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Effects of false statements on visual perception hinge on social suggestibility.

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Abstract

Verbal hints can bias perceptual decision-making, even when the information they provide is false. What makes individuals more or less susceptible to such influences, however, remains unclear. Here, we inquire whether suggestibility to social influence, a high-level trait measured by a standard suggestibility scale, could predict changes in perceptual judgements. We asked naive participants to indicate the dominant color in a series of stimuli, after giving them a short, false verbal statement about which color would likely dominate. We found that this statement biased participants' perceptual judgements of the dominant color, as shown by a correlated shift of their discrimination performance, confidence judgments and response times. Crucially, this effect was more pronounced in participants with higher levels of susceptibility to social influence. Together, these results indicate that social suggestibility can determine how much simple (albeit false) verbal hints influence perceptual judgements.

Significance Statement

At a time when fake news soar, understanding the role that simple verbal descriptions play in how we perceive the world around us is paramount. Extensive research has shown that perception is permeable to well-orchestrated manipulation. However, even simple and straightforward statements can bias individuals' perceptual decisions. Critically, here we show that this effect varies across individuals with the susceptibility to social influence, a high-order trait measured using a self-evaluation questionnaire, reflecting the tendency to orient one's feelings and

behaviors by the feelings and behaviors of others. We argue that researchers, communicators and policy-makers should pay careful attention to the role that social suggestibility plays in how we build perceptual judgments, altering the strength of the simplest messages.

Introduction

In any instance of communication, descriptive statements can be construed as a form of informational social influence (Deutsch & Gerard, 1955). For example, we might feel inclined to misclassify one line as being longer than another one if we saw other people doing it too (Asch, 1951). During the seminal research on informational social influence, these biases were mainly attributed to an impact of social conformity on decisional processes (Asch, 1951; Asch, 1965; Moscovici & Personnaz, 1980). However, recent evidence has shown that social influence can also trigger perceptual judgment distortions through the alteration of sensory and perceptual mechanisms (Mojzisch & Krug, 2008; Germar et al., 2016). Interestingly, these findings are consistent with extensive results of similar nature reported on hypnotic and placebo suggestions, which have also been shown to affect perception in a dramatic fashion, to the point of making participants see things that are not quite there (Spiegel, 2003), preventing them from reading common words (Raz et al., 2006), and even blocking them from feeling the pain of a needle going through their skin (Simmons et al., 2014). This is in line with theoretical accounts of hypnosis and placebo as settings of social influence, where a researcher or medical professional administers suggestions and prescriptive statements to obtain a perceptual effect, therapeutic or otherwise (Lynn et al., 2008; Lynn et al., 2015).

One outstanding question is whether all individuals are equally sensitive to being biased through social influence. On the one hand, hypnosis research has paid extensive heed to understanding how inter-individual differences in hypnotizability determine the magnitude and limits of suggestion effects. However, the unique social context in which hypnotic suggestions are

delivered, with well-orchestrated manipulations, associative training, or active imagination exercises, render conclusions from their study hard to generalize to more basic contexts of social influence (Terhune et al, 2017; Lynn et al., 2019; Geuter et al., 2017). This limitation also extends to a handful of recent studies targeting the issue outside the context of hypnosis, through “active-placebo” and “non-hypnotic” suggestions, as for the most part they persisted in the use of convoluted paraphernalia to manipulate expectations (e.g. “inert” 3D glasses in Sterzer et al., 2008; or a fake “brainwave reader” in Magalhaes et al., 2013). On the other hand, mainstream informational social influence research has left the matter of inter-individual differences mostly unexplored. Despite early evidence from experimental debriefs (Asch, 1951; Asch, 1965) that only some participants ever report experiential changes after being influenced (i.e., “actually seeing” the line as longer), identifying the individual differences in how perception can be influenced across neurotypical individuals remains to be developed (Oakley et al., 2021).

Traditionally, social psychology inventories of suggestibility are built by assessing our tendency to follow instructions, empathize with others, and comply with a group (Kotov et al., 2004). In the present work, we hypothesized that this eminently social, high-order trait would allow us to predict which individuals were more amenable to experience changes at the perceptual level when faced with false verbal hints. To test this hypothesis, we measured the social suggestibility of a group of participants with the Short Susceptibility Scale (SSS; Kotov et al, 2004), a multidimensional self-administered questionnaire based on introspective judgments and attitudinal behaviors linked to various social influence contexts. These same participants were also asked to indicate the dominant color in a stimulus made of dots of two different colors combined in different proportions, and to report their confidence regarding their perceptual judgment. Our key manipulation consisted of providing participants with a written short statement about the likelihood of the dominant color for the forthcoming stimuli, once at the beginning of each block of trials. Crucially, this statement was false half of the time, as to produce a bias that would lead participants to misperceive the dominant color of the stimulus. To minimize effects of compliance

or conformity, we prescinded from advisors or confederates, and presented this statement directly along block instructions.

Our hypothesis was that our descriptive statement would induce a shift in perceptual decisions, which would be more pronounced for individuals with higher levels of suggestibility. This led us in turn to formulate several predictions. First, the false hint would bias perceptual choices relative to a neutral hint, such that participants would overestimate the color mentioned in the hint (hereafter the “target” color). This misrepresentation would be evidenced by a shift (favoring the target color) of the proportion level at which participants deemed both colors as present in equal proportion (i.e., the Point of Subjective Equality, hereafter PSE). Second, since we expected subjective task difficulty to be maximal at the PSE, we predicted that minimal confidence ratings and maximal response times would also shift, following the biased PSE. Third, and most crucially, we expected that the inter-individual variability of this bias effect would depend on susceptibility to suggestion, as measured by the SSS. Namely, of all participants, highly suggestible individuals would overestimate the target color the most.

Our results confirmed these shifts were observable in perceptual choices, confidence ratings and response times. In all three cases, the shift depended on suggestibility. Importantly, social suggestibility mediated this process despite the absence of acting advisors, normative influence or manipulation techniques. Together, these findings indicate that permeability to social influence can predict the impact of false verbal information, and suggest that perceptual judgments are formed both by sensory inputs and the verbal descriptions of these inputs.

Materials and Methods

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in this study. For further accounts on each of these procedures, see the Supplementary Methods.

