification scale)
Potential mediators:
PAMean (positive affect scale mean)
NAMeanT (transformed negative affect scale mean)
SelfDetMean (basic psychological needs scale mean)
VitMean (vitality scale mean)
FMMean (mindfulness scale mean)
PFMean (psychological flexibility scale mean)
WTCMean (willingness to communicate mean)
FOP (reported feelings of power)
SSSC (community subjective social status)
    SSSIMeanT (reverse-coded mean of subjective social status internalization)
RCMean (ruminative coping scale mean)
    IamI (number of TST statements containing first person words; me, myself, I, or the participant's first name)
    BBComposite (standardized mean of all broaden and build mediators)
    NDComposite (standardized mean of all narrow and disrupt mediators)
Interaction terms:
PosturexSOQ (assigned posture by self-objectification interaction term)
PosturexSRPose (assigned posture by self-reported pose interaction term)

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ARTICLE

Meeting your inner super(wo)man: are power poses effective when taught?

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ABSTRACT

Researchers have argued that power poses are an effective way for individuals to empower themselves to achieve personal goals. However, in laboratory studies, individuals are often unaware of the function of the poses; in real-world settings, participants *must know* the function of the pose in order to deliberately use it. We tested whether power poses are effective when an individual knows their consequences by directly manipulating awareness of the function of power poses. We found no evidence for the overall effect of power poses or an interaction with awareness on both traditional measures of power and a more ecologically valid assessment. Our results suggest the benefits of using power poses outside laboratory settings are limited.

KEYWORDS

Power poses; embodiment; external validity

Powerful people have asymmetric control over valued resources, which enables them to pursue goals and obtain desired outcomes (Huang, Galinsky, Gruenfeld, & Guillory, 2011). Recent work has shown that individuals feel more powerful after using expansive "power" poses (Carney, Cuddy, & Yap, 2010; Huang et al., 2011). More importantly, power poses are purported to lead to better behavioral outcomes in situations like "interviewing for jobs, speaking in public, disagreeing with a boss, or taking potentially profitable risks" (Carney et al., 2010, p. 1367). If true, power poses would give low power individuals an easy way to better achieve their goals. It is perhaps not surprising that this possibility has garnered much public interest including a TED talk watched by over 30 million people (Cuddy, 2012) and a *New York Times* bestselling book (Cuddy, 2015).

Much of the draw of power poses comes from the idea that they may have simple, fast, and effective implications out of the lab. However, this idea has yet to be rigorously tested. In almost all extant research on power poses, participants are not told the function of the pose. If poses are only effective when individuals are unaware of their function, their usefulness outside the lab will be limited. Furthermore, most work on power poses has not examined "real-world" tasks that the poses are purported to benefit, such as speaking publicly, negotiating a deal, or interviewing for a job (but

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Supplemental data for this article can be accessed here.

see Cuddy, Wilmuth, Yap, & Carney, 2015). In the present study, we tested whether power poses are effective in more realistic situations by manipulating knowledge of the function of power poses as well as including tasks that more closely mirror situations that exist outside the laboratory.

Does awareness moderate the effectiveness of power poses?

Power poses have typically been studied under the umbrella of "embodied" psychology, which focuses on how bodily states can influence psychological states and behavior (Barsalou, 2008). From this perspective, posing expansively (e.g. standing with arms akimbo) should increase power because physical size has been closely tied to power throughout evolutionary history and across species (Carney et al., 2010). In support of this idea, researchers have found that expansive poses (vs. constrictive poses) increase feelings of power (Carney et al., 2010; Huang et al., 2011), increase risky gambling behavior (Carney et al., 2010; Huang et al., 2011), increase testosterone and decrease cortisol (Carney et al., 2010), and increase effectiveness at mock job interviews (Cuddy et al., 2015).

Despite converging evidence from these power pose studies, one aspect all these studies share is that participants are unaware of the function of the poses. Instead, participants are typically told elaborate cover stories to hide why the experiment requires them to hold a certain position (e.g. physiological measurement). While this procedure is important for reducing positive results solely due to demand characteristics (Weber & Cook, 1972), for individuals to use power poses outside the laboratory, *they must know the function of the pose*. Yet, no research has tested whether awareness changes the effectiveness of power poses, despite its obvious implication for their practical use.

One possibility is that awareness of the function of power poses may not influence their effectiveness. Consistent with this view, ideas from the embodiment literature have been used to argue for a *direct-effect account*: expansive poses influence power directly and are immune to the influence of other more relevant cues to power. For example, Huang et al. (2011) describe the connection between physical pose and power as so close that posture is "one of the most proximate correlates of behavior and therefore activates power-related behavior directly" (p. 97) and that this link is "so deeply wired into people that it 'mutes' the effect of role when postures are sufficiently salient" (p. 100). Thus, researchers have argued that the benefits of power poses should be unaffected by psychological awareness as well as other power information such as hierarchical role.

However, the argument that power poses can override other power-related information has had only mixed empirical support. Huang et al. (2011) found that expansive (vs. constrictive) poses increased the activation of power-related concepts and led participants to take more risks, regardless of whether they had been assigned to the role of a subordinate or a manager in an unrelated laboratory task. One shortcoming of this study is that they did not attempt to create situations where the combination of role information and pose would naturally occur, making their manipulation of role a highly artificial one. Cesario and McDonald (2013) addressed this issue in an experiment where participants held expansive or constrictive poses and then imagined themselves in a dominant or submissive role that would realistically co-occur with the pose. In this case, only information about role influenced risk taking. These results suggest that power poses might not have a direct connection to power and that current theories based solely on the embodiment literature might be inadequate to understand their effects.

An alternative interpretation of the effect of expansive poses on power comes from the priming literature. Insofar as holding expansive poses prime power-related concepts, the *priming account* suggests that individuals might attribute those cognitions to their own personal feelings, in line with the mood-as-information model (Schwarz & Clore, 1983, 2007). This would lead to the typical finding that those who hold expansive poses (vs. constrictive) feel and act more powerfully. However, if informed that the poses might influence their thoughts and behaviors, individuals might discount those feelings when making judgments or decisions and not act in powerful ways. This would suggest that the effectiveness of power poses would actually be attenuated or even eliminated when participants are aware of the function of the pose.

Although no power pose experiment has directly manipulated awareness, evidence from Ranehill et al. (2015) is consistent with the possibility that awareness eliminates the effectiveness of power poses. Participants were told that the researchers were interested in "whether physical position influences hormone levels and behavior" (p. 654). Under these circumstances, no changes in hormone levels or risky gambling behavior occurred as a function of pose.[1]

In contrast, knowing the function of power poses might actually *increase* their effectiveness. This could occur for a variety of reasons: (1) participants might try to fit their behavior to confirm the effects of power poses (i.e. demand characteristics), (2) power-related concepts may be more accessible and thus more likely to be activated and influence behavior, or (3) there could be a placebo effect (participants *think* that they are more powerful even though the pose manipulation does not have an effect). Based on this logic, the *demand/placebo account* suggests that power poses may be even more effective when participants are aware of their intended function.

In sum, there are at least three different accounts of how awareness might influence the effectiveness of power posing. According to a *direct-effect account*, awareness should not moderate the effects of power poses because of the unmediated link between size and power. In contrast, according to a *priming account*, awareness of the function of power poses might reduce or eliminate their effectiveness insofar as individuals discount the feelings they experience. Finally, a *demand/placebo account* suggests that power poses might be even more effective when participants are aware of their function, through some combination of demand characteristics, increased accessibility of power concepts, and/or placebo effect.

According to the principles of strong inference (Platt, 1964), we manipulated awareness of the function of power poses in order to test which account best explains the data. Having a correct theoretical model that accurately predicts when power poses will be effective is crucial for understanding when they will be effective in realistic circumstances like job interviews, as described next.

Does power pose research have external validity?

Another unanswered question in the power pose literature is whether the current work has the external validity necessary to generalize to real-world situations. In a typical power pose experiment, participants hold an expansive or constrictive pose and then complete a variety of tasks designed to measure power (e.g. risky gambling, abstract thinking). In addition to feeling more powerful, participants typically act in ways consistent with possessing power (e.g. gambling more, thinking abstractly). While these measures clearly indicate that poses influence the *psychological experience* of being powerful, they do not actually assess whether the poses have any *behavioral benefit* in situations such as interviewing for a job, speaking in public, or disagreeing with a boss. This is extremely important considering the strength of the claims that power poses have "real-world, actionable implications" that enable a person to "instantly become more powerful" (Carney et al., 2010, p. 1363).

In one notable exception, Cuddy et al. (2015) measured performance in a mock job interview. They found that participants who posed expansively outperformed participants who held constrictive poses. This evidence is suggestive that power poses might have benefits outside the laboratory. However, given the small sample size associated with this single study (n = 61), there is still a great deal of uncertainty surrounding the reliability, generality, and size of the effect. Furthermore, participants were not told the purpose of the pose, again raising the question of whether the poses are effective in more realistic circumstances where participants *know* what the pose is supposed to do.

The present research

The current research had three goals. First, we tested whether power poses are effective under more ecologically valid circumstances by manipulating whether participants are aware of the function of the pose. This test is crucial considering the mismatch between how power poses are tested in the lab (where participants are unaware of the function of the pose) compared to how they are used outside the lab (with conscious intent). In doing so, we tested which theoretical model best accounted for power pose effects (i.e. direct effect, priming, or demand/placebo).

Second, we tested whether or not power poses directly benefit performance under more realistic circumstances (i.e. in a mock interview), compared to more traditional measures of power from the literature (i.e. feelings of power, risky gambling). Risky gambling decisions have been used in previous power pose research (e.g. Carney et al., 2010; Cesario & McDonald, 2013; Huang et al., 2011) as an indicator of power in light of evidence that powerful people are more likely to take risks across a variety of situations (Anderson & Galinsky, 2006; Galinsky, Gruenfeld, & Magee, 2003).

Finally, we collected a large sample of data in order to provide more precise estimates of the size of the effects of power poses.

Method

Design and participants

Our experimental design closely replicated and extended the design of Carney et al. (2010) by crossing the original power pose manipulation with a manipulation of participant awareness of the function of power poses. Thus, the study had a 2 (pose: high vs. low power) by 2 (awareness: aware vs. unaware) between-subjects design. Participants were 292 native English-speaking undergraduates from Michigan State University (65.4% female; 77.4% White).

Exclusion criteria

As preregistered, nine participants were excluded from the analyses for moving from their assigned pose continuously for more than 30 s over the course of the 2- or 5-min pose period, and four participants were excluded for reporting holding a pose while watching the TED talk. Therefore, the final sample size was 279. However, because not all participants completed all measures, sample sizes in each analysis vary slightly across outcome measures. We report power analyses for each analysis before interpreting the results. Although we did not attain our preregistered sample size (N = 300) due to time constraints, the achieved sample size granted us high levels of power (above 94%) to detect the original effect sizes reported by Carney et al. (2010) and Cuddy et al. (2015).

Procedure

The experimental script used in the current study is contained in the supplemental materials.

