# Social and Personal Power

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# Commentary

# Social and Personal Power: A Closer Examination

## An Invited Commentary to Mayiwar and Lai (2019)

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**Abstract:** We thank Mayiwar and Lai (2019) for conducting a replication of Study 1 in Lammers, Stoker, and Stapel (2009) but disagree with their conclusions. Instead, we conclude that their results largely support ours. The results replicate the theoretical distinction between social and personal power, replicate that recalling social versus personal power produces dissimilar levels of stereotyping, and replicate that they produce similar levels of behavioral approach orientation. We discuss the weaker results on stereotyping as the result of the use of an unreliable measure and conclude that despite this, the data are consistent with the possibility of medium-sized effects. We discuss the null-effects on behavioral approach (compared to control) as the result of a change in instructions. We end with a discussion on the implications for the social–personal power distinction and the power literature in general, with a particular focus on how future replication efforts may provide even greater insight.

**Keywords:** power, stereotyping, approach tendencies, replication

One of the most characteristic aspects of the social scientific literature on power is that the concept of power has been defined in so many different ways (Fiske & Berdahl, 2007). We believe that this definitional heterogeneity is intrinsic to power as an amorph and contested cluster concept (Connolly, 1993; Weber, 1914/1978). In other words, positions of power can be experienced in many different ways. Understanding the psychology of power requires us to understand how these differences in experience matter, for example in shaping downstream consequences. One such difference is whether power is experienced in social or in personal terms. Social power can be defined as the ability to control or influence others in an interdependent relationship, while personal power refers to the ability of one person to ignore the influence of others and independently get what she wants (Van Dijke & Poppe, 2006).

We proposed that one important distinction between these two experiences of power, is that social power creates a sense of interdependence, while personal power creates a sense of independence (Lammers, Stoker, & Stapel, 2009). Building on the idea that people expend the effort to individuate only when needed – for example when they have an interdependent relationship with the target (Fiske & Neuberg, 1990; Neuberg & Fiske, 1987), we proposed that the experience of social and personal power would lead to *dissimilar levels of stereotyping*. Specifically, an experience of social power would lead to less stereotyping than an experience of personal power, because the former involves interdependence and the latter independence. To further demonstrate the importance of this independence–interdependence distinction, we argued that the two forms of power experiences should produce similar effects if the effect at hand does not map onto this independence–interdependence distinction. To show this, we also measured effects on approach tendencies and expected that the two power experiences would lead to *similar levels of approach* – a prediction that was already well-supported by ample evidence that power is associated with increased approach tendencies (Anderson & Galinsky, 2006; Boksem, Smolders, & De Cremer, 2012; Galinsky, Gruenfeld, & Magee, 2003; Guinote, 2007; Keltner, Gruenfeld, & Anderson, 2003; Lammers, Galinsky, Gordijn, & Otten, 2008; Smith & Bargh, 2008). We tested these predictions in two studies – an experiment in which the two experiences of power were manipulated independently ($N$ = 113), and a correlational study in which the actual managers in the field reported on their experiences of social and personal power, measured simultaneously ($N$ = 3,082).[1]

[1] Note that the data reported in Lammers, Stoker, and Stapel (2009) were collected by Lammers and Stoker and analyzed by Lammers. Stapel's contribution to the manuscript was only textual. For an explanation of the relevance of this, see https://www.commissielevelt.nl/.


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Mayiwar and Lai (2019; from now on: M&L) approached Lammers in 2017 to replicate the reported experiment. We are enthusiastic about replication efforts and therefore gladly shared our materials. We are also strong proponents of publishing results of replications (independent of their outcomes), and therefore Lammers was happy to support M&L in analyzing and interpreting the data, to assist them in drafting these findings into a manuscript, and to support them in order to navigate the publication process. We thank the editor for allowing us the opportunity to also comment on M&L's findings. We do so in three sections that focus on the manipulation and theoretical distinction, on the effects on stereotyping, and on the effects on behavioral approach, respectively. In our discussion, we interpret the implications of these findings for Lammers et al. (2009) and for the wider literature on the psychological effects of power.