Power analyses

In order to estimate statistical power, we chose a simulation-based approach, which is an advantageous technique to use with mixed-models for its flexibility (Green & MacLeod, 2016). We simulated one Point of Subjective Equality (PSE) for biased blocks, and one PSE for control blocks, for participants of different levels of susceptibility. Based on our initial hypothesis, since target color was favored by the bias (i.e. “<target color> is twice as likely to predominate over <distractor color>”, see *Verbal Hints* below), susceptible participants achieving subjective equality would require less signal than control blocks. We used the SimR package to conduct these simulations, for sample sizes ranging from 5 to 200 participants (Green & MacLeod, 2016). Alpha was set at 0.05. We considered the model $PSE \sim \text{Bias} * \text{SSS category} + (1|\text{Participant})$, and established the detection of a significant Bias x Susceptibility interaction as criterion for success. Each sample was generated, fitted and tested a total of 1000 times, to obtain a power level and the 95% confidence interval for that power level. Figure SM 1 shows power for all possible samples. Under these conditions, a sample of $n = 53$ yielded a power of 83.4% (95% CI 81, 87). For a complete account of power and sample size estimations, see Supplementary Methods - Power Analyses).

Participants.

This experiment was conducted in the Watanabe Cognitive Science Lab (Waseda University, Tokyo, Japan). Participants were recruited throughout the 2018 and 2019 Japanese fiscal year, from the University's database of volunteers. A total of 56 volunteers were recruited to take part in the experiment. Two participants failed to complete the task, leaving a total of 54 participants who completed the experiment (ages between 18 and 23, mean = 19.85 (1.6), 21 female). One participant was excluded as an outlier after task completion (final $N = 53$; see *Additional measures & exclusion criteria*). All volunteers were paid a fixed standard rate of 1000 yen. This

study received approval for participation from the institutional ethics committee of Waseda University (#2015–033).

Stimulus and task.

On each trial, participants were presented with a stimulus made of two colors, and had to indicate which of the two colors was predominant in the stimulus, as well as their confidence in this perceptual decision. The stimulus was a random-dot kinematogram composed of 100 colored dots (radius = 0.2° , lifetime of 250 ms) presented inside of a circle (radius = 5°) located at the center of the screen. Dots moved without coherence, at a speed of 0.5 deg/s. The stimulus was displayed for a total duration of 1750 ms each trial. In some blocks, the stimulus was made of blue dots and yellow dots. In other blocks, it was made of pink dots and green dots. Nine color proportions were utilized for each color set (between 40% and 60% of the total amount of dots, in steps of 2.5%). Relative luminance, calculated from RGB values as $RL = 0.2126R + 0.7152G + 0.0722B$, was set at 186 units for all colors. Background color was set at a light shade of gray throughout the entire experiment ($R=240, B=240, G=240$). Perceptual responses were collected using the arrow keys (the correspondence between color and key was randomly determined for each participant). Confidence ratings were collected by clicking on a scale ranging from 0 (“I am sure I was wrong”) to 50 (“I responded at chance”) to 100 (“I am sure I was correct”). The stimuli and tasks were programmed using Psychtoolbox 3.0.14 (Kleiner et al., 2007) in MATLAB 9.4.0 (R2018a).

Verbal hints.

Trials were presented in blocks, which were preceded by an instruction screen presenting a bias (bias condition) or no bias (control condition). For biased blocks, these instructions stated: “On the following block, there’s a 66% chance that <target color> will overcome <distractor color>; namely, it is twice as likely that <target color> dots will be more numerous than <distractor color>

dots”, where <target color> and <distractor color> were replaced by the corresponding color name. Instructions for control blocks stated: “On the following block, there’s a 50% chance that <target color> will overcome <distractor color>; namely, it is equally likely for <target color> and <distractor color> to outnumber one another”. In order to prevent potential interferences between bias and control blocks, participants received the pink-green set for the control condition and the yellow-blue set for the bias condition, or vice-versa, in a counterbalanced manner across participants. Within each set, the target and distractor colors were also counterbalanced across participants (see Figure 1).

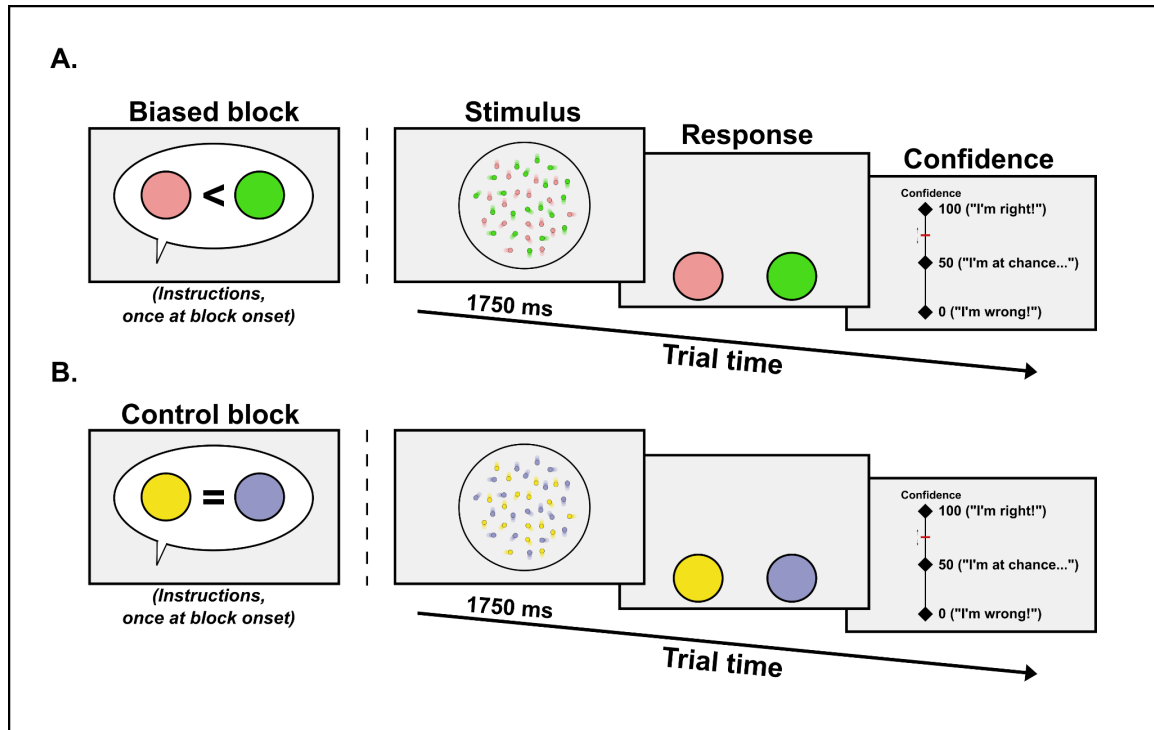


Figure 1. Discrimination paradigm. Participants were presented with a random-dot kinematogram, and then proceeded to give a perceptual discrimination response on which color predominated, followed by a confidence rating (on a scale of 0 to 100). The design was blocked, so that participants did 3 biased blocks and 3 control blocks, for a total of 90 trials per block (total 540 trials). Control and biased blocks used different color sets. A. Biased blocks. At the

beginning of the block, an instruction defining the bias for the biased blocks was given, explaining there was a 66% chance that <target color> dots would overcome <distractor color> dots. B. Control blocks. At the beginning of the block, an instruction was given explaining that there was a 50% chance that <target color> dots would overcome <distractor color> dots. After stimulus offset, participants had to indicate which color was dominant in that stimulus, and their confidence in this decision.

