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Control over the Association of Power and Size

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Abstract

The hypothesis that power is mentally represented as size is tested. Using an interference paradigm, two studies show that judgments of the power of groups are influenced by the font size the group labels are written in. Power judgments were slower and less accurate when the font size did not fit the power of the groups. Informing participants about the possible influence of size and its direction decreased the effect on errors (Study 1). A high likelihood of incompatible trials and information about it decreased effects on both errors and response latencies given sufficient practice (Study 2). The results suggest that the mental representation of power is associated with size cues, but that this influence can be overcome with information and training.

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Control Over the Association of Power and Size

Many social relations humans have are based on *rankings*. In social hierarchies, individuals or groups are ranked in terms of precedence, authority, prestige, status, etc. Interestingly, social hierarchic relations are often constituted, confirmed, communicated and talked about with reference to symbolic representations of spatial dimensions such as size and height, even if physical size of the ranked individuals or groups is not a determinant of the hierarchy. Anthropological evidence confirms that this is true across virtually all cultures, presumably because space is the ideal medium for establishing hierarchies as it affords the necessary asymmetric and transitional ranking (A. P. Fiske, 1992, 2004).

Because asymmetrical distributions of social power are typically inherent in those authority ranking relations, we argue that this results in the inevitable confusion of social power with physical dimensions such as size. This would indeed be a confusion, because the core of social power, the ability to control others' outcomes (S. T. Fiske & Berdahl, 2007; Keltner, Gruenfeld, & Anderson, 2003), is in the social realm by no means identical to physical size. Such confusion could lead to serious consequences for biased perceivers, for instance when they underestimate the power of a short boss. The confusion can also have detrimental consequences for targets of perceptions, for instance when social groups that are less tall are subject to biases because their potential for high status or leadership roles is first underestimated and then diminished trough confirmation processes (e.g., Jussim, Eccles, & Madon, 1996). Such biases can for instance affect women due to the genetically caused height differences between men and women, and ethnic minorities with poorer medical care and nutrition that lead to shorter height. It is thus important to study whether the confusion of size with power can be overcome. In the following studies, we present evidence that irrelevant size cues are indeed automatically interpreted as social power, but that this association can be controlled with sufficient knowledge and practice.

Size Embodies Power

Size is a key dimension of spatial mappings of ranks, in addition to vertical position, bodily strength, and horizontal order (Schwartz, Tesser, & Powell, 1982). Actual size differences between powerful and powerless groups may be at the root of this. Already in animals, size is an important cue to power, and animals of some smaller species are able to increase their apparent size to deter predators (Freedman, 1979). Size differences constitute real power differences throughout childhood and youth. Even in adulthood size often determines power, for instance in sports and coercive acts (Felson, 2002). Bodily size correlates to a certain degree objectively with power and social status precisely because power is more often attributed and given to taller individuals (for an overview, see Judge & Cable, 2004). Similarly, the gender differences in size go along with prevailing gender differences in power.

These objective size differences are accompanied by created size differences between the powerful and the powerless. First, there is language, which links power and size in many metaphors. In addition, according to A. P. Fiske (2004), size is universally used in the constitution, maintenance and challenge of power relationships through communication, evident in the many cultural practices in which power is externally manifested by size, for instance in language, architecture, dress, posture, and numbers (A. P. Fiske, 2004; Hewes, 1955). Finally, people even constitute power as size in their imagination, by overestimating how tall influential people are (Higham & Carment, 1992).

Together, objective differences, language, and communication create an ecology in which power and size are correlated. Based on this, we assume that humans also develop a strong mental association between size cues and power. Such an association of power and size would be in accord with recent theories of embodied conceptual knowledge, which argue that many, if not all conceptual knowledge is based on modal mental representations (Barsalou, in press; Barsalou, 1999; Fischer & Zwaan, in press; Glenberg, 1997; Niedenthal, Barsalou, Winkielman, Kraut-Gruber, & Ric, 2005; Niedenthal, 2007). In his Perceptual Symbol Systems theory, Barsalou (1999) proposed that concepts are developed by schematizing modal perceptual input. Thinking uses the resulting perceptual symbols by reactivating the identical structures that were involved in the perceptual process, and thereby creates simulations by re-enacting perception. Abstract concepts are then assumed to be grounded in the somatosensory experiences on which their learning was based.

One hypothesis that can directly be derived from this framework is that conceptual knowledge is associated with modal content, and that the activation of the concept involves activation of the modal content. Embodied approaches recognize that this idea has a long tradition in social cognition research (Niedenthal et al., 2005). For instance, research on social stereotypes has shown that an unobtrusive activation of the elderly stereotype leads to slower walking (Bargh, Chen, & Burrows, 1996). The reverse also holds: Unobtrusive induction of slow walking activates the stereotype of the elderly (Mussweiler, 2006). Note that these and similar findings are typically framed in, and are consistent with, network models of human memory in which modal content is associated with non-modal, abstract nodes that represent for instance the category of the elderly. However, they can be parsimoniously explained within an embodiment framework (Niedenthal et al., 2005).

In addition to the priming studies just cited, interference paradigms have proven to be valuable tools to test the modal content hypothesis. In these paradigms, modal content is presented in a Stroop-like fashion while another task has to be performed. If the modal content influences performance in the task, it can be assumed to be involved in the mental processes of the task performance. For instance, showing the embodiment of evaluation in locomotion, evaluating a stimulus is facilitated by concurrently present somatic or visual approach cues (Neumann, Förster, & Strack, 2003; Neumann & Strack, 2000a; Neumann & Strack, 2000b; Seibt, Neumann, Nussinson, & Strack, in press). Similarly, evaluating a word as positive is facilitated if it is presented up rather than down on the screen (Meier &

Robinson, 2004). Likewise, interference paradigms have been used to show that divinity is associated with elevated spatial positions (Meier, Hauser, Robinson, Kelland Friesen, & Schjeldahl, 2007).