## Manipulation and Theoretical Distinction

The first aim of the reported experiment by Lammers et al. (2009) was to show the theoretical distinction between feelings of social and personal power. In support of the theoretical distinction, M&L replicated the same clear factorial structure and high-scale reliability coefficients. The replication also found strong evidence that the manipulation is effective in selectively inducing specific feelings of social and feelings of personal power. As can be seen in Table 1 of M&L, the personal (vs. social) power manipulation selectively induced corresponding feelings of personal (vs. social) power, while leaving non-corresponding feelings of social (vs. personal) power unaffected, compared to the control condition. Compared to the original data, the replication data offer even stronger evidence of the effectiveness of the manipulation, given the stronger and more pronounced pattern of selectively increased corresponding feelings and selectively unaffected non-corresponding feelings (Table 1). In summary, the replication further supports the first aim of Lammers et al. (2009).

## Stereotyping

The second aim of Lammers et al. (2009) was to show that social and personal power produce opposite effects on stereotyping. In their replication, M&L found a significant, medium-sized Omnibus test effect of condition, $p = .015$, $\eta_p^2 = .056$, and a similar pattern of means as in the overall study, with highest levels of stereotyping in the personal and lowest in the social power condition, with the control condition in between. M&L also correctly note that the observed effect size is smaller in their replication than in the original. One particular reason why M&L may have found a weaker effect, is that their measure may not accurately measure Norwegian students' gender stereotyping.

Stereotypes of women change over time (Diekman & Eagly, 2000) and therefore a measure of sexist stereotyping that connected well to widely shared stereotypes in 2009, may no longer be as reliable in 2019. Furthermore, a first indication is that the items used in M&L's stereotyping scale indeed show poor internal reliability: Cronbach's α is .514.

Furthermore, unlike in the original data, the mean responses to the stereotyping measure in the replication score consistently (in all conditions) below the neutral midpoint. Also, on 8 of the 10 items used to construct the stereotyping composite measure, the majority of participants disagree with the stereotypical description used. For example, only 9.5% of the sample agrees (somewhat, moderately, or strongly agree) that the stereotypical item dedicated *(dedikert)* applies to the female target, while 75% disagrees with that (15.5% neutral). This suggests that the scale does not connect well to Norwegian students' current gender stereotypes. Given the low α score, low reliability, and low validity of the stereotyping measure, a sizeable portion of the observed variance may be due to randomness, suggesting that the true effect size of the manipulation may be larger than observed by M&L (Trafimow, 2016).

Another issue is that M&L use conventional null-hypothesis testing to conclude that only the difference between the social and personal power manipulation is significant, at $p = .004$, while the effects of these two manipulations compared with the control condition do not replicate, because they are not statistically significant, at $p = .080$ and $p = .267$, respectively. Such a conclusion based on the simple use of null-hypothesis testing in interpreting replication results has recently been criticized (Anderson & Maxwell, 2016; Maxwell, Lau, & Howard, 2015). Instead, we calculated confidence intervals around the effects observed by M&L and found a medium-sized difference in the expected direction between social and personal power, $d = .603$, 95% CI$d$ [0.201; 1.00], and small-to-medium-sized differences in the expected direction between social power and control, $d = .328$, 95% CI$d$ [−0.072; 0.726], and between personal power and control, $d = .239$, 95% CI$d$ [−0.159; 0.636]. Although some are not significant, these confidence intervals clearly do not support the conclusion that there is no difference between conditions. In fact, they are more consistent with the conclusion that there are small- or medium-sized differences between conditions in the expected direction.

In summary, despite the use of an unreliable measure that lacks internal validity and that does not connect well to the sample's current gender stereotypes, M&L find a significant, medium-sized Omnibus effect of condition and small- to medium-sized differences between individual

conditions in the expected direction. The most important prediction by Lammers et al. (2009), namely that social and personal power produce dissimilar levels of stereotyping, is replicated at $p = .004$. Although weaker than in the original, these results support Lammers et al.'s (2009) original conclusions.

## Approach

The third and final aim of Lammers et al. (2009) was to show that the social and personal power manipulation produced comparable effects on behavioral approach tendencies. Consistent with this, M&L also found no difference between the social and personal power conditions on behavioral approach tendencies. Inconsistent with their findings and also inconsistent with much of the literature (for overviews, see Galinsky, Rucker, & Magee, 2015; Hirsh, Galinsky, & Zhong, 2011; Keltner et al., 2003), M&L also found no difference between either experimental power condition and control. M&L suggest that it is possible that the effect of power on approach is actually very small or nonexistent or that the recall-based power manipulation may be unreliable. In the General Discussion, we discuss both claims. Here we only focus only on the Methods and Results.