Procedure.

Participants were greeted by a research assistant (RA), who asked them to sign a consent form. The RA then proceeded to explain the Type 1 task (the perceptual task) and the Type 2 task (the confidence rating task). The RA insisted on the importance of reflecting upon the confidence ratings even while executing the Type 1 task, and of avoiding extreme or automatic responses that could lead to overestimation or underestimation of confidence levels (e.g. to avoid defaulting to 100% or 0% confidence responses if their actual subjective impression was nuanced). Participants were asked to respond as fast as possible to the Type 1 task while remaining precise, and “not to dwell unnecessarily” on the Type 2 task. Participants were informed that by the end of the experiment they would be asked to answer some general questions about themselves.

Each participant was then moved to an individual dim-lit experimental booth, equipped with a low-latency QWERTY keyboard, a mouse, and a chinrest that was fixed 60 centimeters away from a BENQ calibrated color screen. Before starting a practice set, the RA instructed participants to pay careful attention to the instructions that were provided to them at the beginning of each block. Participants then proceeded to do a practice block of 50 trials with a random instruction bias presented at block onset, a random color set and random color ratios. Participants were told that they could stop the training whenever they considered that they had understood the premise

of the task (all participants stopped voluntarily before reaching 10 trials). The actual experimental session started immediately afterwards.

Each participant was required to complete 6 randomly-ordered blocks (3 Bias, 3 Control) of trials. Each block presented 90 trials, with 10 trials for each of the 9 proportions of target color in a random order. All blocks began by presenting a specific set of instructions indicating the color set and the false hint about the most likely answer in the case of biased blocks.

Once all blocks were completed, participants were asked to complete Japanese translations of the Short Susceptibility Scale (SSS, excerpted from the Multidimensional Iowa Susceptibility Scale: Kotov et al., 2004) and the Ten Item Personality Inventory (TIPI, Benet-Martínez & John, 1998; John & Srivastava, 1999; Gosling, Rentfrow & Swann, 2003). These Japanese translations were translated and then back-translated, and judged as adequate by two Japanese-English bilingual researchers. Scale and item order were randomized for each participant. For each item, participants rated a statement on a scale from false/very unlikely (i.e. “1”) to true/very likely (i.e. “5” for the SSS, and “7” for the TIPI), using the numerical keypad of the keyboard. To prevent suspicions concerning the veracity of the instructional bias, the TIPI and SSS scales were always administered at the end of the task.

Testing sessions lasted between 45 and 65 minutes in total. After the testing session, participants were debriefed on the nature of the experiment. In particular, it was explained to them that task hints for biased blocks were false, and they were asked if they had been suspicious about this fact during the task (none had).

Analyses

We analyzed our data using generalized linear mixed-models (Agresti, 2002; Jaeger, 2008). To evaluate the significance of a given factor, we compared a model with and a model without this factor using a likelihood ratio test (Pinheiro & Bates, 2000; Bolker et al., 2008; see Supplementary Methods - Sections 2 & 4). Fits were performed with the lmer and glmer functions, while

convergence estimations and optimizer selection were performed with the allFit function, all from the R package lme4 (Bates et al., 2015). Once a model was selected, ANOVA tables were computed through Analysis of Deviance (Type II Wald χ^2 test) to identify significant effects and interactions, and Tukey post-hoc pairwise comparisons were performed when warranted by the results (using car and emmeans R packages; Fox & Weisberg, 2011 and Lenth, 2016, respectively; see Supplementary Methods - Section 4). We also used Akaike Information Criterion and Bayesian Information Criterion to evaluate the fit quality of the models while penalizing their complexity (Sakamoto et al., 1986). These criteria were defined as follows: $AIC = -2 \cdot \log\text{-likelihood} + 2 \cdot n_{\text{par}}$, where n_{par} equals the number of parameters in the fitted model, and $BIC = -2 \cdot \log\text{-likelihood} + \ln(n) \cdot n_{\text{par}}$, where n is the number of observations. Note that lower values indicate better models for these criteria.

Point of subjective equality.

Discrimination performance was best represented by the Point of Subjective Equality (PSE). We defined the PSE as the value of target color proportion at which the probability of selecting the target color as the predominant color was equal to 50%. Perceptual decisions were modeled using a probit regression (binomial distribution), where the probability of selecting the target color as dominant was a function of the target color proportion, the presence of bias in the block and participants' susceptibility to suggestion. Given a regression model, we obtained the PSE arithmetically for each condition using the estimated regression coefficients (see Supplementary Methods - Section 3.1). To obtain the PSEs' 95% confidence intervals for each condition, we used a bootstrap approach in which we created 1000 resampled datasets by random sampling with replacement at the trial level (for each condition in each participant), and then refitted the regression model for each of these datasets. Finally, we evaluated the 2.5% and 97.5% quantiles of the distribution of PSEs over these resampled datasets. The same approach was used to evaluate 95% CIs for contrasts between conditions.

Point of minimal confidence.

The point of minimal confidence (PMC) was defined as the value of target color proportion at which confidence levels were at their lowest, for a given condition. Confidence was expected to be maximal when target color proportion was at its highest and its lowest, and minimal towards color proportion equality (Lebreton et al., 2015; Barron et al., 2015). Thus, confidence ratings were modeled using a quadratic function (multivariate quadratic density distribution), where target color proportion and its squared value were the explanatory variables, with interactions with Bias and SSS scores (Lebreton et al., 2015; Barron et al., 2015; Maldonado Moscoso et al., 2020). As with the PSE, we used this model's coefficients to estimate the PMCs arithmetically (see Supplementary Methods - Section 3.2), and obtained their 95% CI through bootstrapping, as described above for the PSE.

Point of maximal response time.