Interference paradigms of this type were also used in several studies to show that modal information about elevation in space is directly associated with power (Schubert, 2005). When participants had to judge the power of groups represented by labels, the vertical positions of the labels interfered with the judgments: Powerful groups were judged more quickly as powerful when they appeared at the top rather than at the bottom of the screen, and the reverse was true for powerless groups. This influence of spatial screen position on power judgment held both when groups were presented in pairs and when they were presented alone.

In sum, there is good evidence that space is used to establish, communicate and confirm power relations. There is also growing evidence that mental representations of concepts include concrete modal content. The evidence on power is so far restricted to elevation or vertical differences. The first goal of the present research was therefore to test whether the representation of power is also linked to modal information about size. The following studies test this hypothesis in an interference paradigm, in which the power of groups (represented by labels) had to be judged. Font size operationalized size and was the interfering stimulus. If the size of the font is automatically interpreted as power, it will influence the judgment of the groups' power.

The Uncontrollability of Equating Size with Power

If the interference paradigm indeed shows influences of font size on power judgment, this would suggest that the size unintentionally and very quickly activates knowledge about power. However, given that social power is often unequal to size, and confounding them thus results in error, the question is: Can the equating of size and power be controlled? Is it possible to suppress or even inhibit erroneous judgments of the shorter teacher, professor, or boss as powerless based on their size?

Two types of control are imaginable. First, it seems possible to override an initial, automatic assessment of power based on size, and to form a different impression. This process is comparable to a correction process after the activation of a stereotype. Evidence on the automaticity of stereotyping suggests that influences of activated stereotypes on judgements can be controlled when knowledge about the direction of the relation, cognitive resources, and motivation are available (S. T. Fiske & Neuberg, 1990; Gilbert & Hixon, 1991; Strack & Hannover, 1996; Thompson, Roman, Moskowitz, Chaiken, & Bargh, 1994).

However, this form of control entails only that the outcome of the automatic process is corrected, but not that the process itself is prevented. Prevention of the normally automatic knowledge activation would be a second type of control. Evidence on stereotype activation suggests that this second form of control requires the perceiver to automatize the stereotype correction process itself, and to automatically inhibit the activation of knowledge (Förster & Liberman, 2007). Automatic preconscious control of knowledge activation has been shown for chronic goals, which can intervene before the activation of stereotypic knowledge, and lead to its inhibition (Glaser & Kihlstrom, 2005; Moskowitz, Gollwitzer, Wasel, & Schaal, 1999). Importantly, these results have been found using a priming paradigm with very short stimulus onset asynchronies, too short for conscious control to intervene (Bargh, 1997; Neely, 1977). Recent evidence suggests that such control can also be exerted over evaluative responses in an affective priming paradigm (Degner, 2007; Klauer & Teige-Mocigemba, 2007, see below). Similarly, Logan and Zbrodoff (1979) showed in a Stroop paradigm that participants were able to turn their knowledge about the ratio of compatible and incompatible primes into an effective strategy to enhance their performance (see below for more details).

In addition to demonstrating the association between size and power, the second goal of the following studies was thus to explore whether this association can be controlled. In

Study 1, participants were warned of a possible influence of font size on their responses and motivated to avoid this influence. Study 2 varied the ratio of compatible and incompatible trials, and participants' awareness of these ratios.

Study 1

Method

Participants

German university students were asked to participate in a study on word recognition in exchange for chocolate (a value of 1 €). Data from 81 participants were collected, but the data of 6 participants with 44 or more wrong answers in the 64 trials had to be excluded from the analyses. The highest number of errors in the remaining sample was 14. All of the remaining 75 participants were native German speakers, the mean age was 22.1 (ranging from 18 to 39, SD = 3.3), and 33 were male.

Procedure and Materials

The cover story explained that the study investigated readability of big and small fonts. Participants saw labels of 16 typically powerful and 16 typically powerless groups appearing twice on a computer screen, once in large (26 point) and once in small font (12 point), and had to decide as quickly and as accurately as possible whether the group was powerful or powerless. Pretests assured that the groups were almost unanimously judged as powerful or powerless. The labels were presented in black on a white background, always centered vertically and horizontally on the screen.

The experiment was programmed in DMDX (Forster & Forster, 2003). Assignment of keys (left and right cursor key) to the powerful or powerless answer was balanced between participants. The motivation to control the size influence was manipulated by telling one half of the participants that in previous studies, research had found that font size could influence the power judgments such that a large font could lead to erroneous perception of groups as powerful and a small font size to erroneous perception of groups as less powerful. Participants were urged to avoid this influence on the pretext that it would distort our results. The other half of the participants were told nothing about a possible influence of font size on power judgments. A funnelled debriefing at the end revealed that none of the latter group suspected any such influence.

Results

Response Latencies

Of the total of answers, 4.2% were wrong. All latencies (2.1%) more than 3 *SD*s longer than the grand mean were excluded (Bargh & Chartrand, 2000). Mean response latencies for each of the four combinations of the group power x font size design were computed by averaging the respective latencies of correct responses.

A 2 (group power, within subjects) x 2 (font size, within subjects) x 2 (instruction, between subjects) GLM[1] on the response latencies showed a significant main effect for group power, $F(1,73) = 21.07$, $p < .001$, $\eta_p^2 = .22$, indicating that judgments of a group as powerless took longer than judgments of a group as powerful. In line with our hypothesis, this main effect was qualified by font size, $F(1,73) = 19.43$, $p < .001$, $\eta_p^2 = .21$. No other effects reached significance. Table 1 shows that it took less time to judge a powerful group as powerful when it was written in larger font than when it was written in smaller font. This difference was significant, $t(74) = 3.68$, $p < .001$. The reverse was true for judgments of powerless groups, $t(74) = 2.46$, $p = .016$. Instruction did not qualify the group status x font size interaction, $F < .10$, indicating that participants were not able to control the interference of font size on the latency of their power judgments.

Error Frequencies

Error frequencies for each of the four cells were computed by summing the number of errors (not counting omitted and too long answers), and computing percentages relative to the total of 16 stimuli in each condition.