In the unaware conditions, the materials and procedure closely followed Carney et al. (2010) with two exceptions: (1) participants did not provide saliva samples for hormone analysis and (2) they completed an additional speech task. First, participants were given a cover story that the study was about how people remember and hold physical positions while doing various tasks (see Cesario & McDonald, 2013). Participants were then randomly assigned to hold two expansive or constrictive poses for 1 min each while forming impressions of faces.

After posing, participants completed a risky gambling task and a modified version of the Trier Social Stress Test (TSST) (Kirschbaum, Pirke, & Hellhammer, 1993). In this task, they prepared a speech for a mock interview while in an expansive or constrictive pose (this pose was always consistent with the first set of poses). Finally, they delivered the speech to an experimenter (Cuddy et al., 2015), completed a self-report measure of feelings of power, and reported whether they had knowledge of research on power poses prior to their laboratory session.

The aware condition was identical to the unaware condition but a cover story was not used. Participants first watched excerpts from a TED talk explaining the benefits of power poses (Cuddy, 2012).[2] The video described that holding constrictive poses makes individuals feel less powerful, whereas holding expansive poses makes individuals feel more powerful and translates into better outcomes. Participants were explicitly asked not to hold any poses shown in the video; any participants who reported doing so were excluded from the analyses. After the video, the experimenter explained that the study concerned whether holding certain poses increased power and resulted in better outcomes. Participants then held poses and completed the same outcome measures (i.e. risky gambling task, mock interview task, and feelings of power) as those in the unaware condition.

Experimenters were not blind to the function of power poses because the awareness manipulation required the experimenter to inform the participant of the function of power poses. However, experimenters were not informed of the predictions regarding the interaction between awareness and pose. Furthermore, independent coders blind to condition and hypothesis rated the mock interview, making it less likely that demand characteristics contaminated those measures.

Initial power pose manipulation

Participants were randomly assigned to hold two expansive or constrictive poses from Carney et al. (2010; see Figure 1), for 1 min each. Experimenters assisted participants to make sure that they were in the correct position. While holding the poses, participants were asked to form impressions of five male and five female faces (obtained from Dr. Dana Carney) as a filler task.

Risk-taking measure

After posing for 2 min, participants completed a modified version of the risk-taking measure used in Carney et al. (2010). Participants were given two tickets to a raffle for a $50 gift card to a local store. They were told that they could either keep their two tickets to the raffle or they could role a die for a 50/50 chance of either winning two more tickets or losing them all. Participants made their decision but did not actually roll the die until the end of the experiment, to control for any effects that winning or losing might have on subsequent behavior.

Figure 1. Top: Constrictive poses used in the initial pose manipulation (a and b), and before the interview (c). Bottom: Expansive poses used in the initial pose manipulation (d and e), and before the interview (f).

Job interview task

After the gambling task, participants gave an impromptu interview based on a modified version of the TSST (Kirschbaum et al., 1993) used by Cuddy et al. (2015). Participants were first positioned into the expansive or constrictive poses used in Cuddy et al. (2015; see Figure 1); pose type (i.e. constrictive or expansive) always matched what pose the participant was assigned to earlier in the experiment. They held this position for 1 min. Next, they were told to imagine that they were about to interview for their dream job and had 5 min to prepare a 5-min speech while maintaining the pose. Furthermore, to increase the stakes so as to better approximate an interview context, experimenters explained that the highest rated interview would win an additional $50 gift card to a local store.

After posing during the 5-min preparation period, participants were told to stand freely and give a videotaped 5-min speech to the experimenter. The experimenter wore a lab coat, displayed flat affect, and said "Please continue" if the participant stopped for more than 10 s. If the participant stopped talking for 10 s again, the experimenter asked one of three prompt questions (e.g. "please give more detail about anything you just talked about"; see supplemental materials).[3] These procedures regarding experimenter etiquette are used in the TSST (Kirschbaum et al., 1993) and make it particularly challenging because participants do not receive the real-time feedback they would in a typical interview.

To calculate measures of overall performance and hireability, 244 independent judges (a separate sample of undergraduate students; 66.8% female, 68.9% White) rated the speeches. Each judge rated four randomly selected speeches on a 3-point scale of hireability (1 = *no*, 2 = *maybe*, 3 = *yes*), a 7-point scale of overall performance ranging from 1 (*awful*) to 7 (*amazing*), and a 7-point scale of body expansiveness ranging from 1 (*very constrictive)* to 7 (*very expansive*). The latter measure was used to test for effects of posture on interviews (Cuddy et al., 2015). At least three independent judges evaluated each speech, and the ratings from the first three judges were averaged to form indexes of hireability (intra-class $r = .33$, 95% CI [.25, .41]), performance (intra-class $r = .40$, 95% CI [.32, .48]), and body expansiveness (intra-class $r = .24$, 95% CI [.16, .33]). All judges were blind to condition and hypotheses.

Feelings of power

After giving their speech, participants reported how powerful they felt using the scale items reported in Cuddy et al. (2015). Participants reported how powerful, in charge, dominant, like a leader, and in control they felt on a scale from 1 (*not at all*) to 5 (*a lot*). As pointed out by a reviewer, power poses closely resemble poses associated with the emotional experience of pride (Tracy & Robins, 2004). This raises the question of whether pride might explain the relation between poses and measures of power. To explore this possibility, participants also completed the seven-item authentic pride scale on the same 5-point scale (e.g. accomplished, successful; $\alpha = .94$, 95% CI [.93, .95]; Tracy & Robins, 2007).

Prior knowledge of power poses

Given the ubiquity of the power pose effect in public circles, it was possible that people were aware of power pose research despite being in the unaware condition. Thus, all participants answered *yes* or *no* to the following questions: "Were you aware of research on power poses before today?" "Have you seen the TED talk on power poses before?" and "Have you heard about power poses from some other source, such as a book, television, or friend before today?"

Results

Preregistered analyses

Manipulation check

As a manipulation check,[4] we first tested whether participants felt more powerful after holding expansive versus constrictive poses. A *post-hoc* power analysis assuming an effect size $d = 0.94$ (Carney et al., 2010) with an alpha level of .05 indicated that we had 99.9% power to detect an effect for the unaware participants. Feelings of power were operationalized as the average of how "powerful" and "in-charge" participants felt, $r(280) = .79$, 95% CI [.74, .83], $p < .001$.

A factorial ANOVA was conducted to test whether pose, awareness, and their interaction affected the subjective experience of power.[5] This analysis revealed that participants in the expansive conditions ($M = 2.73$, SD = 1.05) did not feel significantly more powerful than those in the constrictive conditions ($M = 2.58$, SD = 1.00), $F(1, 265) = 1.55$, $p = .214$, $d = 0.16$, 95% CI [−0.08, 0.39]. Those in the aware conditions ($M = 2.70$, SD = 1.02) did not feel significantly more powerful than those in the unaware conditions ($M = 2.62$, SD = 1.04), $F(1, 265) = 0.43$, $p = .511$, $d = 0.08$, 95% CI [−0.16, 0.32]. Finally, there was no interaction between awareness and pose, $F(1, 265) = 1.14$, $p = .287$, $\eta_p^2 < .01$, 90% CI [.00, .03].[6] Descriptive statistics for all four conditions in this analysis and subsequent preregistered analyses can be found in Tables 1 and 2. While these results suggest that power poses were not an effective way of manipulating power, one possibility is that performing a difficult speech may have reduced any feelings of power that were elicited by the poses. We investigate this further in the exploratory analyses.

Main tests

Risk taking

We first tested whether pose influenced willingness to take a risky (double or nothing) gamble. To test this, a factorial logistic regression was performed on the gambling decision (0 = no gamble; 1 = gamble). A *post-hoc* power analysis assuming an effect size $\phi = .30$ and an alpha level of .05 with two-sided significance tests (Carney et al., 2010) indicated that we had 94.4% power to detect the original effect (i.e. the effect of pose for participants in the unaware condition). The analysis suggested no differences between those who held expansive (68.8% chose to gamble) and constrictive poses (61.6% chose to gamble), $b = 0.16$, $z = 1.26$, $p = .210$, OR = 1.17, 95% CI [0.91, 1.51], no differences between those who were aware (62.5%) and unaware (67.9%), $b = -0.12$, $z = -0.93$, $p = .354$, OR = 0.88,

Table 1. Means, standard deviations (or percentages), and number of participants per condition for the preregistered analyses with the full sample.

	Aware						Unaware					
	Expansive			Constrictive			Expansive			Constrictive		
	M	SD	*N*	*M*	SD	*N*	*M*	SD	*N*	*M*	SD	*N*
Feelings of power	2.85	1.06	65	2.55	0.96	66	2.63	1.04	70	2.60	1.04	68
Gambling behavior	65.67%[a]		67	59.42%[a]		69	71.83%[a]		71	63.77%[a]		69
Performance	3.74	1.35	59	4.01	1.11	59	3.81	1.14	64	3.76	1.33	64
Hireability	2.07	0.61	59	2.18	0.55	59	2.14	0.51	64	2.12	0.58	64
Body expansiveness	3.77	0.98	59	3.63	1.20	59	3.68	1.12	64	3.52	1.11	64

[a]Percentage of participants who chose to gamble.

Table 2. Means, standard deviations (or percentages), and number of participants per condition for the preregistered analyses with participants in the unaware conditions.

	Had prior knowledge						No prior knowledge					
	Expansive			Constrictive			Expansive			Constrictive		
	M	SD	*N*	*M*	SD	*N*	*M*	SD	*N*	*M*	SD	*N*
Feelings of power	2.59	1.08	32	2.54	1.16	23	2.66	1.02	38	2.63	0.99	45
Gambling behavior	59.38%[a]		32	69.57%[a]		23	81.58%[a]		38	62.22%[a]		45
Performance	4.01	0.95	31	3.84	1.33	23	3.64	1.30	32	3.71	1.35	41
Hireability	2.19	0.51	31	2.17	0.56	23	2.09	0.52	32	2.09	0.59	41
Body expansiveness	3.67	0.98	31	3.71	1.13	23	3.67	1.27	32	3.41	1.10	41

[a]Percentage of participants who chose to gamble.

95% CI [0.69, 1.14], or an interaction between awareness and pose, $b = -0.03$, $z = -0.20$, $p = .839$, OR = 0.97, 95% CI [0.76, 1.25], on risk-taking behavior. These results suggest that power poses did not influence risk taking, a correlate of powerful decision-making.

Hireability and performance

To test whether pose, awareness, and their interaction affected hireability and job interview performance, we performed separate factorial ANOVAs with Bonferroni corrected alpha levels (i.e. $\alpha = .025$). Hireability and performance scores were computed as the average of ratings from the first three independent judges. *Post-hoc* power analysis indicated that we had 98.4% power to detect the original effect on performance ($d = 0.73$) and 96.8% power to detect the original effect on hireability ($d = 0.68$; Cuddy et al., 2015).