The point of maximal response time (PMRT) was defined as the value of target color proportion at which response times were at their highest. Response times were expected to be minimal when target color proportion was at its highest and lowest, and maximal towards proportion equality (Maldonado Moscoso et al., 2020). Therefore, as with confidence, response times were modeled as a parabolic function of target color proportion (multivariate quadratic density distribution), with additional interactions with Bias and SSS scores (see Supplementary Materials - Section 3.2). As before, we used this model's coefficients to estimate the PMRTs arithmetically, and obtained their 95% CI through bootstrapping.

Social Suggestibility.

The split between low suggestibility (first quartile), medium suggestibility (50% of the data), and high suggestibility (last quartile) used in our study was inspired by previous studies analyzing

SSS data (e.g. Donovan, 2020; Winter & Braw, 2022), and other related measures such as mindfulness level (e.g. Kaviani & Hatami, 2016), or hypnotizability (Shor & Orne, 1962; Laurence & Perry, 1982; Anllo et al, 2017). The motivation of this approach is to compare the extremes of the distribution, as it is there where we expect the larger differences to emerge, while aggregating data across participants to minimize noise. Note that in the case of our sample, these group sizes also reflect the fact that some participants had the same suggestibility scores near the boundaries between quartiles, which prevented us from obtaining a fully symmetric split. Our main motivation to create these categories was to be able to obtain group-level estimates of the PSE, PMC and PMRT through fitting, to better illustrate our findings. However, to ensure that none of our effects were artificially generated by this data split, all statistical modeling concerning suggestibility was also performed using alternative models that implemented SSS scores as a continuous variable instead of as a categorical factor. These additional confirmatory analyses supported our findings at every step, and can be found in Supplementary Materials - Section 4.

Additional measures & exclusion criteria.

The reliability for SSS scores was deemed to be sufficient (21 items, Cronbach $\alpha = .84$; test-retest Pearson correlation coefficient $r = .76$ for $N = 258$, average length of retest interval 63 days as reported by Kotov et al., 2004). As an additional measure of validity, TIPI scores were utilized to confirm that SSS scores correlated with personality scores in a similar fashion as in the original SSS norms, and that they described an individual feature distinct from a basic personality trait (Benet-Martínez & John, 1998; John & Srivastava, 1999; Gosling et al., 2003). RTs, on the other hand, were used as a criterion for trial exclusion: we calculated the mean response time for the Type 1 tasks for each level of signal proportion per participant, and excluded trials where RTs were slower or faster than 3 standard deviations of each of the means for that participant. A total of 1039 trials across participants were excluded (3.6% of all trials). Additionally, one participant was excluded entirely, for presenting a SSS score higher than 3 standard deviations of the mean

(final N = 53). A complete analysis of TIPI scores, as well as comprehensive tests for the metacognitive validity of our confidence measures, can be found in Supplementary Methods - Sections 4.5 & 4.6. When performing regression analyses on individual participant's PSEs/PMCs/PMRTs, individual measures of shifts involved two confidence intervals (one for the biased and one for the neutral blocks). Thus, we took the inverse of the sum of squares of these two confidence intervals as the individual weight in these regressions (the larger the CIs, the smaller the weight for a given individual). Participants whose subjective threshold was estimated outside of the stimulus space were excluded from these regressions (n = 3 for PMC, n = 7 for PMRT).

Results

Perceptual decisions.

We anticipated that the probability of reporting one color as dominant in the stimulus would depend on 3 factors: the actual proportion of that color in the stimulus, whether or not that color was mentioned as more likely at the beginning of the block, and participants' susceptibility to suggestion. To evaluate whether suggestibility was needed to explain perceptual choices, in a generalized mixed model approach, we compared a model that included all 3 factors and their interactions with a simpler version that did not have suggestibility as a factor. The full model outperformed the simpler model ($\chi^2(4)=18$, $p= .0012$, see Supplementary Methods - Section 4.1).

To better illustrate the effect of these 3 factors on perceptual choices, we then split participants in High, Medium and Low susceptibility groups (see Figure 2.A left), and analyzed the corresponding regression model (see Supplementary Methods - Section 4.1.2). As expected, perceptual decisions about color were affected by the color proportion in the stimulus ($\chi^2(1)=1016$, $p< .001$): participants reported seeing the stimulus as "majoritary blue" more often

when the proportion of blue dots was larger in the stimulus (the same happened for all colors). Additionally, when a hint towards one color was introduced at the beginning of the block, participants' decisions were shifted towards that color ($\chi^2(1)=124$, $p < .001$). Crucially, this bias effect also interacted with suggestibility (bias x SSS category interaction: $\chi^2(2)=37$, $p < .001$), confirming our key predictions. These results were replicated by an alternative model, where SSS scores were treated as a continuous predictor instead of a categorical factor, as a measure to confirm the validity of the suggestibility effect (see Supplementary Methods - Section 4.1.2).

Point of subjective equality.

To examine this interaction more closely, we looked at the point of subjective equality (PSE), that is the value of the stimulus for which participants are indifferent between the two response options. Figure 2.A (middle column) illustrates the difference between biased and control PSEs, together with their 95% CIs, for the 3 subgroups of participants, and shows that participants in the Low suggestibility group exhibited no effect of bias ($M=0.002$, 95% CI = [-0.006, 0.003]), whereas participants in the Medium and High suggestibility group both did (Medium: $M = -0.022$ [-0.026, -0.017]; High: $M = -0.024$ [-0.029, -0.018]). To confirm this difference in the bias effect between the High and Low groups, we computed the 95% CI for that difference and found that it did not include 0 (bias difference 95% CI = [-0.03, -0.01]). For completeness, we analyzed the relation between PSE shifts and suggestibility scores in a continuous regression across participants (Figure 2 right column). To minimize the influence of noisy individual estimates in this regression, we used confidence intervals for each PSE estimate to determine the weight for each participant as the inverse of the squared sum of the confidence intervals for the biased and neutral blocks. As it can be seen from the figure, the continuous regression provided a similar pattern as our analysis based on SSS categories, although it failed to reach significance ($F(1,51) = 1.3$, $p = .2$). This does not come as a surprise, as we know from prior research that when suggestibility is used to predict other behavioral measures, it is typically used across categories of participants. In

particular, by focusing on the comparison between low and high suggestibility participants, as to maximize the difference between the two conditions. This has been done before when analyzing SSS data (e.g. Donovan, 2020;. Winter & Braw, 2022), and other related measures such as mindfulness (e.g. Kaviani & Hatami, 2016) or hypnotic suggestibility (Shor & Orne, 1967; Laurence & Perry, 1982).

Confidence ratings.