The above GLM was then repeated for error frequencies. The group power x font size interaction was significant, $F(1,73) = 12.17$, $p = .001$, $\eta_p^2 = .14$. Importantly, this interaction was qualified by instruction, resulting in a three-way interaction, $F(1,73) = 4.14$, $p = .045$, $\eta_p^2 = .05$. No other effect reached significance. To explore the three-way interaction, we computed separate 2 (group power) x 2 (font size) GLMs for each instruction condition. The group power x font size interaction was significant when no instruction was given, $F(1,36) = 12.57$, $p = .001$, $\eta_p^2 = .26$, mirroring the pattern of the response latencies. Table 2 shows that more errors were made in the judgment of powerful groups when they appeared in a small font size than when they appeared in a large font size, $t(36) = 2.74$, $p = .009$. More errors were made in the judgment of powerless groups when they appeared in a large font size than when they appeared in a small font size, $t(36) = 2.83$, $p = .008$. However, when participants received the instruction to avoid an influence of the font size on their judgments, the group power x font size interaction was no longer significant, $F(1,37) = 1.33$, $p = .257$, $\eta_p^2 = .04$.[2]

Correlational Analyses

To explore possible speed-accuracy tradeoffs, we computed overall means of response latencies and errors, and effect scores summarizing the font size x power interaction (subtracting the sum of compatible trials from the sum of incompatible trials). Mean number of errors and mean response time did not correlate significantly when no instruction was given, $r = -.19$, $p = .252$, and neither when a correction instruction was given, $r = .11$, $p = .496$. Interestingly, although there was no main effect of instruction on response times, there was a marginal negative correlation between mean response time and effect score for errors, $r = -.29$, $p = .073$ for participants with correction instruction, but not for those without, $r = -.05$, $p = .770$. The longer the mean response latency, the less participants with a correction instruction were biased by font size in their judgment outcomes. In addition, only for participants without an instruction, mean effect on errors also correlated with mean number of errors, $r = .33$, $p = .044$, but not for participants with a correction instruction, $r = -.15$, $p = .365$. The more errors participants without a correction instruction made, the more bias on errors they showed.

Discussion

The study tested whether the perceptual feature size, operationalized by font size, influenced judgements of power. When participants were not forewarned of a possible influence of font size on judgments, we found that both error frequencies and response latencies for judgments of groups as powerful or powerless were influenced as predicted: Judgments were quicker and more accurate when font size fitted the canonical power of groups, as compared to presentations where font size did not fit.

Interestingly, however, results for response latencies and accuracy diverged when participants were instructed about the possible influence of font size. They were able to avoid this influence only with respect to errors: When instructed to do so, they indeed judged less often powerful groups as powerless or powerless groups as powerful when font size did not fit. However, it was still the case that judgments were slower when font size did not fit canonical power.

The reduced effect on errors was not due to overall slower response latencies, but the marginal negative correlation between overall response latency and the effect on errors could hint to the possibility that the ability to reduce the bias is achieved by slowing down answers. However, this needs to be replicated in Study 2 first.

One shortcoming of Study 1 may be the fact that the cover story mentioned the presence of two different font sizes. While a plausible reason was provided that diverted attention away from any association with power (i.e., the investigation of readability), this might have led to spontaneous categorizations of target words into small and large. It is possible that such a spontaneous categorization and the thereby activated category labels indirectly primed the concepts of power or powerlessness. We averted this potential confound in Study 2 by not drawing attention to the two different font sizes.

Study 2

The goal of Study 2 was to further explore the conditions under which a perceptual interference effect can be controlled. Control of interference or sequential priming effects has been a recurrent topic since the seminal study by Neely (1977), who concluded that it is impossible to control answers when the stimulus onset asynchrony is just 300 ms. However, contradicting evidence now abounds. Logan and Zbrodoff (1979) showed that Stroop interference was as usual (i.e., compatible trials were faster) when a low or intermediate percentage of trials were incompatible (20% or 40%), but that Stroop effects were zero when 60% of trials were incompatible and even reversed (i.e., incompatible trials were faster) when a high percentage (80%) of trials were incompatible. Notably, participants were told before about the percentage they could expect. Apparently, participants were able to use this knowledge of the distribution by forming and implementing an adaptive strategic use of cues even in a Stroop paradigm. This strategy was not based on simply ignoring the dimension on which the answer had to be based, as shown by low overall error frequencies.

Klauer and Teige-Mocigemba (2007) showed in an affective priming paradigm that a biased distribution of incompatible and compatible trials alone was not enough to reverse the priming effect. In a paradigm with two positive and two negative primes, one positive and one negative prime were most likely (in two thirds of the trials) followed by a target consistent in valence. The other two primes were most likely (again in two thirds) followed by a target inconsistent in valence. Some participants were informed about these contingencies, others not. The SOA was very short (275 ms), and participants had only a 800 ms reponse window. Those who were informed about the contingencies were indeed able to reverse the evaluative priming effect for the primes with the inconsistent contingency. It should be noted that participants in this study answered 5 blocks of 48 trials and thus had plenty of practice in prime-contingent answering. The same is true for the studies reported recently by Degner (2007), who showed that participants informed about the nature of an affective priming were able to reduce their priming effects.

In Study 2, we replicated Study 1 building upon these ideas by manipulating both the frequency of different types of trials, and participants' knowledge about this bias in the distribution. In particular, there were three different conditions. A first condition resembled Study 1 in that the number of compatible trials (powerful group labels in big font and powerless group labels in small font) was equal to the number of incompatible trials (powerful group labels in small font and powerless group labels in big font). Participants were not told anything about the distribution. In the second and the third condition, the numbers of compatible and incompatible trials were unequal. As in the study by Klauer and Teige-Mocigemba (2007), two thirds of the stimuli were incompatible regarding power and font size, and only one third was compatible. The second and the third condition differed in whether participants were alerted to this biased distribution. In the second condition, they were not informed. In the third condition, participants were explicitly told that a word in small font was likely to refer to a powerful group, and that a word written in big font size was likely to refer to a powerless group. Thus, we had three different conditions, with uninformed participants seeing an unbiased distribution, uninformed participants seeing a biased distribution, and informed participants seeing a biased distribution.[3] On the basis of Study 1, we predicted that participants who were informed about the biased distribution would be able to correct at least their answers, resulting in a lower number of errors. In order to explore the learning process, we used almost twice as much trials as in Study 1 in two blocks.