The analysis of job interview performance showed no evidence of differences between those who held expansive ($M = 3.78$, SD = 1.24) and constrictive poses ($M = 3.88$, SD = 1.23), $F(1, 242) = 0.38$, $p = .537$, $d = -0.08$, 95% CI [−0.33, 0.17], no differences between those who were aware ($M = 3.87$, SD = 1.24) and unaware ($M = 3.78$, SD = 1.24), $F(1, 242) = 0.32$, $p = .573$, $d = 0.07$, 95% CI [−0.18, 0.32], or an interaction between awareness and pose, $F(1, 242) = 1.05$, $p = .308$, $\eta_p^2 < .01$, 90% CI [.00, .03]. Similarly, hireability judgments did not differ between those in the expansive ($M = 2.10$, SD = 0.56) and constrictive conditions ($M = 2.15$, SD = 0.56), $F(1, 242) = 0.41$, $p = .521$, $d = -0.09$, 95% CI [−0.34, 0.16], or between those in the aware ($M = 2.12$, SD = 0.58) and unaware conditions ($M = 2.13$, SD = 0.54), $F(1, 242) < 0.01$,

p = .963, d = −0.01, 95% CI [−0.26, 0.24], and there was no interaction effect, $F(1, 242) = 0.80$, p = .371, $\eta_p^2 < .01$, 90% CI [.00, .03]. In sum, there was no evidence that power poses improved performance in a realistic situation in which they would likely be used outside the laboratory.

Secondary tests

Body expansiveness

Following the preregistered analysis plan, we also tested whether power poses affected body expansiveness during the interview, as rated by independent judges. This secondary test addresses whether any improvements in performance might be due to changes in posture that carry over from the pose held before the interview. To test this, we performed a factorial ANOVA with pose, awareness, and their interaction as predictors. Body expansiveness did not differ between those in the expansive (M = 3.72, SD = 1.05) and constrictive conditions (M = 3.57, SD = 1.15), $F(1, 242) = 1.12$, p = .292, d = 0.13, 95% CI [−0.12, 0.38], did not differ between those who were aware (M = 3.70, SD = 1.09) and unaware (M = 3.60, SD = 1.11), $F(1, 242) = 0.55$, p = .460, d = 0.09, 95% CI [−0.16, 0.34], and there was no interaction between pose and awareness, $F(1, 242) < 0.01$, p = .958, $\eta_p^2 < .01$, 90% CI [.00, <.01]. Thus, there was no indication that poses changed participants' posture during their interview.

Prior knowledge of power poses

In recent years, research on power poses has received widespread media attention (Cuddy, 2012, 2015). The widespread attention raises an important concern that participants randomly assigned to the unaware condition might nevertheless have heard of the effects of power poses before coming into the laboratory. To address this issue, we repeated the previous analyses among only participants in the unaware conditions using their reports of prior knowledge as the index of awareness. Following our preregistered protocol, we considered participants as having no prior knowledge if they answered no to all three questions about their prior knowledge of power poses. Participants with no prior knowledge were compared to those who answered yes to at least one of the questions. Out of the 142 participants in the unaware condition, 87 (61.3%) reported having no knowledge of power poses. These analyses did not reveal significant effects of pose, prior knowledge, or their interaction on any of the dependent variables (see test statistics in Table 3 and effect sizes in Table 4).[7]

Relationship between power and pride

The final preregistered analysis concerns whether the effects of power poses might be explained by enhanced feelings of pride. Although there was no effect of power poses on any of the outcomes measured, feelings of power and pride were strongly correlated in the current study, $r(267) = .73$, 95% CI [.67, .78], $p < .001$. Thus, it is possible that power poses might exert their influence through feelings of pride or that these measures are tapping into the same, single underlying construct.

In sum, the effect of power poses on feelings of power, risk taking, and performance is likely much smaller than previous studies have estimated. Furthermore, the effects of power poses do not seem to be sensitive to whether participants are aware of their

Table 3. Test statistics for the preregistered analyses with participants in the unaware conditions.

	Pose	Prior knowledge	Prior knowledge × pose
Feelings of power	$F(1, 134) = 0.04$, $p = .847$	$F(1, 134) = 0.16$, $p = .694$	$F(1, 134) = 0.01$, $p = .944$
Gambling behavior	$b = 0.14$, $z = 0.70$, $p = .486$	$b = -0.20$, $z = -1.00$, $p = .315$	$b = -0.36$, $z = -1.85$, $p = .065$
Performance	$F(1, 123) = 0.02$, $p = .891$	$F(1, 123) = 1.38$, $p = .243$	$F(1, 123) = 0.29$, $p = .593$
Hireability	$F(1, 123) = 0.01$, $p = .912$	$F(1, 123) = 0.91$, $p = .341$	$F(1, 123) = 0.01$, $p = .939$
Body expansiveness	$F(1, 123) = 0.40$, $p = .530$	$F(1, 123) = 0.63$, $p = .429$	$F(1, 123) = 0.53$, $p = .470$

Effects for performance and hireability were considered significant at α = .025.

Table 4. Effect sizes for the preregistered analyses (Table 3) with participants in the unaware conditions.

	Pose		Prior knowledge		Prior knowledge × pose	
	ES	95% CI	ES	95% CI	ES	90% CI
Feelings of power	$d = 0.03$	[−0.30, 0.37]	$d = -0.07$	[−0.41, 0.27]	$\eta_p^2 < .01$	[.00, <.01]
Gambling behavior	OR = 1.15	[0.78, 1.68]	OR = 0.82	[0.56, 1.21]	OR = 0.70	[0.47, 1.02][a]
Performance	$d = 0.04$	[−0.31, 0.39]	$d = 0.20$	[−0.15, 0.55]	$\eta_p^2 < .01$	[.00, .04]
Hireability	$d = 0.02$	[−0.33, 0.37]	$d = 0.17$	[−0.19, 0.52]	$\eta_p^2 < .01$	[.00, <.01]
Body expansiveness	$d = 0.09$	[−0.26, 0.44]	$d = 0.13$	[−0.22, 0.48]	$\eta_p^2 < .01$	[.00, .04]

ES: effect size; CI: confidence interval. [a]Confidence intervals for odds ratios are all 95%.

function. However, the validity of this argument for finding a moderating effect of awareness may ultimately depend on finding a significant effect of power poses, a point we return to in the Discussion.

Exploratory analyses

Given the pattern of null results we found using our experimental method, there are several concerns readers might have about the internal validity of our results. For instance, the order in which participants completed the measures may have weakened some of the effects of power poses. Also, the judges' ratings may not have been sufficiently reliable or valid to detect power pose effects. To address these questions and further understand the nature of our findings, we conducted a number of exploratory analyses.

In the current study, the power pose manipulation had no significant effect on feelings of power, which has been used as a manipulation check (Cuddy et al., 2015). However, we note that the only other study in which participants completed an interview paradigm before answering the manipulation check likewise did not find a significant effect of power posing on feelings of power at conventional alpha levels ($p = .076$; Cuddy et al., 2015). There are likely many reasons why the effects of power poses on feelings of power were not significant. One potential reason may be that feelings of power were collected after a challenging and stressful task (i.e. TSST). Thus, feelings of power may have been affected by merely engaging in such a task. Indeed, during and after the interview, many participants reported feeling anxious, awkward, and uncomfortable. These thoughts may have influenced their feelings of power.

To test this question, we examined measures of central tendency as well as the distribution for the aggregated feelings of power measure. We found that the mean

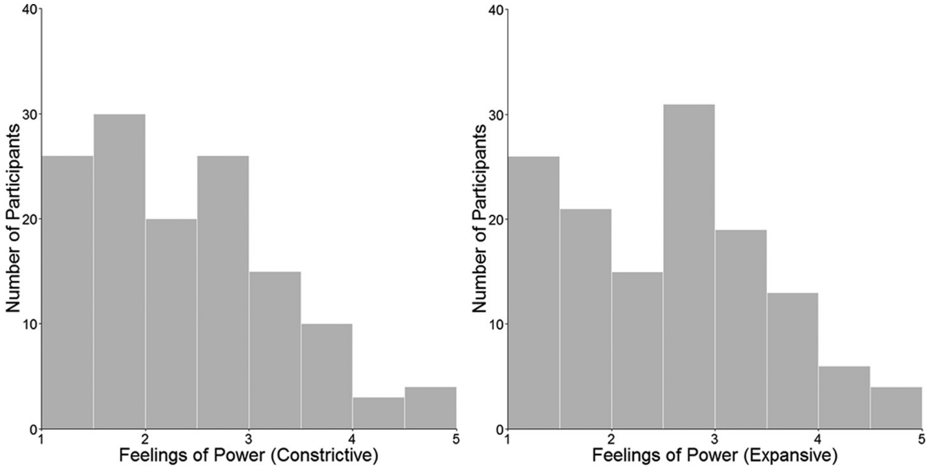

Figure 2. Histogram for the two-item measure of feelings of power for participants in the constrictive (left) and expansive (right) conditions.

feeling of power was significantly below the midpoint of the scale (3.00) for both expansive (M = 2.73, SD = 1.05, $t(134) = -2.94$, p = .004, $d = -0.26$) and constrictive conditions (M = 2.58, SD = 1.00, $t(133) = -4.89$, p < .001, $d = -0.42$), indicating that participants felt generally less powerful in both conditions (see Figure 2 for distributions in each condition). Although the power pose conditions did not differ in their feelings of power, it is possible that giving a difficult speech overwhelmed any changes in feelings of power that resulted from posing.

Another concern is that the measures used in the current study did not reliably assess the constructs of interest. For instance, only two items comprised the feelings of power measure used in Carney et al. (2010) and the present study. Despite significant findings in previous studies, it is possible that our measure was an insufficiently reliable indicator of feelings of power and thus contributed to our null results. We did, however, collect data on the feelings of power measure used by Cuddy et al. (2015) which includes three additional indicators of power, therefore providing a more reliable assessment of the construct (α = .93, 95% CI [.92, .95]). Even with this more reliable measure, feelings of power did not significantly differ between those in the expansive (M = 2.80, SD = 1.06) and constrictive conditions (M = 2.65, SD = 0.94), $F(1, 265) = 1.41$, p = .236, d = 0.15, 95% CI [−0.09, 0.39], nor between those in the aware (M = 2.76, SD = 1.01) and unaware conditions (M = 2.69, SD = 1.00), $F(1, 265) = 0.26$, p = .608, d = 0.06, 95% CI [−0.18, 0.30], and there was no interaction between awareness and pose predicting feelings of power, $F(1, 265) = 1.20$, p = .274, $\eta_p^2 < .01$, 90% CI [.00, .03]. Additional descriptive statistics for these and subsequent exploratory analyses can be found in Tables S4 and S5.

Similarly, another possible concern is that the ratings made by untrained judges did not reliably assess participants' performance and hireability. To examine this, we conducted intra-class correlations of the three ratings used. These revealed a substantial agreement between judges for performance ratings (intra-class r = .40, 95% CI [.32, .48]), hireability ratings (intra-class r = .33, 95% CI [.25, .41]), and somewhat less for body expansiveness ratings (intra-class r = .24, 95% CI [.16, .33]). In addition, for 77% of the interviews, we also had a fourth rater. Ratings by the four judges displayed slightly larger reliability for performance (intra-class r = .41, 95% CI [.34, .48]), hireability (intra-class r = .33, 95% CI [.26, .40]), and body expansiveness (intra-class r = .26, 95% CI [.19, .33]). Analyzing the data with these additional ratings did not support the effectiveness of power poses (see Table S6 for test statistics and Tables S7 and S8 for effect sizes and confidence intervals).