Figure 2.B (left) illustrates the confidence with which perceptual decisions are made by participants. As expected, confidence was higher when the color information in the stimulus was more extreme and thus resulted in easier choices. We thus considered a regression model where this could be captured by a quadratic effect of target color proportion on confidence. As was done for perceptual choices, we first confirmed that suggestibility should be included as a factor to account for variations in confidence, as not including it led to a significantly poorer model fit ($\chi^2(6)=35$, $p < .001$, see Supplementary Methods - Section 4.2.1).

We then examined these effects in a regression model where confidence was predicted by quadratic and linear effects of target color proportion, together with main effects of bias and suggestibility, and their interactions (see Supplementary Methods - Section 4.2.2). The quadratic effect of target color proportion was indeed significant in our data ($\chi^2(1)=536$, $p < .001$), confirming that easier choices were associated with higher confidence. We also conducted further sanity checks showing that confidence also depended on response accuracy and the interaction between difficulty and accuracy (see Supplementary Methods - Section 4.5), as typically found in the literature on metaperception (Kepecs, et al., 2008; Kepecs & Mainen, 2012).

We also found that confidence was affected by the bias ($\chi^2(1)=46$, $p < .001$), in a way that also interacted with suggestibility ($\chi^2(2)=58$, $p < .001$). This interaction corresponded to the observation that for intermediate target color proportions (target color proportion=0.5), confidence was higher for biased blocks than for control blocks in participants with High suggestibility (estimate=1.4,

SE=0.4, $p = .0028$) and Medium suggestibility (estimate: 2.5, SE=0.4, $p < .001$) but not for Low suggestibility participants (estimate: -0.4, SE=0.5, $p = .4$). As with perceptual choice, these results were replicated by an alternative model, where SSS scores were treated as a continuous predictor instead of a categorical factor, as a measure to confirm the validity of the suggestibility effect (see Supplementary Methods - Section 4.2.2).

Point of minimal confidence.

From the previous fit, we could estimate the point of minimal confidence (PMC), that is, the stimulus value for which decisions are subjectively most uncertain, for all conditions. As shown in figure 2.B (middle), this point shifted with the introduction of the biased instruction: confidence was no longer minimal when the stimulus was objectively neutral. Furthermore, this shift in PMCs was present for High suggestibility participants (mean= -0.02, 95% CI = [-0.029, -0.011]), and for Medium suggestibility participants (mean= -0.016 [-0.026, -0.001]) but not for Low suggestibility participants (mean= -0.006 [-0.014, 0.017]). The difference between Low vs High participants was significant, as we found that the 95% CI for this difference in shift did not include 0 [-0.03, -0.002]. This confirmed our prediction that participants incorporated the verbal hint about the stimulus in their confidence ratings too, and even more so when they had high scores on the suggestibility scale. As was done for the PSE, we also regressed PMC against SSS scores across participants (Figure 2, right column) and found a similar pattern of results, although here again the regression on continuous scores failed to reach significance ($F(1,49) = 3.8$, $p = .054$).

Response times.

Figure 2.B (left) illustrates response times in the perceptual task. As expected, response times changed coherently with task difficulty. As before, we considered a regression model where this could be captured by a quadratic effect of target color proportion on response times. We confirmed that suggestibility improved the prediction of response times variations, as not

including it led to a significantly poorer model fit ($\chi^2(6)=52$, $p < .001$, see Supplementary Methods - Section 4.3.1).

When analyzing the winning model, the effect of task difficulty was confirmed by quadratic and linear effects of target color proportion ($\chi^2(1)=293$, $p < .001$). Further, we observed that response times were also affected by the bias ($\chi^2(1)=46$, $p < .001$), and by the interaction between bias and suggestibility ($\chi^2(2)=28$, $p < .001$). In particular, response times were slower in biased blocks compared to control blocks for Low suggestibility participants (for target color proportion=0.5, a 32 ms effect for neutral stimuli, $SE=10$, $p = .0019$), but not for Medium (10 ms, $SE=9$, $p = .3$) or High suggestibility participants (estimate=-3 ms, $SE=10$, $p = .8$). Complete Analysis of deviance for all regressors (Type 2 Wald χ^2 test) and confirmation of these results with a continuous suggestibility score can be found in Supplementary Methods - Section 4.3.2.

Point of maximal response time.

From the fitted models, we could estimate the point of maximal response time for each condition. As shown in figure 2.B (middle), this point was shifted in biased blocks relative to control blocks. This shift of PMRT was significant in High suggestibility participants (mean= -0.03, 95% CI = [-0.044, -0.02]) and Medium suggestibility participants (mean= -0.016 [-0.029, -0.005]) but not in Low suggestibility participants (mean=-0.006 [-0.015, 0.002]). This difference in shift between Low and High participants was significant (95% CI= [-0.04, -0.009]). These results were in line with what was seen for confidence ratings: subjective choice uncertainty was affected by the verbal hint, particularly in highly suggestible individuals. As was done for the PSE and PMC, we conducted a regression of PMRT against SSS scores across participants (Figure 3, right column) and this analysis failed to reach significance $F(1,44) = 0.9$, $p = .3$).