Method

Participants

Data from 155 Dutch students were collected. Five participants with 26 or more errors in the 96 trials were excluded. The highest number of errors in the remaining sample of 150 was 24. Mean age was 22 (SD = 3.7), 51 were male. Participants received 2 € for participation.

Procedure and Materials

The study was run as part of a larger set of studies, all of which were unrelated to power. Participants were first introduced to a task on "complex social judgments," instructed about the task of judging the power of groups, and asked to respond both as quickly and as accurately as possible. An extra bonus of 1 € was promised for exceptionally good performance, and paid to all participants. All participants went through 12 practice items with an equal number of compatible and incompatible trials. After the practice trials, only informed participants received further detailed instructions. They were told that it was likely that the font size influenced their judgments, and that it was therefore important to know that a word written in big font size most likely referred to a powerless group, and that a word written in small font size most likely referred to a powerful group. Font size was not mentioned in the instructions to the other (uninformed) participants.

Stimuli were randomly drawn from a list of 100 Dutch powerful and powerless group labels. Each of the two blocks contained 48 trials. Blocks with unbiased distributions contained 24 compatible and 24 incompatible trials (12 trials of each powerful groups in big font, powerful groups in small font, powerless groups in small font, and powerless groups in big font). Blocks with biased distributions consisted of 16 compatible trials (8 powerful groups in big font, 8 powerless groups in small font) and 32 incompatible trials (16 powerful groups in small font, 16 powerless groups in big font).

All participants indicated a powerful group with the right and a powerless group with the left response key. Details and timing of stimulus presentation were identical to Study 1. The study was programmed in E-Prime.

Results

Response Latencies

Initial preparation of the data followed Study 1. Response times longer than 3 SDs above the Grand Mean were discarded from the analyses. Latencies of correct answers were averaged separately for trials with powerful and powerless targets with big and small font, and the two blocks. Table 3 present the means.

Averaged response times were first submitted to a 3 (distribution) x 2 (group power) x 2 (font size) x 2 (block) GLM with repeated measures on the last 3 factors. The basic power x font size interaction was not significant, $F(1,147) = 1.13$, $p = .289$, $\eta_p^2 = .01$. Block, power, and font size all produced significant main effects, *F*s > 20, *p*s < .001, η_p^2s > .13. Responses were faster for powerful than for powerless groups, for small than for big font, and in the second compared to the first block. There were also significant yet theoretically less interesting two-way interactions of power and condition, $F(2,147) = 4.29$, $p = .015$, $\eta_p^2 = .06$, and power and block, $F(1,147) = 6.64$, $p = .011$, $\eta_p^2 = .04$.

Of more importance are the two significant three-way interactions of power, font size, and condition, $F(1,147) = 3.23$, $p = .042$, $\eta_p^2 = .04$, and of power, font size, and block, $F(1,147) = 5.74$, $p = .018$, $\eta_p^2 = .04$. Moreover, these were qualified by a marginal four-way interaction of power, font size, block, and distribution, $F(1,147) = 2.62$, $p = .077$, $\eta_p^2 = .034$, indicating that the different distribution conditions moderated the basic interference effect, and that this influence depended further on the block.

To ease the interpretation of this four-way interaction, we computed indices of the power x font size interaction effect. To do so, we summed average response times for the incompatible trials (powerful in small, powerless in big font), and subtracted the average response times for compatible trials (powerful in big, powerless in small), separately for first and second block. Large scores indicate that compatibility of font size and power decreases latencies.

These indices were then submitted to 3 (distribution) x 2 (block) GLM with repeated measures on the second factor. The two-way interaction replicates the four-way interaction of the preceding analysis. The upper panel of Figure 2 shows estimated means and standard errors. It is clear from the graph that in the first block, there are no large differences between the three conditions, $F < 1$. Both the effect in the uninformed unbiased condition, $t(41) = 1.55$, $p = .128$, and in the effect in the uninformed biased condition, $t(54) = 1.53$, $p = .131$ failed to reach significance, and the effect in the informed biased condition was far from significance, $t < 1$. Differences only emerged in the second block, as a significant effect of condition there showed, $F(1,47) = 5.49$, $p = .005$, $\eta_p^2 = .07$. In the second block, participants informed about their biased distribution differed significantly from the other two conditions, $t(93) = 2.76$, $p = .007$ for the difference to the uninformed unbiased condition, and $t(106) = 2.79$, $p = .006$ for the difference to the uninformed biased condition. Furthermore, only participants informed about their biased distribution showed changes from first to second block, $t(52) = 3.04$, $p = .004$, both other *t*s < *1*. In fact, these participants showed a reversed compatibility effect in the second block, $t = 2.81$, $p = .007$, while the other two conditions did not show significant effects in the second block either, $t(41) = 1.11$, $p = .272$, and $t(54) = 1.02$, $p = .312$, for the uninformed unbiased and uninformed biased condition, respectively.

Error Frequencies

We repeated the same analytical procedure for numbers of errors. Errors were counted separately for trials with powerful and powerless targets with big and small font, and the two blocks. To make the number of errors in the different conditions with different numbers of types of trials comparable to each other, we then computed percentages of errors for each trial type and condition. Table 4 presents the means.