Finally, although the judge's ratings may have been sufficiently reliable, it is possible that they failed to accurately assess the relevant constructs that affected by manipulations of power. In other words, although there may have been substantial agreement among raters, their judgments may not have actually assessed performance and hireability. Instead, they may have tracked less relevant factors (e.g. attractiveness, likeability). One way to examine the validity of the ratings is to gauge their association with participants' self-reports. If both the participants and the judges had insight into the participants' performances, then the participants' feelings of pride (e.g. accomplishment, success) after their speech should be related to the judges' assessments of performance. As predicted, the judges' ratings of performance ($r(242)$ = .39, 95% CI [.28, .49], $p < .001$) and hireability ($r(242)$ = .31, 95% CI [.19, .42], $p < .001$) were correlated with the authentic pride subscale. In addition, the judges' performance ($r(242)$ = .40, 95% CI [.29, .50], $p < .001$) and hireability ratings ($r(242)$ = .35, 95% CI [.23, .45], $p < .001$) were also associated with the five-item power index, suggesting that their ratings assessed the intended constructs that would have been affected by power-related manipulations if they were effective in doing so.

Based on these exploratory analyses, we see little reason to suspect that the null findings we observed for the effectiveness of power posing are due to methodological or measurement problems in our study.

Discussion

Previous research indicates that holding expansive postures in the laboratory increases feelings of power, risk tolerance, and performance in mock job interviews. Given these results, researchers proposed that power posing might benefit individuals outside the laboratory (Carney et al., 2010; Cuddy et al., 2015). Using a high-powered sample, we tested whether individuals could benefit from power posing in more realistic circumstances in which they were aware of its purported effects. We did not find evidence that they made individuals feel more powerful, engage in more risks, or improve their performance in a mock job interview. Despite three different theoretical accounts of how awareness might influence the effect of power poses, we did not find evidence that power poses were effective under any circumstances. While we stress that no one single study can ever conclusively bolster or refute an effect, our work does not support the idea that power poses are an effective way to help people act more powerfully.

In addition to testing the moderating role of awareness, a major strength of our study was the inclusion of a mock interview. This task is crucial because it allowed us to test whether power poses have statistically detectable effects in the kinds of realistic situations that researchers have touted their benefits for (e.g. Carney et al., 2010). Although some work has found initial evidence that expansive poses might improve interview performance (Cuddy et al., 2015), this evidence was based on a small sample, and performance was scored by a single rater. Our study improved upon this design by using a larger sample with four times as many participants (N = 246 vs. 61) and included multiple raters with high degrees of inter-rater agreement. Our results showed no support for the effectiveness of power poses on interview performance.

Internal validity concerns

A common justification for discrepancies among replications and original studies, especially when those replications produce null results, is that the replication did not meet the necessary conditions to find the effect. Sometimes, these "hidden" moderators are unknown or otherwise unspecified in the original studies. Therefore, below, we discuss some differences between our study and the work it is based on (Carney et al., 2010) in order to comment on the internal validity of our study and discuss possible moderators of the power pose effect.

The main difference between our procedure and Carney et al. (2010) is that participants did not complete a self-report measure of power directly after they completed the risk-taking measure. The reason for this change was that we did not wish to alert participants in the unaware condition to the relationship between the poses and power. Instead, participants completed the self-report measure of power after they gave their mock interview. As discussed in the exploratory analyses, this may have attenuated the effects of power poses on feelings of power. However, this change was consistent with the procedures employed in a later study by Cuddy et al. (2015). Similarly, Cuddy et al. (2015) did not find effects of power poses on feelings of power at conventional alpha levels after completing the mock interview, and the point estimate of effect size for power poses was half as large as when no speech was given; d = 0.46 versus d = 0.94 (Carney et al., 2010). These data suggest that the stressful interview itself may influence feelings of power, overriding the effect of power poses.

Another important difference is that we used three untrained judges per video to evaluate performance on the mock interviews, as opposed to a single trained judge (Cuddy et al., 2015). Using one trained judge in the study was not feasible given the large number of videos we collected from participants. We believe that our approach was not problematic because judges made global evaluations (i.e. hireability and performance), rather than specific judgments that would require training. Given that interview performance is somewhat subjective, ratings from more judges give more stable estimates of performance. Furthermore, intra-class correlations between judges' ratings indicated high reliability – the judges agreed substantially on their ratings of the interviewees. Those ratings were also correlated with participants' own reports of both pride and power, suggesting that they were accurately assessing the constructs of interest. Thus, we think it is unlikely that our different rating system might have somehow been an unreliable or invalid measure of interview performance.

There will always be subtle differences between replications and original studies that can account for discrepancies in results. For instance, participants posed twice during our study, and it may be that the effect is substantially weakened after posing the first time. Also, power poses may only be effective in interviews with trained judges. While these and other differences could moderate the effects in the current study, one must consider what they mean for the practical use of power poses. If these variables or others were shown to moderate the effects of power poses, it would greatly limit the situations they would be useful in, and thus their practical application.

Limitations and future directions

Once criticism of the current set of studies is that the mock interview design lacks externally validity because no job was actually on the line. Despite this difference, the task was psychologically engaging, as judged by participants' reports of its difficulty as well as an exploratory analysis of their feelings of pride. It is perhaps unsurprising, then, that the best predictor of participants' feelings of power after the interview was not how they posed, but their actual performance as rated by outside observers. Because the vast majority of expansive and constrictive participants felt relatively powerless after the interview, this suggests that any effects of power poses were wiped out by the stress of the interview.

This raises the possibility that power poses might have an immediate effect on the psychological experience of power that lasts until a challenging situation arises. Future research could test this by having participants hold power poses and then assessing feelings of power before and after a mock interview. We suspect that – consistent with past research – expansive posers would feel more powerful before the interview than constrictive posers. After the interview, both groups would feel equally less powerful, as was found in the current study. This finding would be especially important because if the effects of pose are eliminated in a low stakes situation like a mock interview, it would be highly unlikely that they would have a positive benefit in an actual interview, where arousal and anxiety are much higher.

Our study was also designed to test whether power poses would be effective if an individual was aware of their effects. However, we did not find that power poses were effective under any circumstances. While it is possible to conclude that our test of awareness was not informative because we did not find a significant effect of power pose, we believe that this would be short sighted. Our study adds to the list of power pose replications that have not found the predicted effects of pose (e.g. Ranehill et al., 2015). In fact, since this article was preregistered, another highly powered (N = 305) direct replication of power pose research failed to find an effect of pose on risky gambling behavior or behavior in an economic game (Garrison, Tang, & Schmeichel, 2016). Additionally, and contrary to predictions, in this study, posing expansively actually made participants feel *less* powerful than when they posed constrictively. Given this evidence, it may be that the current theory of power poses does not specify all necessary conditions for finding the effect. Future research should test revised theories that can explain and predict how subtle aspects of the situation may reduce the effect of power poses. These revised theories might find that – in some circumstances – there might be psychological and behavioral benefits of power poses. However, if the benefits of power poses are restricted by situational features, such as how stressful a situation is, their applications may not be as widespread as has been suggested.

Conclusion

We are encouraged by the growing popularity of replication research and use of preregistration. This special issue is unique because experts in the power pose literature reviewed all studies before they started. After taking into account the suggestions of experts, the studies were preregistered, "locking in" the revised protocols for data collection, analysis, and reporting. Because publication was dependent on the strength of the experimental method rather than the study results, there was no incentive to find a particular result. We believe that this is a positive direction for research on power poses and social psychology more generally. We hope that researchers investigating power poses will continue to seek expert advice and conduct highly powered preregistered studies while replicating methods across multiple laboratories.

Practically speaking, low power individuals have limited access to valued resources, which makes it difficult for them to obtain desired goals. Developing a simple intervention like power posing that could help them obtain these goals has a strong egalitarian appeal. However, for an intervention to be widely disseminated, its effectiveness must first be demonstrated and the conditions under which it works (and does not work) must be identified. Given the results of our study and other replications (Garrison et al., 2016; Ranehill et al., 2015), it seems likely that the effect of power poses is much smaller than previously assumed and may be sensitive to unknown contextual variables. We recommend researchers stop unilaterally suggesting that power poses will allow people to obtain desired outcomes until definitive and supportive research is established.

Notes

1. A recent review also suggested that awareness might reduce the effectiveness of power poses (Carney, Cuddy, & Yap, 2015).
2. To reduce demand characteristics (Weber & Cook, 1972), parts of the TED talk were removed that reference findings relevant to the current experiment (e.g. behavior in the gambling task).
3. These standardized questions were not in the preregistered protocol; however, it was apparent very early in data collection that participants would otherwise not speak for the full 5 min if they were not prompted to continue when they stopped talking.
4. Past work has used changes in the subjective experience of power as both an outcome and as a manipulation check. We are agnostic to this issue but defer to more recent studies that characterize it as a manipulation check (e.g. Cuddy et al., 2015).
5. There was no evidence for heterogeneity of variance in any of the analyses prior to conducting the factorial ANOVAs.
6. We report 90% confidence intervals for partial eta-squared because, contrary to 95% CIs, they exclude zero when one-sided tests, such as the *F* test, are significant (Smithson, 2001).
7. We also compared participants in the aware and unaware conditions who reported having no prior knowledge. Still, there were no significant effects (Tables S1–S3).

Disclosure statement

No potential conflict of interest was reported by the authors.

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ARTICLE

A Bayesian model-averaged meta-analysis of the power pose effect with informed and default priors: the case of felt power

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ABSTRACT

Earlier work found that – compared to participants who adopted constrictive body postures – participants who adopted expansive body postures reported feeling more powerful, showed an increase in testosterone and a decrease in cortisol, and displayed an increased tolerance for risk. However, these power pose effects have recently come under considerable scrutiny. Here, we present a Bayesian meta-analysis of six preregistered studies from this special issue, focusing on the effect of power posing on felt power. Our analysis improves on standard classical meta-analyses in several ways. First and foremost, we considered only preregistered studies, eliminating concerns about publication bias. Second, the Bayesian approach enables us to quantify evidence for both the alternative and the null hypothesis. Third, we use Bayesian model-averaging to account for the uncertainty with respect to the choice for a fixed-effect model or a random-effect model. Fourth, based on a literature review, we obtained an empirically informed prior distribution for the between-study heterogeneity of effect sizes. This empirically informed prior can serve as a default choice not only for the investigation of the power pose effect but for effects in the field of psychology more generally. For effect size, we considered a default and an informed prior. Our meta-analysis yields very strong evidence for an effect of power posing on felt power. However, when the analysis is restricted to participants unfamiliar with the effect, the meta-analysis yields evidence that is only moderate.