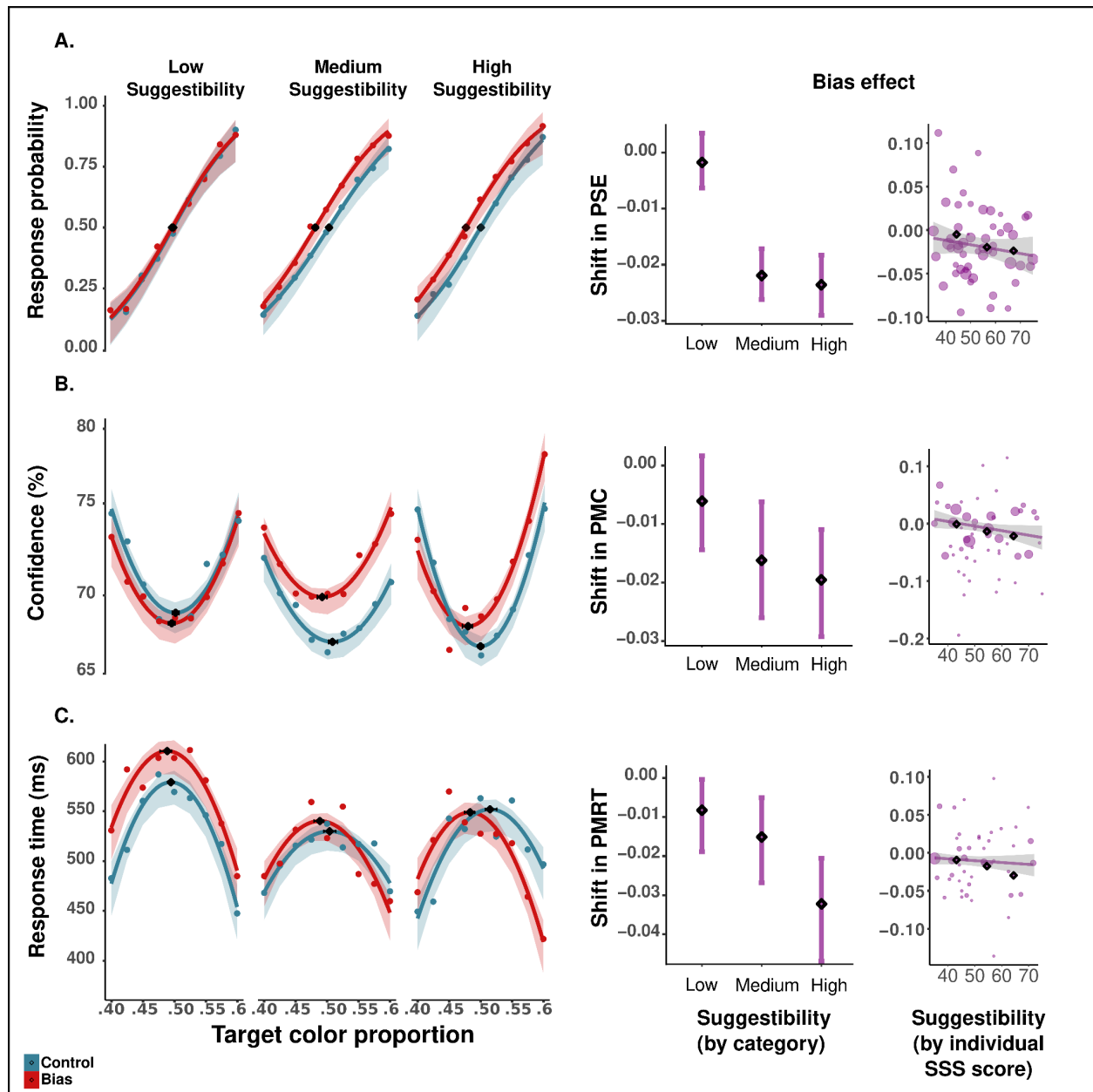


Figure 2. Perceptual choices, confidence and response times. The variations of perceptual choices (A), of confidence (B), and of response times (C), as a function of the proportion of the target color in the stimulus, separately for high, medium and low suggestibility participants, and for biased and control blocks. Shaded areas represent the 95% CI of the fit for each condition. The underscored rhomboid dots mark the point of subjective equality (PSE), the point of minimal confidence (PMC), and the point of maximal response times (PMRT) for each condition.

Horizontal error bars represent the 95% CI for these estimated values. The middle and right columns illustrate how the bias effect (i.e. the shift in PSE, PMC, and PMRT in bias blocks relative to neutral blocks) depends on suggestibility. *Middle column:* The different shifts were estimated on aggregated data across individuals within each suggestibility category. Error bars represent 95% CI. *Right column:* Shifts were estimated for each individual, and a weighted regression across individuals is shown. Since individual measures of shifts involve two confidence intervals (one for the biased and one for the neutral blocks), we took the inverse of the sum of squares of these two confidence intervals as the individual weight in these regressions (the larger the CIs, the smaller the weight for a given individual). Participants whose subjective threshold was estimated outside of the stimulus space were excluded from the regression (n = 3 for PMC, n = 7 for PMRT).

Joint shifts in perceptual decisions, confidence and response times.

Results above confirmed that the presence of a simple verbal hint coherently biased perceptual decisions, confidence ratings and response times, and that these shifts were more pronounced for highly susceptible participants. We then proceeded to conduct further analyses to confirm if confidence and response times shifts were mapped onto shifts in subjective equality. First, we tried to predict individual PMCs and PMRTs from individual PSEs. Fitting psychometric curves for confidence and response times was challenging at the individual level, as it led to estimations of subjective equality well outside the bounds of stimulus space for a small subset of participants (n=3 for the PMC, and n=7 for the PMRT). We excluded these participants from this part of the analyses. Possible reasons behind these abnormal response patterns could include the potential implementation of rogue response strategies, or unwillingness/inability to follow instructions concerning a particular aspect of the task. With the remaining participants, we conducted a weighted regression to predict individual PMCs from PSEs, using the inverse of the PMCs' 95% CI as individual weights¹ (Figure 3.A, left). Results showed that PSEs significantly predicted

PMCs ($F(1,87)= 21, p< .001$). The effect of PSEs on PMCs did not interact with bias and suggestibility, when these were included as factors in this regression: the shift in PMC was sufficiently explained by the shift in PSE, with no significant contribution from suggestibility and bias. In addition, the PSE difference between biased and control also predicted the PMC difference (Figure 4.A, right; $F(1,45)= 66, p< .001$). The same relation was confirmed between individual participants' PSEs and PMRTs, both when predicting PMRT across all conditions ($F(1,84)= 30, p< .001$) and when predicting the shift in PMRT between the biased and control condition ($F(1,40)= 43, p< .001$). See Supplementary Methods - Section 4.4 for complete ANOVA tables.