A 3 (distribution) x 2 (group power) x 2 (font size) x 2 (block) GLM with repeated measures on the last 3 factors revealed a significant two-way interaction of block and condition, $F(2,147) = 4.01$, $p = .020$, $\eta_p^2 = .05$. The power x font size interaction was not significant, $F(1,147) = 1.23$, $p = .269$, $\eta_p^2 = .01$. There was however a three-way interaction of power x font size x condition, $F(2,147) = 8.43$, $p < .001$, $\eta_p^2 = .10$, which was further qualified by block in a four-way interaction, $F(2,147) = 3.67$, $p = .028$, $\eta_p^2 = .05$. Besides a theoretically less relevant interaction of power and condition, $F(2,247) = 4.52$, $p = .012$, $\eta_p^2 = .06$, no other effects reached significance.

To explore the four-way interaction further, we again computed indices for the compatibility effect, in the same way as for response latencies, and submitted them to a 3 (distribution) x 2 (block) GLM. The lower panel of Figure 2 graphs these difference scores; positive scores indicate that compatibility of font size and status interferes with accuracy. This analysis revealed that there were already differences between conditions in the first block, $F(2,147) = 3.84$, $p = .024$, $\eta_p^2 = .05$, and even larger differences in the second block, $F(2,147) = 8.80$, $p < .001$, $\eta_p^2 = .11$. In the first block, participants with an unbiased distribution had higher effect scores than both participants not informed about their biased distribution, $t(95) = 2.49$, $p = .014$, and those informed about their biased distribution, $t(93) = 2.59$, $p = .011$. Also, only participants with an unbiased distribution showed a significant effect at all, $t(41) = 3.43$, $p = .001$, but not the other two conditions, *t*s < 1.

Effect scores in the second block differed significantly from those in the first block only for participants informed about their biased distribution, $t(52) = 2.35$, $p = .023$, but not for participants uninformed about the biased distribution, $t(54) = 1.46$, $p = .15$, nor for participants with an unbiased distribution, $t < 1$. In the second block, participants with a biased distribution did not differ anymore in their effect scores from those not informed about their biased distribution, $t(95) = 1.12$, $p = .266$, but the former showed a significant effect, $t(41) = 2.30$, $p = .027$, while the latter did not, $t(54) = 1.45$, $p = .154$. Finally, participants informed about their biased distribution had significantly lower effect scores in block 2 than both participants with an unbiased distribution, $t(93) = 3.70$, $p < .001$, and participants uninformed about the biased distribution, $t(106) = 3.23$, $p = .002$. In fact, as for the response latencies, participants informed about a biased distribution showed a reversed compatibility effect, $t = 2.97$, $p = .004$.

Correlational Analyses

We again explored possible speed-accuracy tradeoffs by correlating mean response latencies and number of errors, and the effect scores for response latencies and errors, separately for blocks and condition. Correlations between mean response latencies and errors were all negative, but differed in significance levels between conditions. They were not significant for uninformed participants with an unbiased distribution, $r_{block\ 1} = -.14$, $p = .386$, and $r_{block\ 2} = -.24$, $p = .122$, but marginal for uninformed participants with a biased distribution, $r_{block\ 1} = -.25$, $p = .068$, and $r_{block\ 2} = -.23$, $p = .099$, and significant for informed participants with a biased distribution, $r_{block\ 1} = -.28$, $p = .040$, and $r_{block\ 2} = -.34$, $p = .012$. Response latency effect scores were not systematically related to any variable. Effect scores from number of errors correlated positively with overall sum of errors for uninformed participants with an unbiased instruction, $r_{block\ 1} = .34$, $p = .027$, and $r_{block\ 2} = .59$, $p < .001$. For participants uninformed about their biased distribution, this correlation was only present in the second block, $r_{block\ 1} = -.03$, $p = .825$, and $r_{block\ 2} = -.29$, $p = .033$, and for informed participants with a biased distribution, none was significant, $r_{block\ 1} = -.04$, $p = .769$, and $r_{block\ 2} = -.13$, $p = .366$. Notably, and in contrast to Study 1, there were no correlations of the error effect score with overall response latency.

Discussion

The data from Study 2 extend Study 1 by suggesting that participants who were informed about the potential influence of font size on their judgment and given incentive to avoid equating large font with high power and small font with low power were not only able to control, but produced the reversed effect of font size on their power judgments. They did so not only with regard to errors, as in Study 1, but also with regard to response latencies, but only with sufficient practice in the second block.

As both Logan and Zbrodoff (1979) and Klauer and Teige-Mocigemba (2007) discussed, there is one risk to the validity of this interpretation: Participants could choose a simpler strategy to solve the task by simply ignoring the power of a stimulus and only reacting on the basis of the font size. This strategy would thus imply errors in all or most compatible trials, and would be detectable in the data by a large increase in errors in the compatible trial types in block 2 of the informed biased condition. However, none of the participants in the informed biased condition committed more than 6 errors in the 16 compatible trials in the second block, which rules out that participants simply ignored the content of the words.

We would like to highlight three aspects of the data. First, the compatibility effects were stronger on accuracy than on response latencies. The most likely reason for this is that stimuli in Study 2 were drawn from a large list, instead of coming from a small list of stimuli that all were presented twice, as in Study 1. This probably introduced more error variance, leading to a less significant effect.

Second, participants who were told about the biased distribution reversed the interference effect not immediately, but only in the second block. This suggests a learning process during the first block that involves the automatization of an intention to expect a power judgment opposing the size cue. This assumption is line with the fact that we did not again find a negative correlation between effects on errors and overall response latency, as in Study 1, and there was also no general increase in response latency due to condition. Thus, it is unlikely that participants just slowed down to adapt to the biased distribution. In sum, these participants seem to have acquired an association of powerful and small, and powerless and big. Notably, this reversed bias is still a bias: These participants overcorrected in the adjustment, and now had a too strong tendency to judge small targets as powerful and big targets as powerless.

Third, being exposed to the biased distribution alone, without explicit information about it, did not lead to the same changes in performance compared to being informed about the biased distribution. However, these participants did not equal those with an unbiased distribution either. Most notably, they showed no bias on accuracy either in the first or second block. The process underlying this reduced interference effect is unclear. It might be that some, but only some, of the participants noticed that the distribution was biased, and formed an intention to react appropriately. Another possibility is that simply experiencing this increased number of incompatible trials resulted in an implicit learning process that muted the effect of compatibility (Betsch, Plessner, Schwieren, & Gütig, 2001; Goschke & Bolte, 2007; Reber, 1989). Whether such an implicit learning can indeed lead to decreased interference effects remains a task for future research.

