

**Replication of Study 1 in ‘Differentiating Social and Personal Power: (...)’
by Lammers, Stoker, and Stapel, 2009.**

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Author Contributions

Lewend Mayiwar initiated the project, oversaw data collection, and drafted the first version of the manuscript. Linda Lai performed additional statistical analyses and drafted the final version of the manuscript. Both authors are responsible for the final and present version of the manuscript.

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Abstract

We performed an independent, direct and better powered ($N = 295$) replication of Study 1, an experiment ($N = 113$) by Lammers, Stoker, and Stapel (2009). Lammers and colleagues distinguished between social power (influence over others) and personal power (freedom from the influence of others) and found support for their predictions that the two forms of power produce opposite effects on stereotyping, but parallel effects on behavioral approach. Our results did not replicate the effects on behavioral approach, but partially replicated the effects on stereotyping. Compared to personal power, social power produced less stereotyping, but neither form of power differed significantly from the control condition, and effect sizes were considerably lower than the original estimates. Potential explanations are discussed.

Keywords: power; stereotyping; behavioral approach; priming; replication.

Introduction

Over the past decades, an increasing number of studies have explored the psychological, interpersonal, and behavioral effects of possessing power. The corruptive, self-serving and anti-social effects of power have received considerable attention, and are no longer considered newsworthy (Cislak, Cichocka, Wojcik, & Frankowska, 2018). The possession of power has been found to increase the tendency to: distance oneself socially from individuals with lower power; generate self-serving attributions of their behavior (Kipnis, 1972; Kipnis, Castell, Gergen, & Mauch, 1976); objectify others (Gruenfeld, Inesi, Magee, & Galinsky, 2008; Inesi, Lee, & Rios, 2014); form stereotypical perceptions of others (Fiske, 1993; Fiske & Dépret, 1996; Goodwin, Gubin, Fiske, & Yzerbyt, 2000; Guinote & Phillips, 2010); and miscomprehend how other people see, think, and feel (Galinsky, Magee, Inesi, & Gruenfeld, 2006). Power has also been associated with lower levels of enacted justice toward others (Blader & Chen, 2012) and higher levels of cheating (Lammers, Stapel, & Galinsky, 2010).

Other studies, however, suggest that power may have less self-serving and more prosocial effects, such as more accurate and individuated perceptions of others (Chen, Ybarra, & Kiefer, 2004; Overbeck & Park, 2001, 2006), enhanced interpersonal sensitivity (Mast, Jonas, & Hall, 2009), and a sense of social responsibility towards the completely powerless (Handgraaf, Van Dijk, Vermunt, Wilke, & De Dreu, 2008). Power has also been found to strengthen the positive association between prosocial orientation and empathic accuracy (Côté et al., 2011).

Due to these and other inconsistent findings, an increasing number of scholars have attempted to identify factors that determine and potentially moderate the direction of effects of power (e.g., Bendahan, Zehnder, Pralong, & Antonakis, 2015; DeCelles, DeRue, Margolis, & Ceranic, 2012; Sturm & Antonakis, 2015). Lammers, Stoker, and Stapel (2009) propose that the mixed results regarding the effects of power may be attributed to the ways in which

power is typically defined and a frequent lack of conceptual and empirical distinction between two forms of power: social power and personal power. According to Lammers and colleagues, social power refers to “the ability to influence others”, whereas personal power reflects “the ability to do and get what you want, without being influenced by others”. The distinction between the ability to influence and control others (social power) and the independence from the influence and control from others (personal power) is reflected in many definitions of power and in the conceptualization of power as asymmetric control over critical resources (e.g., Dépret & Fiske, 1993; Emerson, 1962; French and Raven, 1959). Although conceptualizations vary across studies, the effects of personal power have been addressed in several studies, prior to and following Lammers and colleagues’ (2009) study (e.g., Bendahan et al., 2018; Galinsky, Magee, Gruenfeld, Whitson, & Liljenquist, 2008; Pratto, 2016; Van Dijke & Poppe, 2006).