We propose that there is an alternative and more parsimonious explanation for the absence of any effect of power on approach in M&L's replication. Participants in the control condition of M&L's replication recalled their last time *shopping*. In contrast, Lammers et al. (2009) asked participants in the control condition to recall their last time *visiting the supermarket for groceries*. Although the difference is subtle, it is likely that the potentially exciting memory of searching for shoes, fashion, or bags (i.e., shopping) recalled by the Norwegian participants in the replication, activated more disinhibited and approach-related thoughts than the memory of the mundane and potentially boring activity of selecting vegetables, fruit, and bread (i.e., grocery shopping) recalled by the Dutch participants of the original study. In other words, a small change in the wording of the control condition may have led to a calibration problem, where the control instructions in the replication activated disinhibited thoughts, thus increasing scores on the behavioral approach measure in the control condition and thus masking any effect induced by the power-recall task in the experimental conditions.

We note that the empirical results support this explanation (see Table 2 in M&L). In particular, where the mean scores on behavioral approach orientation of the two experimental conditions are similar between the original study and replication (the 95% CI of the replication overlap with the original means), the means of the control condition are dissimilar. Specifically, the replication means are more than 1 *SD* higher than the original means, consistent with this explanation. Also, where the means of the control condition are close to the midpoint in the original, they are more than 1 *SD* above the mid-point in the replication. These two observations suggest that because control participants in the replication recalled more disinhibited experiences of shopping, rather than of buying groceries, they also experienced more elevated levels of approach – thus hiding any effect due to recalling an experience of power.

To further test this possibility, we approached M&L with the suggestion to content analyze participants' written responses. This would have allowed us to test whether participants in the control condition indeed generated disinhibited thoughts about shopping. Unfortunately, M&L were unable to share these written responses because they had already discarded the raw data. That is, after entering the quantitative part of the data, they threw away the booklets, including participants' qualitative responses to the manipulation. This premature discarding of the raw data thus prevents further testing of our alternative explanation.

# General Discussion

We are pleased to see that one study conducted as part of our research into the effects of social and personal power was subject of a replication study. We discuss the findings and implications of this replication in two parts, focusing first on the implications for the claims made in the replicated manuscript (Lammers et al., 2009) and next on the wider literature on the psychology of power.

## Implications for the Social–Personal Power Distinction

The results of this replication largely support the predictions made by Lammers et al. (2009) and tested in their Study 1. In particular, the replication supports the theoretical distinction between social and personal power; it supports the prediction that the social (vs. personal) power recall manipulation can activate specific feelings of social (vs. personal) power; it supports the prediction that the two experiences of power produce opposite effects on variables that map onto the independence–interdependence distinction, such as stereotyping; and it supports the prediction that the two do not produce opposite effects on variables that do not map onto that distinction, such as behavioral approach tendencies. Although the manipulation produced a weaker overall effect on stereotyping in the replication by M&L ($N = 295$), possibly due to the use of an unreliable measure of the dependent variable, it does



 

show a medium-sized effect in the same direction as found in the original experiment ($N$ = 113), and found in the correlational study on actual power differences in management ($N$ = 3,082). The theoretical implication of the replication is thus added support for the social–personal power distinction, but the methodological implication is that future replication efforts should use a better-powered design, primarily using a more reliable and valid dependent measure (of stereotyping).

## Implications for the Wider Power Literature

Although the replication by M&L found no difference in behavioral approach between any of the two power conditions (consistent with Lammers et al., 2009), it also found no difference with the control condition. M&L raise the possibility that the effect of power on approach is actually very small or non-existent. If so, this would not only be inconsistent with Lammers et al. (2009), but it would also be inconsistent with much (if not all) of the published literature on power. To give a few examples, recalling an experience of power (similar as here) has been shown to affect responses on explicit measures of BIS/BAS activation (also similar as here) and other measures of approach-related cognition, behavior, and neural activity measured using EEG (Anderson & Galinsky, 2006; Boksem et al., 2012; Galinsky et al., 2003; Guinote, 2007; Lammers et al., 2008; Lammers, Stoker, & Stapel, 2010; Smith & Bargh, 2008). Furthermore, the same effect of power on approach has been demonstrated using other manipulations of power, such as by giving control over resources or assigning roles in a hierarchy. Finally, it has been demonstrated using measures of actual power differences, such as between high and low management position or between strong and weak feelings of power (for an overview, see Galinsky et al., 2015). In fact, there is even evidence for the link between power and behavioral approach in non-human primates. For example, monkeys and apes with a higher position in the hierarchy of their social group (corresponding to a high-power position in human hierarchies) are more disinhibited and bold in their behavior and attention than their more cautious and inhibited lower-ranked counterparts (Chance, 1967; Shepherd, Deaner, & Platt, 2006). In summary, the idea that power increases a behavioral approach (or disinhibition) tendency is considered to be one of the best-supported conclusions in research on power (Galinsky et al., 2015; Hirsch et al., 2011; Keltner et al., 2003). Given the strength of this evidence, we warn against interpreting the current null-effect as strong evidence against the relation between power and behavioral approach.