Introduction

Could adopting a powerful body posture make us more powerful? Carney, Cuddy, and Yap (2010) found that participants who adopted expansive, high-power body postures (Figure 1, top row) as opposed to constrictive, low-power body postures (Figure 1, bottom row) reported feeling more powerful and in charge, showed an increase in testosterone and a decrease in cortisol, and displayed an increased tolerance for risk. The power pose

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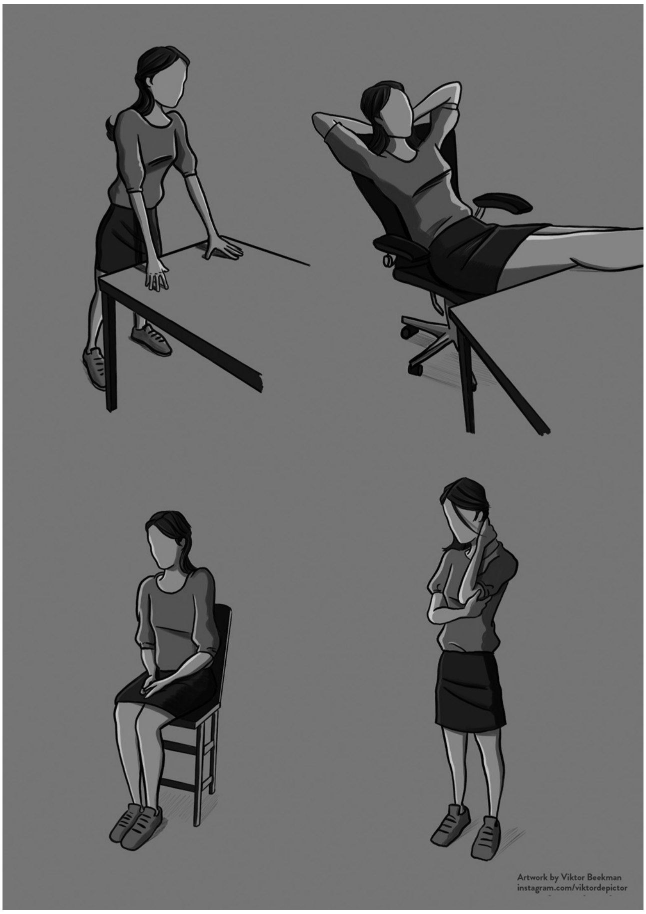

Figure 1. High-power poses (top row) and low-power poses (bottom row). CC-BY. Artwork by Viktor Beekman, commissioned by Eric-Jan Wagenmakers.

effect has attracted a lot of attention, partly due to the anticipated consequences for day-to-day life suggesting that it might be possible to "fake it 'til you make it.'"

However, this power pose effect has recently come under scrutiny. When Ranehill et al. (2015) attempted to replicate the effect, they found – similar to the original study – that adopting high-power poses increased participants' self-reported feelings of power; nevertheless, they did not find an effect on testosterone or cortisol nor on behavioral measures such as risk taking. Carney, Cuddy, and Yap (2015) pointed out a number of methodological differences that they believe might have been the cause for the diverging results. Recently, Garrison, Tang, and Schmeichel (2016) conducted a preregistered replication and extension of the power pose study, and they failed to identify an effect of power posing on risk taking behavior. Furthermore, in contrast to Ranehill et al. (2015), these authors did not find evidence for a power pose effect on subjective feelings of power.

In the present special issue, seven preregistered studies investigated the effect of power posing under various circumstances (i.e. Bailey, LaFrance, & Dovidio, this issue; Bombari, Schmid Mast, & Pulfrey, this issue; Jackson, Nault, Smart Richman, LaBelle, &

Rohleder, this issue; Keller, Johnson, & Harder, this issue; Klaschinski, Schröder-Abé, & Schnabel, this issue; Latu, Duffy, Pardal, & Alger, this issue; Ronay, Tybur, Van Huijstee, & Morssinkhof, this issue). Here, we present a meta-analysis of the effect of power posing on self-reported felt power, which was included as a dependent variable in six of the seven studies in this special issue.

Our analysis improves upon classical analyses in several ways. First, we only consider a set of preregistered studies which comes with the advantage that publication bias can be ruled out a priori (cf. the concept of a *prospective meta-analysis* in medicine). Second, the Bayesian approach enables us to quantify evidence for both the alternative hypothesis *and* for the null hypothesis; note that this evidence can be seamlessly updated as future studies on the effect become available. Third, Bayesian model-averaging enables us to fully acknowledge uncertainty with respect to the choice of a fixed-effect or random-effect model; in the fixed-effect model, the effect is assumed to be identical across studies; in the random-effect model, the effect is assumed to vary across studies. Instead of adopting one model for inference and ignoring the other model entirely, we can weight the results of both models according to their posterior plausibilities. This yields a model-averaged measure of evidence and a model-averaged estimate for the meta-analytic effect size. Fourth, the Bayesian approach enables us to incorporate existing knowledge into our analysis (e.g. Rhodes, Turner, & Higgins, 2015). Based on an extensive literature review of meta-analyses in the field of psychology, we obtained an informed prior distribution for the between-study heterogeneity. This informed prior distribution can serve as an informed default not only for the investigation of the power pose effect in the present meta-analysis, but for the field of psychology more generally. For effect size, we also consider an informed prior distribution based on knowledge about effect sizes in the field of psychology. As a robustness check with respect to the prior choice, we show that qualitatively similar results are obtained when we instead use a default prior for the effect size parameter.

The outline of this article is as follows: first, we explain the details of our analysis. Second, we present the results of an extensive literature review that allowed us to specify an informed prior distribution for the between-study heterogeneity. Third, we present the results of the model-averaged Bayesian meta-analysis for two different prior choices for effect size. Finally, we investigate whether the results change when only participants unaware of the power pose effect are included in the analysis.

Method

In our meta-analysis, we focused on the dependent variable *felt power* which was measured in all replication studies in the present issue except for the study by Jackson et al., which was therefore not considered in the analysis. We investigated the question whether felt power was higher in the high-power condition than in the low power condition.

Analysis of individual studies

When considering a single study, the power pose effect can be tested using a standard one-sided, independent-samples *t*-test. Hence, the first step in our analysis was to compute one-sided Bayesian *t*-tests (Gronau, Ly, & Wagenmakers, 2017; Ly, Verhagen, & Wagenmakers, 2016; Rouder, Speckman, Sun, Morey, & Iverson, 2009). This allowed us (1) to estimate for each study the posterior distribution of the standardized effect size that represents our beliefs about the effect size after having observed the data of that study and (2) to quantify the evidence that each study provides in favor of the hypothesis that the power pose effect is positive (H_+) versus the null hypothesis that the effect is zero (H_0).

To quantify the evidence that the data provide for or against H_+, we computed the Bayes factor (Jeffreys, 1961; Kass & Raftery, 1995) which is the predictive updating factor that quantifies how much the data have changed the relative plausibility of the competing models. The Bayes factor has an intuitive interpretation: when $BF_{+0} = 10$, this indicates that the data are 10 times more likely under H_+ than under H_0; when $BF_{+0} = 1/5$, this indicates that the data are 5 times more likely under H_0 than under H_+.

Meta-analysis

The next step in our analysis was to combine the studies with the help of a Bayesian meta-analysis (e.g. Marsman et al., 2017) to obtain an estimate of the overall effect size and to quantify the evidence for an effect that takes into account all studies simultaneously. In a classical meta-analysis, the analyst has to make a choice between a fixed-effect and a random-effect model. A fixed-effect model makes the assumption that there is one underlying effect size so that the true effect in each study is identical; differences in the observed effect sizes are solely due to normally distributed sampling error. This can be formalized as follows: we assume that $y_i \sim N(\delta_{\text{fixed}}, \text{SE}_i^2)$, where y_i, $i = 1,2,\ldots,n$ denotes the observed effect size in the ith of n studies, SE_i denotes the corresponding standard error which is commonly assumed to be known, and δ_{fixed} corresponds to the common true effect size.

In contrast, a random-effect model allows for idiosyncratic study effects, that is, we no longer impose the constraint that there exists one common true effect size for all studies. The random study effects are usually assumed to follow a normal distribution with a mean equal to the overall effect size that we are interested in and a standard deviation that corresponds to the between-study heterogeneity. Note that analogously to the fixed-effect model, the model still incorporates random sampling error so that the observed effect size for a given study is not necessarily identical to the true effect size for that study. These assumptions yield a model with a hierarchical structure, which can be formalized as follows: let δ_{random} denote the mean of the normal distribution of the study effects (i.e. the quantity that we are interested in), τ denote the standard deviation of that normal distribution (i.e. between-study heterogeneity), and θ_i denote the true study effect for the ith study. Then, $\theta_i \sim N(\delta_{\text{random}}, \tau^2)$ and $y_i \mid \theta_i \sim N(\theta_i, SE_i^2)$. The structure of the model allows one to analytically integrate out the random study effects so that the model can equivalently be written as $y_i \sim N(\delta_{\text{random}}, \tau^2 + SE_i^2)$ which can be more convenient from a computational perspective.

Bayesian model-averaging

The choice of a fixed-effect or random-effect model commonly relies on a test for heterogeneity or on a priori considerations. Final inference is then based on either the

Table 1. The four meta-analysis models included in the Bayesian model-averaging for hypothesis testing.

Hypotheses	Fixed-effect meta-analysis	Random-effect meta-analysis
H_0: No effect	Fixed overall effect size $\delta_{\text{fixed}} = 0$	Mean overall effect size $\delta_{\text{random}} = 0$ Study heterogeneity τ Study effect size θ_i ($i = 1,2,\ldots,n$)
H_+: Positive effect	Fixed overall effect size δ_{fixed}	Mean overall effect size δ_{random} Study heterogeneity τ Study effect size θ_i ($i = 1,2,\ldots,n$)

fixed-effect or random-effect model. When the number of studies is small, this choice may be difficult; and in certain cases, the choice may be consequential. The Bayesian approach, however, allows a compromise solution: instead of selecting either a fixed-effect or random-effect model, we can use Bayesian model-averaging (e.g. Haldane, 1932; Hoeting, Madigan, Raftery, & Volinsky, 1999) and retain all models for final inference. Conclusions are then based on a combination of all models where the results of each model are taken into account according to the model's plausibility in light of the observed data. Concretely, Bayesian model-averaging allows us to obtain a model-averaged estimate for the meta-analytic effect size (Sutton & Abrams, 2001) and to quantify the overall evidence for an effect that considers both the fixed-effect and random-effect model (Scheibehenne, Gronau, Jamil, & Wagenmakers, 2017).

With respect to hypothesis testing, for the current analysis, we entertained four models of interest, shown in Table 1: (1) the fixed-effect model H_+, (2) the fixed-effect model H_0 (i.e. $\delta_{\text{fixed}} = 0$), (3) the random-effect model H_+, and (4) the random-effect model H_0 (i.e. $\delta_{\text{random}} = 0$). The fixed-effect meta-analytic Bayes factor was obtained by comparing case (1) to case (2); the random-effect meta-analytic Bayes factor pitched case (3) against case (4). To compute the model-averaged Bayes factor, we contrasted the summed posterior model probabilities (i.e. the probability of a model given the data) for cases (1) and (3) against the summed posterior model probabilities for cases (2) and (4). This assumes that all four models are equally likely a priori, a common assumption in model-averaging scenarios. In case the prior model probabilities were not identical, the ratio of the summed posterior model probabilities for cases (1) and (3) over (2) and (4) would need to be divided by a ratio obtained in a similar fashion but this time based on the prior model probabilities.