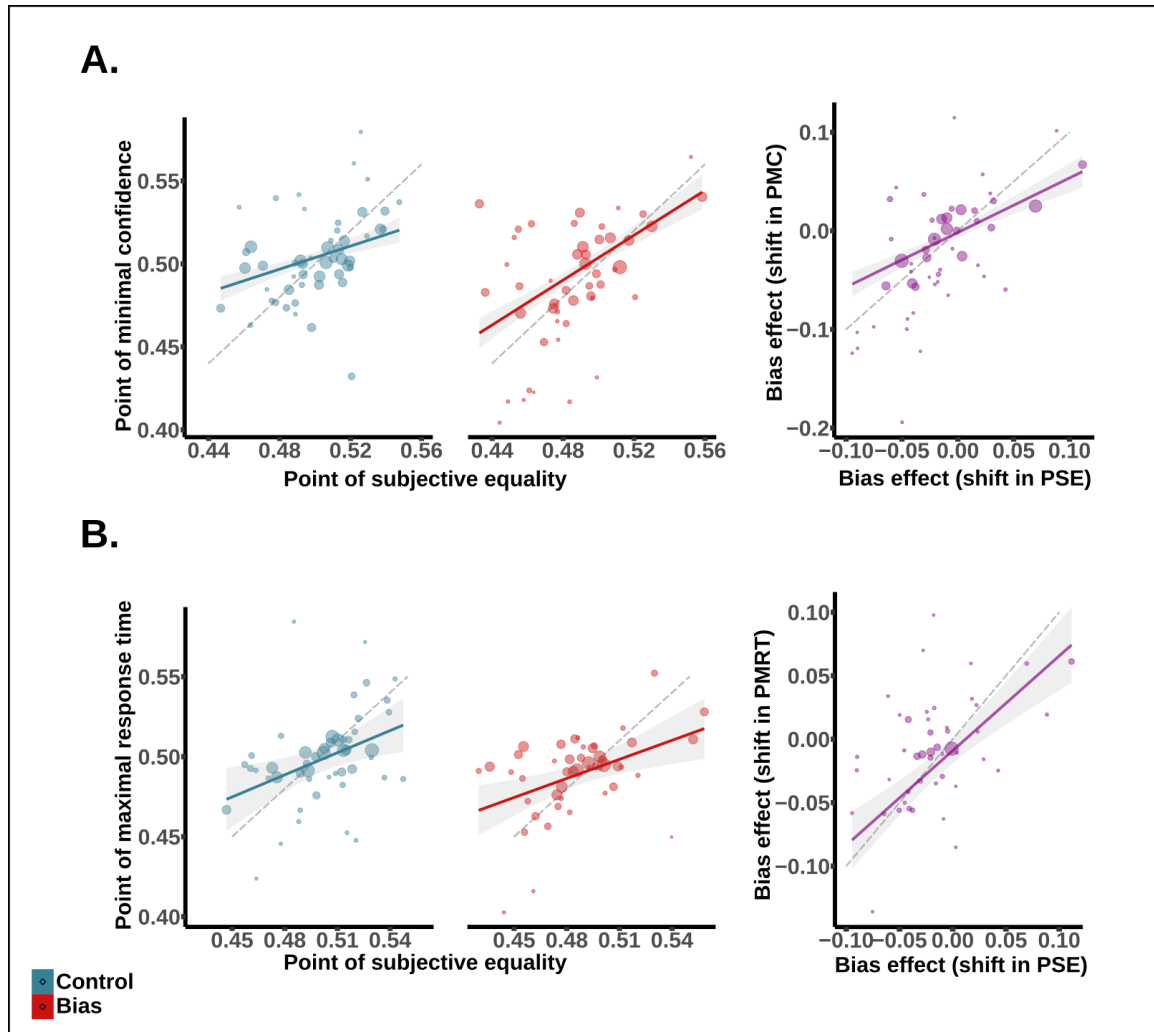


Figure 3. Joint shifts in performance, confidence and response times. A. Discrimination performance and perceptual confidence. (Left) Regression of individual PSEs against individual PMCs for biased and control conditions. Dot size was determined by the weight of PMCs in the regression, calculated as the inverse of the square sum of biased and control confidence intervals for each condition (*i.e.* the larger the CI, the smaller the weight). This ensured that less noisy individual PMCs contributed more to the regression than noisier PMCs. **(Right)** Regression of the shift induced by the bias on PMC (PMC biased minus PMC control) against the same shift for PSE (PSE biased minus PSE control). Dotted line represents the diagonal (slope=1). **B. Discrimination performance and response times (Left)** Regression of

individual PMRTs against individual PSEs for biased and control conditions. Dot size was determined by the weight of PMRTs in the regression, calculated as the inverse of the PMRT's 95% CI (*i.e.*, the larger the CI, the smaller the weight). **(Right)** Regression of the shift induced by the bias on PMRT (PMRT biased minus PMRT control) against the same shift for PSE (PSE biased minus PSE control).

---FOOTNOTE 1 START---[comes from "...using the inverse of the PMCs' 95% CI as individual weights¹."]]

Considering the inherent noise of individual-level polynomial fits implemented to obtain PMCs and PMRTs, we took this step to ensure that each PMC and PMRT would affect the regression only as a function of its reliability, in so privileging higher-quality fits (*i.e.*, the larger the CI, the smaller the weight). We argue that this measure is more reliable and sensitive than other measures of goodness of fit, which are limited to establishing a threshold based on observing if the p-value for the difference between fitted model and data is above 0.05.

---FOOTNOTE 1 END-----

Our second approach was based on comparing whether confidence ratings (or response times) were best predicted at the individual level by the actual stimulus proportion, or by the subjectively biased stimulus proportion. To do so, we fitted two different quadratic models, one based on target color proportion as an explanatory variable, centered around the objective point of equality (*i.e.*, target color proportion minus 0.5), and the other centered around subjective equality (*i.e.*, Target color proportion minus each participant's PSE). Recentering the proportion this way allowed us to simplify the model (by removing the linear component), and most importantly, to evaluate which was the better predictor of confidence and response times: actual signal equality

or subjective equality. Both models also included suggestibility and bias as additional regressors. As shown in Table 1, the subjective model conformed to our predictions and presented lower AIC and BIC in both cases, indicating that perceptual confidence and response times were better predicted by discrimination performance, rather than actual sensory information.

Table 1. Comparison for subjective and objective models predicting confidence and response times

	BIC	AIC	R ²
Confidence (Subjective model)	224351	224236	.36
Confidence (Objective model)	224504	224389	.35
Response times (Subjective model)	224389	224306	.36
Response times (Objective model)	224525	224443	.35

Objective models: models where target color proportion was re-centered around objective equality (i.e. 0.5). Subjective models: models where target color proportion was re-centered around each participants' point of subjective equality. Both models included suggestibility and bias as additional regressors, and a random intercept per participant. Goodness-of-fit was calculated as the R² for each model, evaluating fixed and random effects together. BIC: Bayesian Information Criterion. AIC: Akaike Information Criterion.

Discussion

In the present work we set out to test how, when adding a short false verbal instruction at the beginning of a standard psychophysical task, different individuals would exhibit different sensitivities to the resulting bias. In particular, we hypothesized that the magnitude of the perceptual bias would depend on suggestibility (construed here as a measure of social steerability). Our findings confirmed this hypothesis.