General Discussion

Our findings confirmed the hypothesis that visual information about size can interfere with judgments about the social dimension of power. This effect supports both A. P. Fiske's (2004) argument that power is partly constituted by spatial dimensions, and Perceptual Symbols System theory (Barsalou, 1999), which argues that abstract concepts such as power are mentally represented by modal content. These results are also in line with previous research showing that power includes modal content related to elevation (Giessner & Schubert, 2007; Schubert, 2005; Schubert, Waldzus, & Seibt, in press). In fact, elevation may simply be a surrogate of size, which seems to be the more important dimension because it directly translates into physical power. However, an orthogonal comparison of the influences of size and elevation remains a task for future research.

In addition, the findings go beyond previous data by showing that control and even reversal of the interference is possible given sufficient knowledge and practice. While equating size with power is the default expectation, Study 1 showed decreased interference effects on accuracy when participants were alerted to the possible influence and asked to avoid it. Similarly, Study 2 showed decreased interference effects on accuracy already in the first block of trials (roughly equivalent to the number of trials in Study 1) when participants were warned about a biased distribution, compared to those who experienced an unbiased distribution and were not warned. In addition, Study 2 revealed that such a warning can result in reversed interference effects on both errors and response latencies with practice (i.e., in the second block). Accuracy thus seems to be controllable before speed. This suggests that the failure to find successful control in the instructed condition of Study 1 may have been due to too little time to practice. Just experiencing a biased distribution simply removed the interference effects in both blocks of Study 2 in comparison to participants who experienced the unbiased distribution.

On the basis of the literature on the control of stereotype activation, we assume participants who were informed about the biased distribution automatized an intention to expect size-incompatible power, which helped to control and even reverse the default power of associating large with powerful and small with powerless. The present findings thus add to the recent findings reporting controllability of response latency-based paradigms, such as the affective priming paradigm (Degner, 2007; Klauer & Teige-Mocigemba, 2007). It is worth reiterating, however, that this reversed effect is not an index of successful control, but of a control gone too far. These participants were expecting too strongly that small targets were powerful, and large targets powerless. They overcorrected their expectation, instead of adjusting it to the actual ratio of compatible and incompatible trials. This resulted in the reverse bias.

We also explored correlations between response latencies and errors, and the effects on both. However, there was no clear pattern predicting the effects on latencies or errors.

Caveats

One potential problem of interference studies like the current experiments is that they cannot rule out a mediation of the effects by the following process: The modal stimulus could be categorized, and this categorization could then semantically prime the relevant answer. Our cover story in Study 1 might have fostered such a categorization; we avoided this problem in Study 2. We have to note that in principle, it cannot be ruled out that categorizations of the font size mediated their effect on the power judgment, even though it appears unlikely given the short response latencies. In fact, this is a problem that basically all interference paradigms testing the influence of modal content face. One way to address this problem in future research may be to work with load manipulations that reduce the likelihood of a covert (subverbal) categorization of the modal interfering stimulus (de Houwer, 2003). Another possibility might be a comparison of actual semantic priming with the influence of size cues.

The problem might seem to apply especially to the current studies because size is inherently comparative: The big font size in our studies was big in comparison to the small font size in the other condition, in comparison to the font size of the instructions, and in comparison to the usual font size read on computer monitors. Other interference manipulations, for instance performing approach vs. avoidance movements during the categorization of positive vs. negative targets (Neumann & Strack, 2000b), depend less on context stimuli. However, we would like to argue that comparison of sizes does not necessarily require a judgment of the targets into small and large. Take as an example the Ebbinghaus illusion, in which a circle surrounded by larger circles appears smaller than the physically identical circle surrounded by smaller circles. Current models of the Ebbinghaus illusion do not assume that the experience of the illusion requires a preceding judgment of the central circle as smaller or larger than the surrounding circles (Roberts, Harris, & Yates, 2005). Instead, object size seems to be coded preattentively (Treisman & Gormican, 1988).

Another potential concern is a confound of power with valence. Could it be that size equals power because both are positive? Polynyms like the English "great", which denotes both powerful and good, seem to suggest this. However, empirical evidence contradicts this possibility. Schubert (2005) compared the effect of elevation on both power judgments and evaluations of positive powerful groups, negative powerful groups, and negative powerless groups. Results showed that only the judged dimension was mapped onto elevation: Elevation facilitated either powerful targets when power was judged, or positive targets when valence was judged, but not both positivity or high power in the same task. Corroborating evidence was recently presented by van Dantzig, Boot, Pecher, Giessner, and Schubert (2007). In their studies, power, but not valence of a stimulus predicted attentional shifts towards upper vs. lower regions of space after power of the stimuli was judged. Likewise, Meier and Robinson (2004) reported that valence of a stimulus predicted the same attentional shift when valence was judged. It thus seems unlikely that the power effects in the current studies are actually due to valence.

Implications

The processes indicated in our results may have interesting implications for the social regulation of power relations. One interesting implication could be that understanding power as size may underlie the tendency to ignore the relativity of power. Strictly speaking, power is a property of the relation between two interacting parties. Size however is a physical attribute of objects and not relative, but merely comparative: Something is large in comparison to something else, but not only in relation to that object. However, if power is cognitively somehow inseparable from such physical characteristics as height or size, the relational aspect of power might easily get lost. Power might be attributed to one party only, in the same way as physical characteristics are attributed to objects. This can be the basis for the unjustified generalization of power attributions across relations. As a result, a group that is powerful in relation to another group might be wrongly judged as being powerful in general.