Lammers and colleagues argue that since social power is associated with interdependence with others, whereas personal power is associated with independence from others, the two forms of power will produce different effects when the interdependence-independence distinction is relevant and similar effects when this distinction is irrelevant. According to Lammers and colleagues, stereotyping represents a type of effect that is relevant to the interdependence-independence distinction, whereas behavioral approach represents a dependent variable that is unrelated to the interdependence-independence distinction. Previous research has shown that power tends to activate behavioral approach tendencies through positive affect, attention to rewards, and automatic information processing (Anderson & Galinsky, 2006; Keltner, Gruenfeld, & Anderson, 2003; Galinsky, Gruenfeld, & Magee, 2003; Lammers, Galinsky, Gordijn, & Otten, 2008; Lammers, Stoker, & Stapel, 2010; Maner, Kaschak, & Jones, 2010; Smith & Bargh, 2008). Hence, Lammers and colleagues propose that whereas personal power provides independence and the freedom to rely on cognitive

heuristics such as stereotypes of others, social power involves interdependence and responsibility for others and should elicit less stereotyping. In contrast, due to the assumed irrelevance of the interdependence-independence distinction in behavioral approach, Lammers and colleagues propose that the two forms of power will produce parallel effects, i.e., that both forms of power will elicit an increase in behavioral approach tendency.

Lammers and colleagues tested their hypotheses in an experiment with Dutch psychology students ($N = 113$) and a large correlational study with Dutch working professionals ($N = 3082$). Consistent with their predictions, results from both studies suggested that compared to the control condition (general power), social power reduced stereotyping whereas personal power increased stereotyping, and that the two forms of power produced parallel effects on behavioral approach. The experiment also offered support to the empirical distinction between social power and personal power by means of several manipulation checks.

The Current Research

The aim of the current study is to replicate the experimental test of the empirical distinction between social and personal power and their respective effects on stereotyping and behavioral approach. To our knowledge, this is the first replication of Lammers et al.'s (2009) experimental study (Study 1). Although Lammers and colleagues also present support for the proposed pattern of effects in a correlational study (Study 2), the nature and direction of causality cannot be determined based on correlational data, and the results of an open survey may be influenced by, for example, selection effects as well as omitted variable problems (Antonakis, Bendahan, Jacquart, & Lalive, 2010; Sturm & Antonakis, 2015).

Furthermore, although the findings from the original experiment support the expected patterns of effects, the sample size is very small ($N = 113$), and the average number of

participants in each of the six experimental groups falls below twenty. In order to provide more precise estimates of effects and more confidence in potential null results, a better powered replication of the experiment is called for.

In view of recent research that has demonstrated that many well-cited classic and contemporary findings from psychological experiments fail to be replicated, replications are increasingly being encouraged to put reported findings at test (Unkelbach, 2016). Concerns have also been raised about the validity of the type of power primes used in the original study (recall-based power priming) as well as the replicability of results based on such primes (Cesario, 2014; Kahneman, 2012; Newell & Shanks, 2014; Sturm & Antonakis, 2015). The results from several studies that have employed recall-based priming have failed to be replicated (see, e.g., Zhang and Smith, 2018; Ranehill et al., 2015). Although priming effects can be expected to be highly sensitive to variations in experimental procedures and samples (Cesario, 2014), many scholars argue that initial replication attempts should be direct or as close to the original study as possible (Earp & Trafimow, 2015; Simons, 2014). Following this advice, we aimed to replicate the original experiment as closely as possible with respect to procedures, sampling, and analytic strategy. Our replication was also paper-based, and the sample was similar to the original, i.e., a student sample from a Western-European country characterized by low power-distance (Hofstede, 1983).

Method

We obtained the materials from the original experiment from the lead author of the original study. The materials were translated from Dutch into Norwegian and pretested on a small group of graduate students ($N = 5$).

Our replication follows the original procedure and employs a 3 (manipulation: personal power, social power, or control) x 2 (dependent variable: stereotyping, approach) between-participants design.

Participants and Design. We set total sample size a priori to $N = 300$. This corresponds to the replication sample size recommended by (Simonsohn, 2015), which is 2.5 times the size of the original sample. After deleting incomplete responses, our final sample consisted of 295 undergraduate and graduate students, compared to 113 in the original study. Participants were randomly assigned to either the stereotyping condition ($N = 148$) or the behavioral approach condition ($N = 147$). There were no less than 48 participants in any of the six experiment groups. 288 participants completed the manipulation checks.

Of the total sample, 114 were male, 180 were female, and 3 were not indicated. Age was reported in intervals. Almost all (96.3%) participants were younger than 30 years old, and more than half (56.5 %) were between 22 and 25 years old.