Our main result was that the perceptual judgment bias induced by our false statement depended on participant suggestibility. As expected, high suggestibility participants were influenced by the false statement, whereas the perceptual reports of low suggestibility participants remained unaffected by the induced bias. Importantly, the induced bias affected not only perceptual choices, but also confidence judgments and response times, in a joint manner. We note that such coupled shifts are informative, as they indicate an association of participants' objective and subjective uncertainty in our paradigm. This is not always the case, as oftentimes observers do not necessarily know when their performance is maximally uncertain (see e.g. Graves et al., 2021). Further, this joint shift is useful to discard a scenario where participants would just follow the (false) statement when their confidence was at its lowest (i.e., "when in doubt, I go with the color from the instruction"). Namely, if participants had adjusted their behavior solely in a strategic manner to conform to the false instruction, the latter would have induced a shift in perceptual responses but not necessarily in subjective confidence. It should be underscored however that this does not rule out other scenarios in which decisional effects could be consistent with the observed joint shift. As a matter of fact, determining whether our findings reflect purely decisional or strictly perceptual modulations would plunge us in a much deeper debate, which lies beyond the reach of our findings. On the one hand, when encountering such joint shifts in subjective equality, maximal response times and minimal confidence, several recent studies have converged

in interpreting that the induced bias was strictly perceptual (Sterzer et al., 2008; Gallagher et al., 2019; Maldonado Moscoso et al., 2020; Gallagher et al., 2021; Caziot & Mamassian, 2021). The study by Gallagher et al (2019), in particular, separates two empirical phenomena, with on the one hand perceptual changes producing a joint change on categorical decisions and confidence reports, and on the other hand decisional effects impacting only decisions but not confidence. Likewise, Caziot & Mamassian (2021) have shown that maximal response times (and thus maximal task difficulty) also shifted with subjective equality, favoring in their view the hypothesis of a perceptually-driven response shift. On the other hand, other authors have argued that expectation effects are located at the decision level. For instance, Rungratsameetaweemana et al (2018) show that EEG markers of early sensory processing and sensory evidence accumulation during decision-making remained impervious to feature or motor expectations, despite the presence of robust behavioral expectation effects. In this same line, Bang & Rahnev (2018) contrasted pre-stimulus and post-stimulus cueing trials in a visual orientation task, and found that expectations likely impacted decision criteria, rather than directly modulate sensory processing efficiency. This second line of findings clearly suggests that prior knowledge about statistical regularities and trends in the sensory environment would primarily influence late-stage cognitive processes related to response preparation and execution, strongly favoring the decisional-driven shift hypothesis. In any case, deciding in favor of one alternative or the other lies beyond the scope of the present work. Regardless of the shifts' mechanics, our findings show that its magnitude was reliably associated with participants' permeability to high-order social influence.

It is important to underscore that while this bias did hinge on suggestibility, the actual coupling of perceptual reports, confidence judgments and response times did not: our analyses revealed that PSE shifts (i.e. the difference in PSEs between biased and neutral blocks) could predict shifts in PMCs or PMRTs, with no interaction with suggestibility (see Figure 3 and Tables SM 15-16, 19-20). This indicates that shifts in PMCs and PMRTs were sufficiently explained by shifts in PSEs alone, irrespectively of suggestibility. We interpret this as evidence that subjective equality,

minimal confidence and maximal response times are tied to the outcome of the perceptual process, rather than to the factors that might affect this outcome, such as suggestibility and induced expectations.

An additional set of interesting albeit unexpected findings concerned slower response times and lower confidence observed in low suggestibility participants in biased blocks. By contrast, high suggestibility participants presented the highest overall confidence levels for the biased condition, and response times that did not differ significantly from control. We interpret this delay in response times and general lack of confidence in low suggestibility as reflecting some level of cognitive conflict introduced by the false instruction. Such a delay would be coherent with the incongruity effects typically seen in stroop effects and other forms of cognitive conflict (Cohen, 2014). We find this interpretation all the more likely considering how choice behavior remained unchanged for these participants regardless of bias: low suggestibility participants resisted the cue whereas high suggestibility participants incorporated the cue as additional (and non-conflicting) information.

The fact that a simple description, even when false, would interact with perceptual judgments in a manner coherent with susceptibility, reminds us of other forms of influence such as hypnosis and placebo (Geuter et al., 2017; Terhune et al., 2017). Indeed, hypnosis and placebo research too have studied manipulations of perception through suggestion, paying special attention to individual differences in susceptibility (Cardeña & Terhune, 2014; Sheiner et al., 2015). Unlike the present study, however, hypnosis and placebo studies rely on the installation of an elaborate social and behavioral context. Active placebo oftentimes couples suggestion with an associative learning phase consisting of convoluted paraphernalia, behavioral routines and elaborate mechanisms of benign deceit (Benedetti et al., 2003; Schafer et al., 2015; Benedetti et al. 2016; Geuter et al., 2017). Hypnosis generally uses mental routines to induce participants to actively imagine agency and perceptual changes (Terhune et al., 2017). In our work, however, we show

that false simple verbal hints and instructions suffice to trigger shifts in perceptual judgements, despite the absence of associative learning schemes or mental exercises.

Our findings support the idea of examining all mechanisms of suggestion under the same lens (i.e., as instances of perceptual psychosocial influence), and set the terrain for a study of suggestibility as a core feature of standard perceptual processing. Namely, simple short phrases and verbal instructions may actually condition what we see and feel in everyday life, particularly if we are highly permeable to social influence. Our findings warrant further study of this phenomenon, and invite the possibility that other findings in the perceptual literature may be the result of suggestion and expectation. For example, recent findings showing the ostensible impact of hypnotizability on the rubber-hand illusion (Lush et al., 2020), or confederate influence on perceptual judgements during the Asch experiment (Hajnal et al., 2020) support this proposal.

Along the same lines, our work brings forth the pertinence of inventories where suggestibility is construed as a measure of social steerability to study influence under regular perceptual contexts. Such inventories might be relevant for the development of improved measures of suggestibility, an issue recently revisited in the hypnosis domain (Acunzo & Terhune, 2021; Oakley et al., 2021; Kallio, 2021). Moreover, our findings suggest that scales of suggestibility could be valuable tools for studying not only perception, but more generally the construction of judgments, beliefs and trust. Currently, efforts are being made to implement Signal Detection Theory to unravel the appeal of fake news (Batailler et al., in press), and great attention is being paid to the role of direct hints and labels to estimate social media's trustworthiness and virality (Moravec et al., 2018). Tackling these questions from a suggestibility perspective appears in our view as a natural step towards the development of a renewed psychology of influence, which should interest researchers, communicators and policy-makers alike.

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Open practices statements

The data for the experiment is available at the Open Science Framework repository (https://osf.io/cxbaf/?view_only=0b0c4edbd3224aaf845e1a34584505d5). A preprint for this experiment is available online (<https://www.biorxiv.org/content/10.1101/2021.04.28.441710v1>).

Due to a technical error in the preregistration procedure on behalf of the authors, this experiment was not preregistered.

Author note

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