The second explanation offered by M&L draw is that the particular recall-based power manipulation is "inadequate". They support this idea with a literature discussion on the unreliability of that manipulation (see section: The Current Research).

Unfortunately, this discussion is incorrect. First, M&L cite a study by Ranehill and colleagues to support their argument that the recall-based power manipulation often produces null-effects. However, that study does not use a recall-based power manipulation, but instead uses power posing, which is quite different (Ranehil, Dreber, Johannesson, Leiberg, Sul, & Weber, 2015).[2] Second, M&L cite research by Zhang and Smith (2018), to further support the argument that recall-based manipulations are unreliable. However, these authors actually argue in favor of the effectiveness of the recall-based power manipulation.[3] Third and finally, M&L cite Cesario (2014) who indeed argues that priming manipulations (such as the power-recall) often have low replicability. In our understanding, however, Cesario's position is that this low replicability can be expected a priori because priming manipulations are often highly sensitive to variations in experimental features. Therefore, Cesario argues that although priming manipulations may occasionally produce null-effects, such failures to replicate should not be interpreted as strong evidence against the existence of an effect, but should rather be used as part of a systematic investigation what features of a replication may be responsible for null-findings.

Consistent with Cesario's (2014) recommendation, we tried to answer the question why the predicted effect of approach tendencies did not replicate, while the predicted effect on stereotyping did replicate (albeit weaker). We have argued that a change in the formulation of the control condition instructions – in particular, by changing grocery shopping to shopping – may have activated more disinhibited thoughts in the control condition and may have thus masked any effect of the power manipulation. Although we could not test this hypothesis in detail in a content analysis (due to the data being discarded), we note that patterns in the data support this idea. Future research could test this by experimentally changing the formulation of the control condition instructions and by analyzing the qualitative responses to the recall instructions.

[2] In addition, Ranehill et al. (2015) do find that the power posing manipulation "works", in the sense that it affects the manipulation check. They find no effects on physiological measures and on financial risk taking. For a discussion on the difference between these manipulations, see Huang, Galinsky, Gruenfeld, and Guillory (2011).

[3] Zhang and Smith (2018) report an unsuccessful replication of Joshi and Fast (2013) and conclude that although the recall manipulation does reliably induce a sense of power, it does not reliably affect the specific effect postulated by Joshi and Fast.



 

Of course, there may also be other explanations for the failed replication of the effect of approach. This includes the possibility that the effect is actually very small or negligible. But before concluding this, it would be prudent to systematically analyze the role of potential moderators. In particular, research has shown that power-induced effects on approach-related variables can be reduced or reversed if participants have difficulty recalling the episode of power or powerlessness (an ease of retrieval effect) or if participants consider the recalled example of power to be illegitimate or undeserved (Lammers et al., 2008; Lammers, Dubois, Rucker, & Galinsky, 2017). Other findings suggest that perceptions of the stability of the power position, feelings of competence and status, and differences in power motivation may also be important moderators (Fast & Chen 2009; Fast, Halevy, & Galinsky, 2012; Jordan, Sivanathan, & Galinsky, 2011; Maner, Gailliot, Butz, & Peruche, 2007). Future replication efforts may want to include such or related measures, designed to test these and other potential moderators, in order to make the results of such replications more informative, independent of their success.

## Conclusion

We thank M&L for conducting a replication of Study 1 in Lammers et al. (2009) but disagree with their conclusions. We conclude that M&L replicate the theoretical distinction between social and personal power, replicate that social and personal power produce dissimilar levels of stereotyping (albeit weaker), and replicate that social and personal power produce similar levels of behavioral approach. Although it is true that they find no effect between either power manipulation and the control condition, we warn against concluding on the basis of this that this often-observed effect is nonexistent or that this often-used manipulation is unreliable. Instead, we have pointed to the possibility that a change in instructions masked the effect. We welcome future replication efforts of this and other studies and hope these make use of our suggestions on how to design these studies to provide even greater insight.



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