With respect to parameter estimation, we computed a model-averaged effect size estimate based on the four model versions described above, except that we no longer imposed the constraint that the effect size has to be positive. In other words, consistent with standard practice, we imposed a directional constraint for testing but not for estimation (cf. Jeffreys, 1961, who also used different priors for estimation and testing). This reflects the fact that the estimation framework is generally more exploratory in nature, and this mindset is inconsistent with the use of hard boundaries. The combined estimate was obtained by combining the estimates of models (1) and (3) – but without the order-constraints – according to their posterior model probabilities. To conduct the model-averaged Bayesian meta-analysis, we used the R package *metaBMA* (Heck & Gronau, 2017) available from https://github.com/danheck/metaBMA.

Prior distributions

In the Bayesian approach, model parameters are assigned prior distributions that reflect the knowledge, uncertainty, or beliefs for the parameters before seeing the data. Using Bayes' theorem, these prior distributions are then updated by the data to yield posterior distributions, which reflect the uncertainty for the parameters after the data have been observed. Consequently, in order to conduct our Bayesian analyses, prior distributions were required for all model parameters.

For the standardized effect size, we considered two different prior choices. First, we used what has now become the default choice in the field of psychology, that is, a zero-centered Cauchy distribution with scale parameter equal to $1/\sqrt{2}$ (Morey & Rouder, 2015). Second, we considered the informed prior distribution reported in Gronau et al. (2017): a *t* distribution with location 0.350, scale 0.102, and three degrees of freedom, which is displayed in Figure 2. This prior distribution was elicited from Dr. Oosterwijk, a social psychologist at the University of Amsterdam, for a reanalysis of the Registered Replication Report on the facial feedback hypothesis (Wagenmakers et al., 2016). We believe this prior distribution is generally plausible for a wide range of small-to-medium effects in social psychology (i.e. for effects whose presence needs to be ascertained by statistical analysis). One could elicit a "power pose prior," but we believe the resulting distribution would be highly similar to the Oosterwijk prior and therefore yield highly similar inferences. Researchers interested in using a specific "power pose prior" are invited to explore this option using the R code provided online (https://osf.io/r2cds/).

For the one-sided hypothesis tests, the priors were truncated at zero, that is, the model encoded the a priori assumption that negative effect sizes are impossible. For estimating the effect size, however, we removed this truncation. The informed and default priors are depicted in Figure 2. The informed prior expresses the belief that the effect size is positive but most likely small to medium in size. The default prior on the other hand is more spread out (i.e. less informative) and it is centered on zero. Figure 2 also illustrates how the priors were truncated at zero for testing whereas for estimation, this truncation was removed.

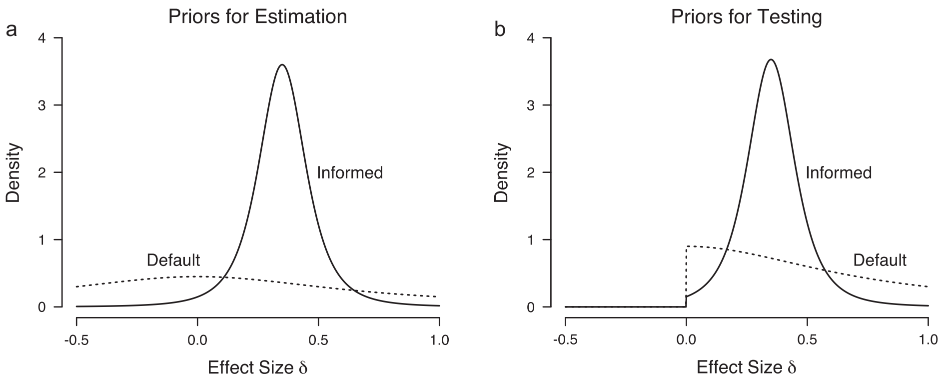

Figure 2. The left-panel (a) depicts the priors used for estimation, the right-panel (b) the truncated versions used for testing. Depiction of the default and informed prior distribution for the standardized effect size. The default prior is a Cauchy distribution with scale $1/\sqrt{2}$, the informed prior is a *t* distribution with location 0.350, scale 0.102, and three degrees of freedom. Figure available at http://tinyurl.com/j9dthma under CC license https://creativecommons.org/licenses/by/2.0/.

In addition to the prior distribution for the effect size, the Bayesian meta-analysis required a prior distribution for the between-study heterogeneity. Here, we chose an informed prior distribution for the between-study standard deviation τ. This informed prior was based on all available between-study heterogeneity estimates for mean-difference effect sizes in meta-analyses reported in *Psychological Bulletin* in the years 1990–2013 (Van Erp, Verhagen, Grasman, & Wagenmakers, 2017, https://osf.io/preprints/psyarxiv/myu9c). The distribution of these 162 estimates is shown in Figure 3. Note that we have excluded between-study heterogeneity estimates that were exactly equal to zero, as the prior should reflect knowledge conditional on the assumption that the random-effect model is true; between-study heterogeneity estimates of exactly zero, however, suggest that the fixed-effect model was more appropriate. The distribution of the estimates in Figure 3 suggests that (1) the between-study standard deviations in the field of psychology range from 0 to 1, and (2) there are more small estimates than large ones. These two features are captured by an Inverse-Gamma(1, 0.15) distribution (depicted in Figure 3 as a solid line).[1] Note, however, that this prior distribution does not completely rule out the possibility that between-study heterogeneity is larger than 1; the distribution merely assigns values larger than 1, a relatively small prior credibility. This inverse-gamma distribution resembles the one obtained when maximum-likelihood methods are used to fit an inverse-gamma distribution to the between-study heterogeneity estimates. However, in our opinion, the maximum-likelihood inverse-gamma distribution slightly overemphasizes small between-study heterogeneity values. In Appendix, we present the results obtained under two alternative prior choices for between-study heterogeneity: (1) the maximum-likelihood inverse-gamma distribution; and (2) a Beta(1, 2) prior distribution. The results are robust across all of these prior choices.

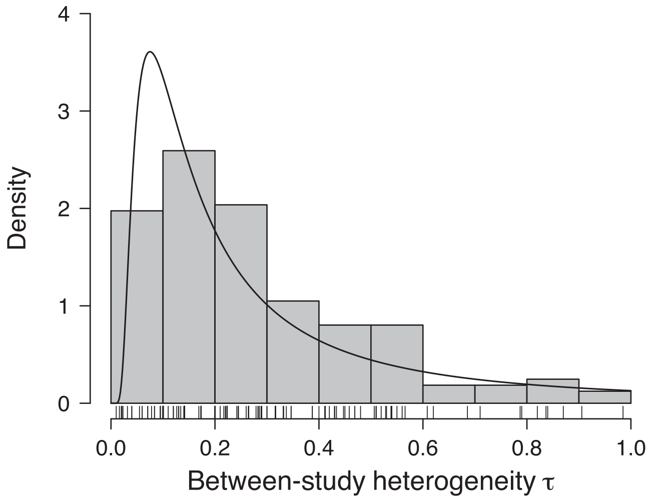

Figure 3. Distribution of the nonzero between-study standard deviations from meta-analyses reported in *Psychological Bulletin* (1990–2013; Van Erp et al., 2017). The informed Inverse-Gamma (1, 0.15) prior distribution is displayed on top. Figure available at http://tinyurl.com/lwfa9rd under CC license https://creativecommons.org/licenses/by/2.0/.

Having specified the models and prior distributions, we needed to compute the probability of the data given each model under consideration. This was achieved by integrating out the model parameters with respect to their prior distributions. For the models for which this was not possible analytically, we evaluated this quantity using numerical integration as implemented in the R package *metaBMA* (Heck & Gronau, 2017). R code for reproducing all analyses can be found on the Open Science Framework: https://osf.io/r2cds/.[2]

Results

Analysis of reported studies: default prior on effect size

Figure 4 displays the results of the Bayesian analysis using the default effect size prior for the studies as reported in this special issue. Note that most studies did not exclude participants who were familiar with the effect, for instance, from viewing the TED talk about power posing, which is currently the second most popular TED talk of all time (https://www.ted.com/playlists/171/the_most_popular_talks_of_all). This analysis is based on a total of 1071 participants. Below, we investigate how the results change when considering only those participants who indicated not to know the power pose effect. The upper part of Figure 4 displays the results of the Bayesian *t*-tests. The left part of the figure displays for each study the median of the posterior distribution for the effect size (grey dots) and a 95% highest density interval (HDI; i.e. the shortest interval that captures 95% of the posterior mass). The right part of the figure shows the one-sided default Bayes factors in favor of H_+ and, for comparison, the (two-sided) *p*-values obtained from classical independent samples *t*-tests.

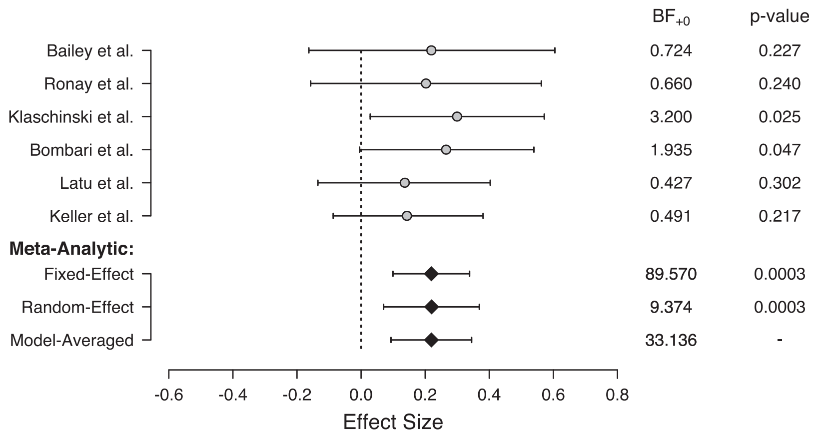

Figure 4. Bayesian model-averaged meta-analysis using the default Cauchy prior with scale $1/\sqrt{2}$ for the standardized effect size. The dots and diamonds correspond to the median of the posterior distribution for the effect size; the lines correspond to the 95% highest density intervals. The one-sided Bayes factors are displayed on the right, flanked by classical two-sided *p*-values. Figure available at http://tinyurl.com/kz2jpwb under CC license https://creativecommons.org/licenses/by/2.0/.

Based on the posterior distributions, it appears that there might be a positive effect. However, this is hard to assess since the 95% HDIs are relatively wide. All Bayes factors except one are between 1/3 and 3 indicating that there is not much evidence for H_+ or H_0. Hence, when considering the individual studies separately, we cannot draw strong conclusions about whether there is an effect or not.