Procedure. Participants were recruited among students at two large universities in Norway (University of Oslo and BI Norwegian Business School) over two weeks in November 2017. Students were approached in common areas inside the university campuses and invited to participate in a study. No information was given about the purpose of the study.

After accepting the invitation to participate, participants were handed a paper booklet that included: (a) instructions; (b) power manipulation (social, personal, or general power); (c) dependent variable (measures of stereotyping or approach tendencies); (d) manipulation check; (e) demographic questions; and (f) a question about whether they knew the purpose of the study. All participants completed their tasks individually and were monitored to ensure that they refrained from communicating with others. None of the participants correctly guessed the nature or purpose of the study.

Manipulation. Similar to the original procedure, participants in the personal power condition were asked to recall a particular situation in which “*you personally had power, where you were independent from the influence of others*”, participants in the social power condition were asked to recall a situation *in which you had power over another individual or individuals*”, and participants in the control condition were instructed to recall the last time they were “*shopping/purchasing something*”. All participants were requested to describe this situation, what happened, and how they felt.

Measures:

Stereotyping. Participant in the stereotyping condition ($N = 148$) read a short story about a girl who behaved in an ambiguously stereotypically female manner (e.g., dependent, indecisive, empathetic). No changes in original the story were made except for the name of the girl, which was substituted with a typical Norwegian female name. Stereotyping was measured by asking participants to rate the girl in the story on 10 female-stereotypical traits, on a 9-point scale from “*Not at all*” (1) to “*Very much*” (9). The traits included were translated directly from the Dutch original version of the materials: dependent, dedicated, social, worried, sensitive, modest, naïve, greedy, friendly, and nice. No changes were made except from reversing the trait “*dependent*” to “*independent*”, given that the direct translation of “*dependent*” into Norwegian is associated with addiction and holds a strong negative connotation. Following the original procedure, the mean score across items was used for hypothesis testing.

Behavioral Approach. Participants in this condition ($N = 147$) completed a 12-item behavioral approach scale (e.g., “*Currently, I would like to do my best to get the things I want*”). Participants rated each item on a 9-point scale from “*Not at all*” (1) to “*Very much*” (9). Again, following the original procedure, the mean score across items was used for hypothesis testing.

Manipulation Checks. The manipulation check included twelve items, of which four items measured feelings of personal power (self-governing, unrestrained, independent, and free), four items measured social power (feeling accountable for given results and/or actions, feeling as if leading others, feeling responsible for directing others, and feeling as taking on responsibility), and four items measured general power as a control (feeling important, powerful, strong, and forceful). All items were measured between “*Not at all*” (1) and “*Very much*” (9).

Results

Manipulation Checks. Following the analytic procedures reported by Lammers and colleagues, we conducted a factor analysis and correlation analysis of the 12 items included in the manipulation checks. Confirmatory factor analysis (with Oblimin rotation) revealed the same distinct three components as in the original study. Each item produced satisfactory loadings ($> .61$) on their respective component and lower loadings ($< .37$) on the other components. (Results from the factor analysis are reproduced in Appendix A.)

Correlation analyses showed that general power correlated strongly with social power ($r = .61, p = .000$), which corresponds to the results of the original study ($r = .68, p = .000$). General power (control) and personal power were not correlated, which is also similar to the original study. Finally, a weak and negative correlation was found between personal power and social power ($r = -.18, p = .003$), whereas no significant correlation was identified in the original study.

The results from the three manipulation checks were tested by analyses of variance (ANOVA) and contrast analyses, yielding the same pattern of results as in the original study. First, there was a significant overall effect of condition on the social power manipulation check, $F(2, 285) = 84.47, p < .001, \eta^2_p = .37$. Contrast analyses revealed that participants in

the social power condition reported stronger social power than participants in the personal power condition ($t(285) = 8.86, p = .000$) and control condition ($t(285) = 12.71, p = .000$), respectively. Second, there was a significant overall effect of condition on the personal power manipulation check, $F(2, 285) = 25.62, p = .000, \eta^2_p = .15$. Contrast analyses showed that participants in the personal power condition reported stronger personal power than participants in the social power condition ($t(285) = 6.95, p = .000$). The difference between the personal power condition and the control condition, however, was less robust and showed statistical significance only when performing a one-tailed test ($t(285) = 1.89, p < .003$). Finally, there was a significant overall effect of condition on the general power manipulation check, $F(2, 285) = 45.89, p = .000, \eta^2_p = .24$. Contrast analyses also revealed that participants in the control condition reported lower general power compared to participants in the social power condition ($t(285) = 8.84, p = .000$) or the personal power condition ($t(285) = 7.59, p = .000$), which is similar to the pattern reported by Lammers and colleagues. Table 1 gives means, standard deviations, and sample sizes within each experimental condition.