Another implication may be that because of the equivalence of power and size, size may become a surrogate and substitute of power. For an informed observer, however, such a substitution can be obvious and the resulting suggested power attribution may be prevented. However, such a learning process needs practice and motivation, and may actually lead to overcorrection and a reversed and equally inappropriate bias. Given these hurdles to successful control, the establishment of size differences may actually often work as a means to create power differences. In the same way, subtle interventions on the size dimension may work as subversive tactics to undermine established power relations.

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Footnotes

[1] As in similar studies reported in Schubert (2005), findings were not influenced by key assignment. This factor was therefore dropped from further analyses and the design of Study 2.

[2] After the study, we were alerted to the fact that four targets (*USA, Lithunia, apprentice, children*) may have confounded power with physical size. In additional analyses excluding these targets, all effects were found to have the same significance level except for the three-way interaction font size x power x instruction, which was not significant any more, $F(1,73)=2.33$, $p=.131$, $\eta_p^2=.03$. However, this drop is due to the size x power interaction becoming weaker in the no instruction condition, $F(1,36)=8.97$, $p=.005$, $\eta_p^2=.20$, and by it becoming *stronger* in the instruction condition, $F(1,37)=1.91$, $p=.176$, $\eta_p^2=.05$. This pattern of results does not speak for the concern that the interference effect was caused by a confounding of actual size and power. Study 2 did not use such targets with objectively smaller or larger referents.

[3] We decided against including a condition in which participants saw an unbiased distribution but expected a biased one, because this would have led to additional processes based on expectancy violations that would go beyond the current interests.

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Table 1

Average Response Latencies (ms), Depending on Instruction, and Status and Font Size of the Judged Group, Study 1

		Group Status			
		Powerful		Powerless	
Condition	Font Size	*M*	*SD*	*M*	*SD*
No instruction	big	713	124	796	122
	small	745	129	778	126
Correction instruction	big	728	128	781	114
	small	760	124	762	128

Table 2

Average Error Rates (%), Depending on Instruction, and Status and Font Size of the Judged Group, Study 2

		Group Status			
		Powerful		Powerless	
Condition	Font Size	*M*	*SD*	*M*	*SD*
No instruction	big	2.87	5.63	5.41	7.24
	small	6.42	8.27	2.70	5.62
Correction instruction	big	4.11	7.59	3.45	6.28
	small	5.43	7.43	3.13	5.58

Table 3

Average Response Latencies (ms), Depending on Status and Font Size of the Judged Group, and Block and Condition, in Study 2

			Group Status			
			Powerful		Powerless	
Condition	Block	Font Size	*M*	*SD*	*M*	*SD*
Uninformed about Unbiased Distribution	1	big	715	101	762	97
		small	727	113	741	114
	2	big	699	98	718	105
		small	692	111	692	107
Uninformed about Biased Distribution	1	big	690	118	752	132
		small	674	105	713	126
	2	big	661	120	705	99
		small	658	98	685	116
Informed about Biased Distribution	1	big	708	130	740	118
		small	699	102	719	117
	2	big	696	116	682	97
		small	654	96	691	110

Table 4.

Average Error Rate (%), Depending on Status and Font Size of the Judged Group, and Block and Condition, in Study 2

			Group Status			
			Powerful		Powerless	
Condition	Block	Font Size	*M*	*SD*	*M*	*SD*
Uninformed about Unbiased Distribution	1	big	4.37	5.59	11.31	11.47
		small	7.34	7.63	6.75	6.94
	2	big	6.55	6.77	12.90	12.24
		small	9.33	12.91	8.53	9.11
Uninformed about Biased Distribution	1	big	10.45	12.45	9.20	8.41
		small	8.64	8.46	9.32	10.82
	2	big	6.36	8.96	7.84	10.71
		small	8.41	10.91	6.82	10.71
Informed about Biased Distribution	1	big	9.20	12.29	7.31	9.63
		small	8.14	8.00	6.84	9.36
	2	big	8.49	10.34	4.48	5.40
		small	7.55	7.69	11.32	11.82

Figure captions

Figure 1. Examples of the stimuli presentation with compatible combinations of size and power on the left and incompatible combinations on the right side.

Figure 2. Estimated means (+/- 1 *SE*) of facilitation of response latencies (upper panel) and of accuracy (lower panel) by compatibility of power with font size, depending on block and condition (Study 2).