Each study alone does not provide much evidence in favor of either hypothesis; however, a Bayesian meta-analysis allows us to obtain an impression of the overall evidence obtained when considering all studies simultaneously. The lower part of Figure 4 displays the result of the Bayesian meta-analysis using the default Cauchy prior with scale $1/\sqrt{2}$ for the meta-analytic effect size. The black diamonds display the median of the posterior distribution of the meta-analytic effect size for the fixed-effect, random-effect, and model-averaged analysis, and the lines correspond to the 95% HDIs. The model-averaged posterior distribution is obtained by combining the estimates of the fixed-effect and the random-effect model according to their plausibility in light of the data. The lower right part of Figure 4 shows the meta-analytic one-sided Bayes factors and, for the fixed-effect and the random-effect model, the two-sided *p*-value obtained by conducting classical meta-analyses. The meta-analytic fixed-effect Bayes factor equals $BF_{+0} = 89.6$, indicating very strong evidence in favor of an effect of power posing on felt power. The meta-analytic random-effect Bayes factor is less extreme but still indicates evidence for an effect: $BF_{+0} = 9.4$. The observed data support a fixed-effect model more than a random-effect model: the Bayes factor that compares case (1), fixed-effect H_+, to case (3), random-effect H_+ (not displayed), indicates that the data are 4.0 times more likely under the fixed-effect model than under the random-effect model. This is reflected in the model-averaged result: the meta-analytic model-averaged Bayes factor equals $BF_{+0} = 33.1$, indicating very strong evidence in favor of an effect of power posing on felt power. The median of the model-averaged meta-analytic effect size is equal to 0.22 [95% HDI: 0.09, 0.34].

To sum up, the Bayesian meta-analytic results based on the default prior for the effect size provide very strong evidence in favor of the hypothesis that power posing leads to an increase in felt power.

Analysis of reported studies: informed prior on effect size

Next, we consider the results based on the informed *t* prior distribution for the effect size with location 0.350, scale 0.102, and three degrees of freedom (cf. Figure 2). The results are displayed in Figure 5. The effect size posterior distributions for the individual studies clearly show the influence of the informed prior distribution: the posteriors are narrower and slightly shifted toward the location of the informed prior. The individual study one-sided informed Bayes factors are larger than the default ones. This can be explained by interpreting the Bayes factor as an assessment tool of the predictive success of two competing hypotheses. The informed alternative hypothesis makes much riskier predictions than the default alternative hypothesis; however, these risky predictions are rewarded because the observed effect sizes fall within the range of values predicted by the informed hypothesis. Hence, since the predictions match the observed data, the informed hypothesis yields more evidence for the presence of the power pose effect as compared to an alternative hypothesis that specifies a default prior

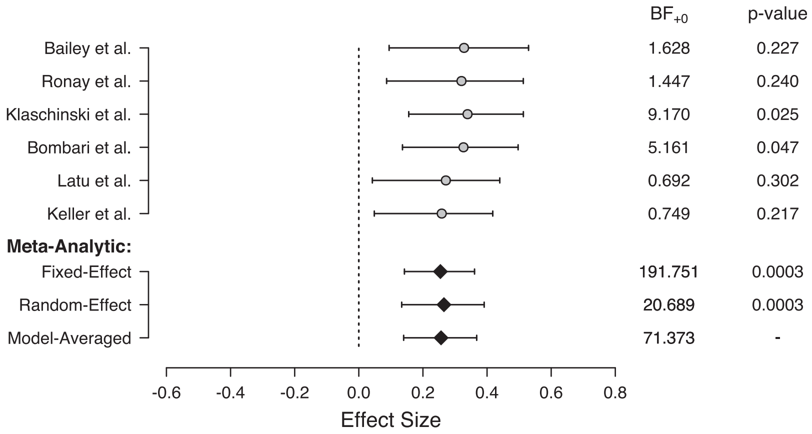

Figure 5. Bayesian model-averaged meta-analysis using the informed *t* prior with location 0.350, scale 0.102, and three degrees of freedom for the standardized effect size (depicted in Figure 2). The dots and diamonds correspond to the median of the posterior distribution for the effect size; the lines correspond to the 95% highest density intervals. The one-sided Bayes factors are displayed on the right, flanked by classical two-sided *p*-values. Figure available at http://tinyurl.com/n8mwfsv under CC license https://creativecommons.org/licenses/by/2.0/.

for the effect size. Nevertheless, only two of the study-specific Bayes factors provide moderate evidence for an effect, whereas the other four provide only anecdotal evidence for H_+ or H_0.

The informed meta-analytic fixed-effect Bayes factor is BF_{+0} = 191.8 indicating extreme evidence in favor of an effect of power posing on felt power. The informed meta-analytic random-effect Bayes factor is less extreme but still indicates strong evidence for an effect: BF_{+0} = 20.7. As for the default prior, the observed data support a fixed-effect model more than a random-effect model, the Bayes factor that compares case (1), fixed-effect H_+, to case (3), random-effect H_+ (not displayed), indicates that the data are 3.9 times more likely under the fixed-effect model than under the random-effect model (not displayed). The informed meta-analytic model-averaged Bayes factor is equal to BF_{+0} = 71.4 indicating very strong evidence in favor of an effect of power posing on felt power. The median of the model-averaged meta-analytic effect size is similar to the default one and is equal to 0.26 [95% HDI: 0.14, 0.37].

To sum up, the Bayesian meta-analytic results based on the informed prior for the effect size provide very strong evidence in favor of the hypothesis that power posing leads to an increase in felt power. The informed analysis yields more evidence for an effect as compared to the default analysis indicating that the successful predictions of the informed hypothesis are rewarded.

Moderator analysis: knowledge of the effect (default prior on effect size)

Next, we investigate whether and how the results change when considering only participants who indicated to be unaware of the power-posing effect. Hence, participants who could guess the goal of the study or were familiar with the power pose TED

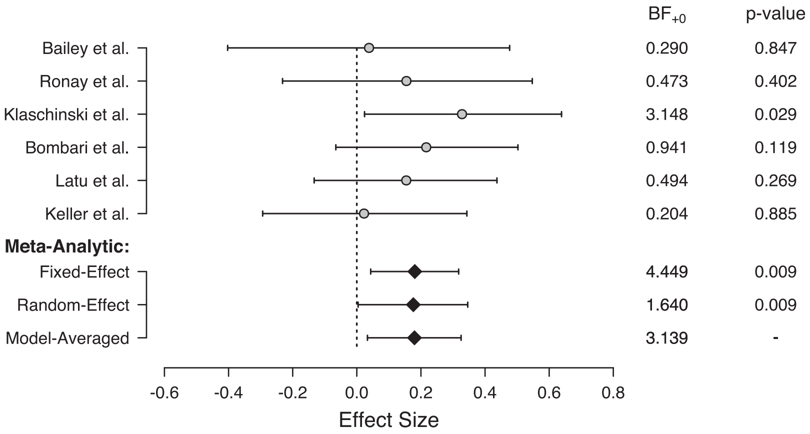

Figure 6. Bayesian model-averaged meta-analysis for the subset of participants unfamiliar with the effect using the default Cauchy prior with scale $1/\sqrt{2}$ for the standardized effect size. The dots and diamonds correspond to the median of the posterior distribution for the effect size; the lines correspond to the 95% highest density intervals. The one-sided Bayes factors are displayed on the right, flanked by classical two-sided *p*-values. Figure available at http://tinyurl.com/kmfcnhz under CC license https://creativecommons.org/licenses/by/2.0/.

talk were excluded in all studies under consideration, leaving a total of 809 participants. Figure 6 displays the results of the Bayesian analysis using the default effect size prior.

Compared to Figure 4, the posterior distributions are shifted toward smaller values and the 95% HDIs are relatively wide (due to the reduced sample size). Three Bayes factors are between 1/3 and 3 indicating that there is little evidence for H_+ or H_0, one Bayes factor indicates moderate evidence for the alternative hypothesis, and two Bayes factors indicate moderate evidence for the null hypothesis. Hence, similar to the previous analysis, when considering the individual studies separately, we cannot draw strong conclusions about whether or not there is an effect.

The lower part of Figure 6 displays the result of the Bayesian meta-analysis using the default Cauchy prior with scale $1/\sqrt{2}$. The meta-analytic fixed-effect Bayes factor equals BF_{+0} = 4.4 indicating moderate evidence in favor of an effect of power posing on felt power. The meta-analytic random-effect Bayes factor equals BF_{+0} = 1.6 indicating only anecdotal evidence for the alternative hypothesis. The observed data support a fixed-effect model more than a random-effect model: the Bayes factor that compares case (1), fixed-effect H_+, to case (3), random-effect H_+ (not displayed), indicates that the data are 3.1 times more likely under the fixed-effect model than under the random-effect model. This is reflected in the model-averaged result: the meta-analytic model-averaged Bayes factor is equal to BF_{+0} = 3.1 indicating moderate evidence in favor of an effect of power posing on felt power. The median of the model-averaged meta-analytic effect size is equal to 0.18 [95% HDI: 0.03, 0.33].

To sum up, when considering only participants who were unaware of the effect and using the default effect size prior, we obtain only moderate evidence for an effect of power posing on felt power. This is in contrast to the results of the previous analysis in which participants who were familiar with the effect were mostly not excluded.

Moderator analysis: knowledge of the effect (informed prior on effect size)

Next, we consider the results based on the informed *t* prior distribution for effect size with location 0.350, scale 0.102, and three degrees of freedom (depicted in Figure 2) when taking into account only participants unfamiliar with the effect. The results are displayed in Figure 7. As before, the effect size posterior distributions for the individual studies clearly show the influence of the informed prior distribution: the posteriors are narrower and slightly shifted toward the location of the informed prior. Again, the individual study one-sided informed Bayes factors are larger than the default ones. Nevertheless, only one Bayes factor provides moderate evidence for an effect, four provide anecdotal evidence for the alternative or the null hypothesis, and one provides moderate evidence for the null.

The informed meta-analytic fixed-effect Bayes factor equals $BF_{+0} = 6.8$, indicating moderate evidence in favor of an effect of power posing on felt power. The informed meta-analytic random-effect Bayes factor is $BF_{+0} = 2.6$, indicating anecdotal evidence for an effect. As for the default prior, the observed data support a fixed-effect model more than a random-effect model, the Bayes factor that compares case (1), fixed-effect H_+, to case (3), random-effect H_+ (not displayed), indicates that the data are 3.0 times more likely under the fixed-effect model than under the random-effect model. The informed meta-analytic model-averaged Bayes factor is equal to $BF_{+0} = 4.9$ indicating moderate evidence in favor of an effect of power posing on felt power. The median of the model-averaged meta-analytic effect size is equal to 0.23 [95% HDI: 0.10, 0.36].

To sum up, when considering only participants who were unaware of the effect, the results were robust with respect to using the informed or the default prior for the effect size. In both analyses, we found only moderate evidence in favor of the hypothesis that power posing leads to an increase in felt power.