Table 1. Means (and SDs) of manipulation checks across conditions.

Condition	MC: Personal power		MC: Social Power		MC: General power (control)	
	Original*	Replication	Original	Replication	Original	Replication
Personal power	7.45 (1.08)	7.34 (1.44)	6.38 (1.54)	5.06 (1.76)	6.56 (1.51)	5.64 (1.83)
Social power	6.17 (1.39)	5.80 (1.64)	7.28 (1.17)	7.22 (1.54)	6.57 (1.21)	6.02 (1.72)
Control	6.79 (1.58)	6.92 (1.51)	5.02 (1.72)	4.10 (1.75)	3.22 (1.69)	3.63 (2.02)
Cronbach's Alpha	.84	.82	.83	.91	.96	.84

Notes:

* Original study: Lammers et al. 2009

Sample size for the replication MC: Social power (N = 92), Personal power (N = 99), Control (N = 97). Sub-sample sizes for the original study were not reported.

Hypothesis testing. We analyzed the effects of personal power, social power, and general power (control) on stereotyping and behavioral approach tendencies by following the same analytic procedure as the original study. For each dependent variable, Lammers and colleagues first performed an ANOVA to assess the overall effect of condition. Second, three contrast analyses were performed. The two primary contrasts compared the control condition to the personal power condition (Contrast 1) and the social power condition (Contrast 2), whereas the third contrast compared the effect of the personal power condition to the social power condition. Results are reported in Table 2 and Table 3, along with results from the original study, for direct comparison. Table 2 provides means and standard deviations across conditions. Confidence intervals for the mean are included for our replication, but were not reported for the original study. Table 3 gives an overview of the results from ANOVA and the three contrast analyses.

Stereotyping. Lammers and colleagues found a robust overall effect of condition ($\eta^2_p = .23$). The pattern of means as well as the results from contrast analyses showed that compared to the control condition, personal power increased stereotyping, whereas social power decreased stereotyping.

The results from our replication also indicates an overall effect of condition on stereotyping, but the estimated effect size is considerably smaller ($\eta^2_p = .056$). Contrast analyses reveal that although the difference between the personal power condition and the social power condition is statistically significant and in the expected direction, neither condition differ significantly from the control condition. Consequently, our results offer very limited support to the proposed *opposite* effects of the two forms of power, but suggest that compared to personal power, social power produces *less* stereotyping.

Behavioral approach tendencies. Lammers and colleagues found a robust overall effect of condition, and the pattern of means as well as the results from contrast analyses

supported their predictions. Compared to the control condition, both forms of power *increased* participants' behavioral approach tendencies, and the social power and personal power condition did not significantly differ.

The results from our replication do not suggest an overall effect of condition, nor any significant differences in effects between the three experimental conditions. Compared to the control condition, behavioral approach did not increase nor decrease in the personal power or social power conditions. Accordingly, our results fail to replicate the original pattern of effects.

Table 2. Means (and SDs) of dependent variables across conditions.

Condition	Stereotyping		Behavioral Approach	
	Original (N = 61)*	Replication (N = 148)	Original (N = 52)	Replication (N = 147)
Personal Power	6.07 (.47)	4.95 (0.56) CI [4.78; 5.11]	5.68 (.74)	5.80 (0.84) CI [5.56; 6.04]
Social Power	5.44 (.49)	4.53 (.80) CI [4.30; 4.75]	5.79 (.74)	6.07 (0.97) CI [5.79; 6.35]
Control	5.74 (.47)	4.78 (0.77) CI [4.56; 5.01]	5.11 (.87)	5.98 (0.76) CI [5.76; 6.19]

Notes:

* Original study: Lammers et al. 2009

95 % CIs for the mean are reported for the replication, but were not reported for the original study.

Replication sub-samples:

Personal power/stereotyping (N = 50); Social power/stereotyping (N = 50); Control/stereotyping (N = 48)

Personal power/approach (N = 50); Social power/approach (N = 48); Control/approach (N = 49).