Figure 1

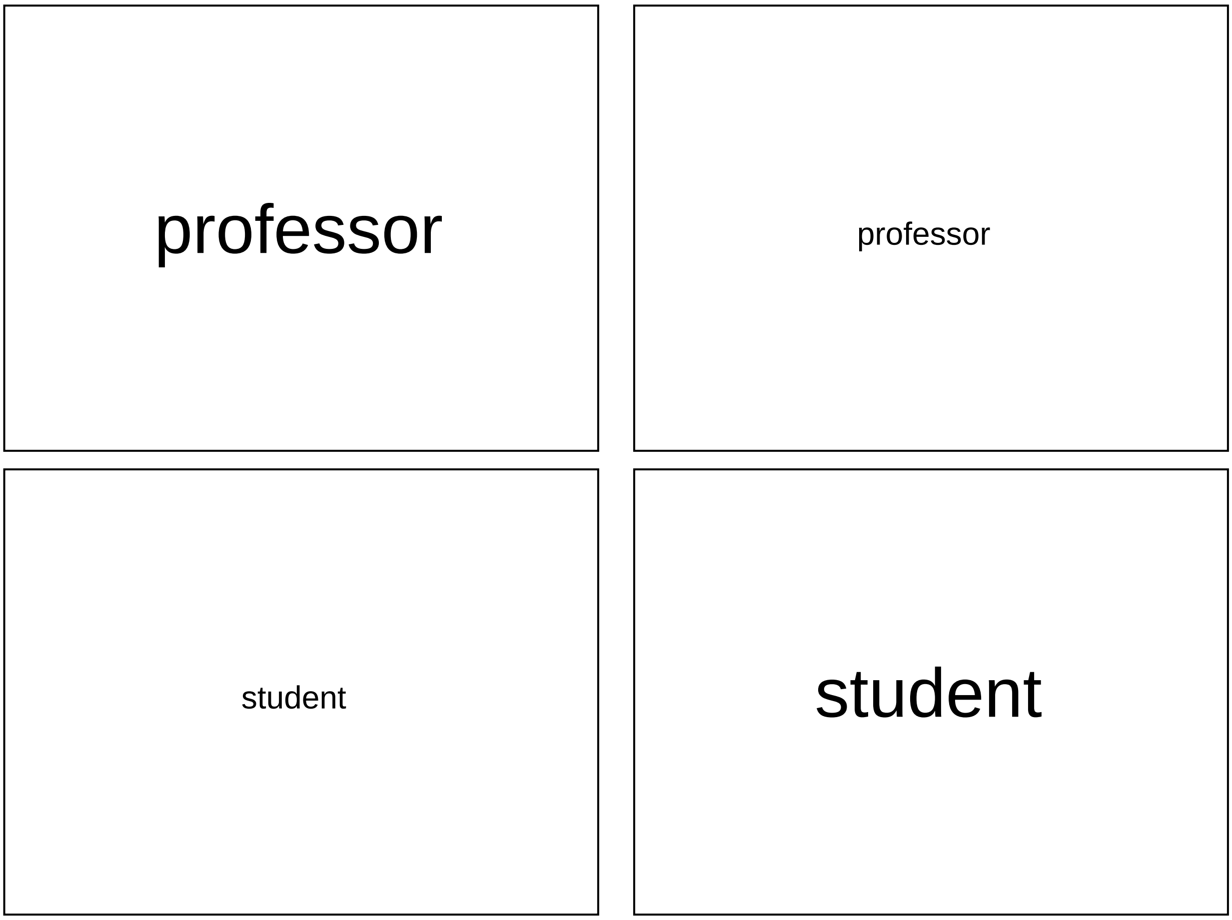
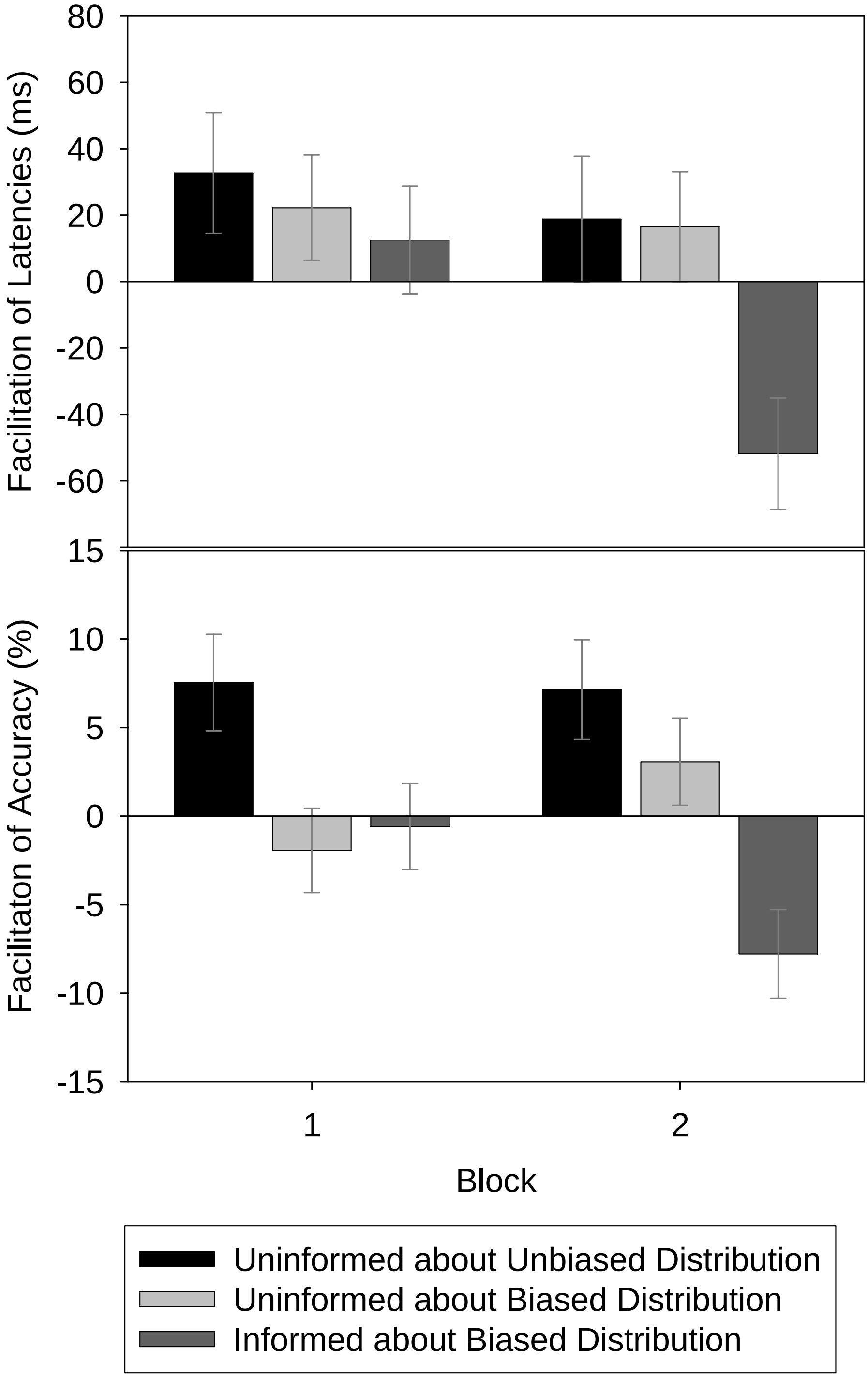


Figure 2


80
60
40
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0
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-60
Facilitation of Latencies (ms)
15
10
5
0
-5
-10
-15
Facilitaton of Accuracy (%)
1
2
Block
Uninformed about Unbiased Distribution
Uninformed about Biased Distribution
Informed about Biased Distribution