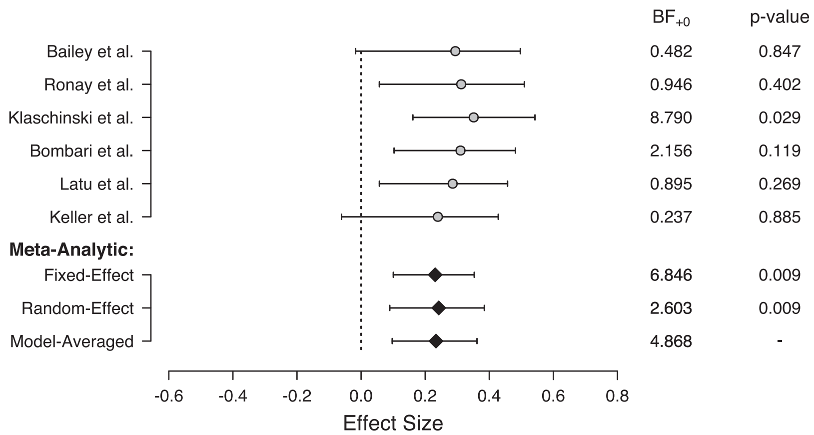

Figure 7. Bayesian model-averaged meta-analysis for the subset of participants unfamiliar with the effect using the informed *t* prior with location 0.350, scale 0.102, and three degrees of freedom for the standardized effect size. The dots and diamonds correspond to the median of the posterior distribution for the effect size; the lines correspond to the 95% highest density intervals. The one-sided Bayes factors are displayed on the right, flanked by classical two-sided *p*-values. Figure available at http://tinyurl.com/n7r4huj under CC license https://creativecommons.org/licenses/by/2.0/.

Discussion

Six preregistered studies in this special issue were subjected to a Bayesian meta-analysis of the effect of power posing on self-reported felt power. The Bayesian approach enabled us to fully acknowledge uncertainty with respect to the choice of a fixed-effect or a random-effect model and allowed us to incorporate prior information about between-study heterogeneity and plausible effect sizes in the field of psychology. The informed prior distribution for between-study heterogeneity was based on an extensive literature review, and we believe it may serve as an informed default in the field of psychology more generally (cf. Rhodes et al., 2015, for a similar approach in medicine).

When considering the studies as reported (i.e. most studies did not exclude participants who were familiar with the effect), we obtained very strong evidence that adopting high-power poses increases subjective feelings of power; this was the case for both the analysis based on a default prior and an informed prior for the effect size. However, when considering only participants unfamiliar with the effect, we obtained only moderate evidence for an effect for both the default and informed effect size prior analysis. This suggests that knowledge of the effect might play a role with respect to the size of the effect of power posing on felt power, although a formal assessment of this possibility requires a different statistical analysis (e.g. Gelman & Stern, 2006; Nieuwenhuis, Forstmann, & Wagenmakers, 2011), the development of which is beyond the scope of this paper. Future studies might investigate this potential moderating effect and explore the extent to which the felt power effect is a demand characteristic. Note that the Bayesian approach allows us to seamlessly update the evidence as more studies become available (e.g. Scheibehenne et al., 2017).

Our meta-analysis focused on the effect of power posing on feelings of subjective power and did not consider behavioral or hormonal measures. Nevertheless, we would like to emphasize that given a set of preregistered studies that include the behavioral and hormonal measures of interest, our methodology can readily be applied to quantify evidence in a coherent Bayesian way for those measures as well.

Notes

1. For computational convenience, it is common practice to assign an inverse-gamma prior to the variance instead of to the standard deviation. Here, we use the inverse-gamma as a convenient summary for the empirical distribution of the between-study heterogeneity estimates.
2. The R code also allows one to explore alternative prior choices easily.

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Appendix

Here, we investigate whether and how the analyses results change under different priors for the between-study heterogeneity. Specifically, we explore two alternative prior choices to the Inverse-Gamma(1, 0.15) prior: (1) the maximum-likelihood inverse-gamma distribution (depicted as a dashed line in Figure A1), and (2) a Beta(1,2) prior distribution (depicted as a dotted line in Figure A1).

Table A1 displays the results for the reported data and Table A2 displays the results for the data of the subset of participants who were unfamiliar with the power pose effect: for all three prior choices for the between-study heterogeneity, the results are highly similar.

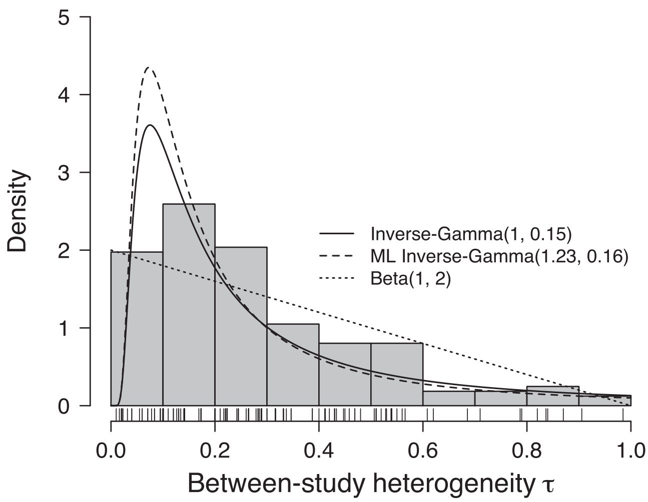

Figure A1. Distribution of the nonzero between-study standard deviations from meta-analyses reported in *Psychological Bulletin* (1990–2013; Van Erp et al., 2017). The informed Inverse-Gamma (1, 0.15) prior distribution is displayed on top as a solid line, the maximum-likelihood inverse-gamma distribution is depicted as a dashed line, and the Beta(1, 2) distribution is depicted as a dotted line. Figure available at http://tinyurl.com/k6yyz6b under CC license https://creativecommons.org/licenses/by/2.0/.

Table A1. Meta-analytic Bayes factors (BF_{+0}) for different prior choices for the between-study heterogeneity (reported data).

	Inverse-Gamma(1, 0.15)	ML Inverse-Gamma	Beta(1, 2)
Meta-analytic fixed-effect Bayes factor	89.6	89.6	89.6
Informed meta-analytic fixed-effect Bayes factor	191.8	191.8	191.8
Meta-analytic random-effect Bayes factor	9.4	10.0	9.2
Informed meta-analytic random-effect Bayes factor	20.7	22.0	20.2
Meta-analytic model-averaged Bayes factor	33.1	32.1	35.1
Informed meta-analytic model-averaged Bayes factor	71.4	69.1	75.5

Table A2. Meta-analytic Bayes factors (BF_{+0}) for different prior choices for the between-study heterogeneity (unfamiliar participants).

	Inverse-Gamma(1, 0.15)	ML Inverse-Gamma	Beta(1, 2)
Meta-analytic fixed-effect Bayes factor	4.4	4.4	4.4
Informed meta-analytic fixed-effect Bayes factor	6.8	6.8	6.8
Meta-analytic random-effect Bayes factor	1.6	1.7	1.7
Informed meta-analytic random-effect Bayes factor	2.6	2.7	2.7
Meta-analytic model-averaged Bayes factor	3.1	3.1	3.3
Informed meta-analytic model-averaged Bayes factor	4.9	4.8	5.1

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SUMMARY

Power poses – where do we stand?

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As editors, reviewers, and authors, we are very pleased with the output of this Special Issue. We received a robust number of interesting and diverse submissions, and we were very lucky to convince one of the authors of the original effect, Dana Carney, to review all submissions and to give feedback from an insider's perspective. This is unique and linked to the preregistration format of this Special Issue, as this most likely would not have happened if all authors had independently approached the original researchers. Finally, the format of preregistration allowed us to run a meta-analysis across the individual studies without any file drawer bias (or the need to correct for it). In sum, we believe that peer-reviewed preregistration projects and associated meta-analyses form the gold standard for future similar endeavors. This format allows for the optimal use of resources and collaboration among original authors, new authors, and meta-analytic perspectives. Still, there is room for improvement and learning. Although we were able to coordinate the measure of felt power to be included across the studies in the special issue, further coordination of more dependent variables or similar designs could be beneficial. Such procedures can provide a more robust basis to judge where we stand with a research field.

While some colleagues have hoped that this Special Issue would provide the definitive answer on the replicability and evidence for or against power poses, we believe that conclusions need to be appropriately tempered. Looking across the studies, it is clear that an effect on felt power was observed. What this *means*, in terms of whether this is more than just a demand characteristic, is still unclear and can use further investigation. Considering the preregistered predictions for the behavioral and hormonal measures, however, presents a clearer picture:

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- Bailey, LaFrance, and Dovidio (2017) sought to investigate an interaction of power posing, target gender, and participant gender. They did not replicate the effect of power poses on risky behavior.
- Bombari, Schmid Mast, and Pulfrey (2017) planned to test whether imagined or performed power poses had similar effects. They did not replicate the effect of power poses on risky behavior.
- Klaschinski, Schnabel, and Schröder-Abé (2017) wanted to replicate the effects of power posing on dominance and social sensitivity in an interview context, but they did not replicate the effects.
- Jackson, Nault, Smart Richman, LaBelle, and Rohleder (2017) sought to test the effect of power posing on self-concept. Although a preliminary study obtained an interesting effect, they did not replicate this in the higher-powered, preregistered study.
- Keller, Johnson, and Harder (2017) wanted to test whether awareness of the function of power poses moderates their effectiveness. They did not replicate the basic power pose effect.
- Latu, Duffy, Pardal, and Alger (2017) tested an interesting dependent variable in the context of power poses, persuasive messages. They did not observe any effect of power poses on persuasive message perception.
- Ronay, Tybur, van Huijstee, and Morssinkhoff (2017) wanted to investigate the mediating role of testosterone and overconfidence on the link between power posing and risk taking, but they did not replicate the effect.

As can be seen, there was virtually zero effect of power poses on any of the behavioral or hormonal measures. However, a strong contribution of preregistration is evident in the exploratory analyses conducted across the different studies. Most of the studies did reveal some effects of power poses on non-preregistered, exploratory analyses. The preregistration format, rather than inhibiting scientific discovery or exploration, actually then points researchers to the next direction for their research, while at the same time making it clear to the reader that such obtained effects were exploratory and *not* confirmatory.

Prior to our special issue, there were other attempts to replicate the power pose effect (Garrison, Tang, & Schmeichel, 2016; Ranehill et al., 2015). Ranehill et al. could not replicate the effect on hormonal level, but found the manipulation to influence felt power. Garrison et al. also attempted to replicate the original effect and extend it with a dominance manipulation via eye gaze. They also could not find the effect on their dependent variable, an ultimatum game, nor on felt power. Based on the papers in the special issue, and prior replication attempts, one could conclude that the power pose effect on behavioral outcomes does not replicate.

Next to these individual papers and replication attempts, it is relevant to take a more abstract view. We were able to conduct a Bayesian meta-analysis on the papers of the Special Issue that contained the felt power variable (Gronau et al., 2017). In merely eye-balling the results of the studies in this Special Issue, we would have guessed that there was no overall effect. But we were surprised to find the (small) overall effect for felt power. Critical colleagues will remark that this finding says little about the actual power pose effect, because they see felt power as a manipulation check. Even if one would side with this argument, then there is a successful manipulation, which demands further analysis of its effects on a number of suggested and novel dependent variables and their boundary conditions. Jackson et al. (2017) identified cognitive flexibility, Bombari et al. (2017) participant gender, and Klaschinski et al. (2017) extraversion: This is where we believe future research should start off, and can help to contribute to unpacking the power pose effect. *CRSP* as a journal is happy to provide a platform for this.

Disclosure statement

No potential conflict of interest was reported by the authors.

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