Sub-samples were not reported for the original study.

Table 3. Results from ANOVA and direct contrasts between conditions.

	Stereotyping		Behavioral Approach	
	Original (N = 61)*	Replication (N = 148)	Original (N = 52)	Replication (N = 147)
Overall difference between conditions	F(2,58) = 8.68, $p = .000$ $\eta^2_p = .23$	F(2, 145) = 4.30, $p = .015$ $\eta^2_p = .056$	F(2, 49) = 3.71, $p = .03$ $\eta^2_p = .13$	F(2, 144) = 1.27, $p = .283$ $\eta^2_p = .017$
Direct contrasts:				
1) Personal power vs. control	$t(58) = 2.21, p = .030$	$t(145) = 1.11, p = .267$	$t(49) = 2.56, p = .01$	$t(144) = -1.05, p = .298$
2) Social power vs. control	$t(58) = -2.01, p = .049$	$t(145) = -1.76, p = .080$	$t(49) = 2.11, p = .04$	$t(144) = .52, p = .602$
3) Personal power vs. Social power	$t(58) = 4.17, p = .000$	$t(145) = 2.91, p = .004$	$t(49) = .41, p = .68$	$t(144) = 1.56, p = .120$

Notes: * Results reported in Lammers et al. (2009).

Discussion

The purpose of the study reported here was to replicate an experiment (Study 1) by Lammers et al. (2009), which suggests that personal power and social power may be differentiated empirically (and not only theoretically), and that the two forms of power produced opposite effects on stereotyping, but parallel effects on behavioral approach.

We were able to replicate the empirical distinction between the two power constructs. The manipulation checks provided support for the distinction between personal and social power by a clear factorial structure, high discriminative validity, and satisfactory scale reliability coefficients.

Our results also offer partial support for the originally reported effects of the two forms of power on stereotyping. Lammers and colleagues found that, compared to the control condition, personal power *increased* stereotyping whereas social power *decreased* stereotyping. Our results do not support the proposed *opposite* effects of the two forms of power, but suggest that relative to personal power social power produces *less* stereotyping. The estimated overall effect size is, however, considerably smaller than in the original study.

Our results failed to replicate the effects of the two forms of power on behavioral approach. Whereas Lammers and colleagues found support for the hypothesized parallel effects of personal and social power, we found no effects of personal power nor social power compared to the control condition.

Several potential explanations for the differences in findings between the original study and our replication should be considered. First, considering the effects of the two forms of power on stereotyping, our results indicate an overall pattern of effects that is consistent with the original study, although substantially weaker. Some scholars argue that due to regressive shrinkage, effects from replications cannot be expected to be equally strong as original effects, and the reliability of the original study as well as the replication must

therefore be taken into account when evaluating differences in effect sizes (Fiedler & Prager, 2018). Our replication was direct and considerably better powered, with a sample about 2.5 times larger than the original sample, which suggests that the originally reported effects on stereotyping may have been overestimated.

It is also possible that Norwegian students (majoring in different fields) are less likely than Dutch students (in psychology) to engage in stereotyping or behavioral approach. Norwegian participants reported lower stereotyping in the control condition, which may offer some support to this idea. Yet, they did not respond to the two forms of power by significantly increasing or decreasing stereotyping, which was the main idea to be tested here. Compared to the Dutch sample, the Norwegian students also reported slightly higher (not lower) approach tendencies across all conditions. Of course, we cannot entirely dismiss the possibility that there were subtle differences in the material due to translation or that the Norwegian participants generally responded in a less careful manner than the Dutch participants. As noted previously, however, we performed the replication as closely to the original as possible, and all participants were supervised while taking part in the experiment. We do speculate, however, that psychology students may be prone to more hypothesis guessing and hence biased responses due to prior knowledge of experimental research designs and priming methods in psychological experiments.

We also speculate that the reliability of power primes and manipulation checks employed was insufficient to underpin the empirical distinction between personal and social power. First, considering the manipulation checks, inspection of the materials used (in the original study as well as the replication) reveals that the items referring to each of the three conditions were visually separated by a horizontal line, making the three categories of items highly discernable. Participants in the personal power and social power conditions could therefore easily detect which category of items that better matched their initial priming

instructions. This implies a serious threat to the reliability of the manipulation checks and, consequently, to the validity of inferring an empirical distinction between personal and social power based on the manipulation check responses.

Another and related concern regards the manipulation check results in the control condition. In the original study as well as in the replication, participants in the control condition not only reported considerably higher personal power and social power compared to general power, but also relatively high levels of personal and social power even when compared to the two power conditions. These patterns of results not only prompt concerns about the reliability of the control manipulation, but also represents a potential explanation for the lack of significant differences between the two power conditions and the control condition, particularly when tested in a better powered replication as the one reported here. We therefore performed additional, exploratory analyses based on actual manipulation check scores. Since manipulation checks were designed to measure feelings associated with each form of power, these feelings could be expected to be associated with scores on stereotyping and approach tendencies. We therefore performed regression analysis in which actual manipulation check scores relating to personal power, social power, and general power were regressed onto stereotyping scores and approach scores, respectively. However, results revealed no significant relationships between manipulation check scores and the dependent variables. We also performed the same tests within condition, i.e., by splitting the data file into conditions, to examine whether there was an association between participants' manipulation check scores for their relevant condition and their responses to the measures of the dependent variables. Again, results revealed no significant relationships between personal power and social power manipulation check scores and the two dependent variables. However, in the control condition results reveal a significant and positive relationship between participants' score on the general power manipulation check and stereotyping ($F(1,$

46 = 6.655), $\beta = .356$, $R^2 = .126$, $p = .013$). Results also reveal a positive relationship between participants' score on the general power manipulation check and approach tendencies, although the relationship is only significant when tested one-tailed ($F(1, 46) = 3.415$), $\beta = .260$, $R^2 = .068$, $p = .071$). The latter findings suggest that participants' sense of general power affects stereotyping as well as approach tendencies, but only when participants were in the control condition, i.e., not exposed to any of the two power primes. These effects were not detectable when using the original analytic procedure, in which the control condition was treated as a single base-line to which the two forms of power were compared. The original experimental design involved one-sided manipulations (high only) of the two power conditions, as opposed to two-sided (high-low), which may have yielded more nuanced and perhaps different results (Schaerer, du Plessis, Yap, & Thau, 2018).

The results from our additional analyses based on the manipulation checks scores indicate that the measures of the dependent variables were adequately designed to capture potential effects on stereotyping and approach, and that participants in the replication responded in a sufficiently careful manner. Accordingly, one possible explanation for the lack of results revealing effects of personal or social power on stereotyping or approach in the present and better powered replication, is that the particular power primes used were inadequate, as indicated above. More precisely, it seems plausible that the manipulation check results were unreliable for the two main conditions, despite appearing to support the empirical distinction between personal and social power. It is, however, also possible that the effects of the two forms of power do not follow the pattern suggested by the original study or that the effect sizes are diminishing or even absent when tested in a better powered replication. A single replication such as the study reported here cannot, however, be interpreted as conclusive evidence for either of these possibilities.

In order to establish a more robust empirical distinction between personal and social power and to put the pattern of effects proposed by Lammers and colleagues better to test, more rigorous research is called for. We hope that our replication inspires further replications across samples and settings and with different types of power manipulations as well as other dependent variables that tap into the interdependent-independent dimension, which according to Lammers and colleagues represents the underlying mechanism of the expected differences in effects of the two forms of power.

Conclusion

We performed a direct and better powered replication of the experiment (Study 1) reported by Lammers et al. (2009). The original results offer support to an empirical distinction between personal and social power, and that the two forms of power have opposite effects on stereotyping, but parallel effects on behavioral approach. Our results offer limited support to the originally reported *opposite* effects on stereotyping, but indicate that compared to personal power, social power produces *less* stereotyping, although the effect size is small. We found no support for the previously reported parallel effects of personal and social power on behavioral approach. Neither personal power nor social power elicited an increase in behavioral approach compared to the control condition. Our additional, exploratory analyses suggested, however, that general power (control) was associated with increased stereotyping as well as increased behavioral approach. We discuss potential explanations, including statistical power, sampling, measures of the dependent variables, the feasibility of the primes used, and the validity of the manipulation checks. Hopefully, our replication inspires further inquiries into the effects of power in general and the distinction and respective effects of personal versus social power in particular.

Statement of completeness

The replication reported here is the only replication of the original study that has been performed by the authors. All independent and dependent variables included in the replication attempt are reported, and full copies of the experimental material are included in appendices/supplementary material.

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