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Does unconscious control depend on conflict?

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Abstract

Unravelling the mechanisms that trigger cognitive control is a central question in cognitive science. Cognitive conflict is closely associated with the activation of cognitive control, helping individuals to follow their own goals. In this study, we examined whether the association of conflict and control holds when people are not aware of their intentions (i.e., they experience involuntariness regarding their behaviours). To induce unconscious control, we employed a posthypnotic suggestion (word blindness: that words will appear as a meaningless foreign script) on highly suggestible participants, a manipulation which has previously been shown to halve the Stroop interference effect. To alter the amount of conflict, we manipulated the proportion of incongruent, congruent and neutral Stroop trials between blocks in two experiments. The analyses revealed that the Stroop effect was reduced by the suggestion in the high conflict conditions (conditions with 33% or more incongruent trials), and barely at all in the low conflict conditions (conditions with 10% incongruent trials) compared to no suggestion conditions, thus, supporting the idea that a certain amount of conflict is required to activate unconscious control. This finding can also be interpreted in light of the two competing accounts of the word blindness effect (de-automatisation of reading and response competition models). The results imply that conflict between the response options occurs even in the suggestion condition and so the word blindness suggestion does not influence semantic processing as the de-automatisation of reading account postulates, rather, it is more likely that the suggestion facilitates the reduction of response competition.

Keywords: Cognitive control, Unconscious control, Stroop effect, Post-hypnotic suggestion

Public Significance Statement

Cognitive control is the ability with which we can achieve goals even when we are surrounded by distractions. However, the application of cognitive control is effortful and so we need to regulate the level of control so as to not waste resources. According to a seminal theory of the field, we are continuously monitoring our environment and when a conflict occurs in the system, for instance, multiple responses seem sensible, the use of cognitive control increases and top-down processes intervene to ensure that we behave in accord with our goals. This study tested whether there is a link between cognitive conflict and the use of control when people are not aware of their own intentions, or in other words, they use unconscious control processes. We employed a posthypnotic suggestion to evoke unconscious control: We suggested that the English words on the screen would appear as words in a meaningless foreign script. It had previously been shown that the meaning of the words interfere with ongoing tasks less because of the posthypnotic suggestion, even though subjects do not experience themselves as intentionally dealing with the conflict. We found that the more conflict there was between the meaning of the words and people's actions, the greater effect the suggestion had. Our results indicate that conflict plays a crucial role in the use of cognitive control irrespective of the conscious nature of our intentions.

Cognitive control is the hallmark of human cognition as it allows people to achieve their goals, even in constantly changing environments, through processes of error monitoring, conflict resolution and response control. How people know when to intervene and activate control over their behaviour is a central question for cognitive science (Botvinick, Braver, Barch, Carter, & Cohen, 2001). The *conflict-monitoring theory* (Botvinick et al., 2001; Botvinick, Cohen, & Carter, 2004) provides a behavioural and neuropsychological account, according to which cognitive control can be triggered by the conflict of opposing responses, with this monitoring and evaluation process localised to the dorsal anterior cingulate cortex (dACC). In other words, humans are capable of activating and maintaining goal-directed behavior by constantly assessing task-demands in the form of the evaluation of response conflict. The theory has gained support from behavioural phenomena such as the *Congruency Sequence Effect* (CSE) and the *Proportion Congruency Effect* (PCE). These phenomena can be observed in various interference tasks, such as the Stroop task (Stroop, 1935; see MacLeod, 1991, for a review), and their cornerstone is that people tend to adapt to incongruent tasks (trials in which two conflicting responses can be activated by the presented stimulus) owing to preceding conflicts (Botvinick et al., 2001). For example, the congruency effect (incongruent-congruent trial performance) is shown to be decreased on trial n when trial $n-1$ is an incongruent trial compared to when it is a congruent trial (CSE; Duthoo, Abrahamse, Braem, Boehler, & Notebaert, 2014; Egner, 2007; Gratton, Coles, & Donchin, 1992). Moreover, the congruency effect is decreased in contexts where the proportion of incongruent trials is higher compared to contexts with lower proportions of incongruent trials (PCE; Bugg, 2012; Gratton, Coles, & Donchin, 1992; Logan & Zbrodoff, 1979; Tzelgov, Henik & Berger, 1992). In both instances, incongruent trials are considered to be the index of conflict that eventually triggers cognitive control processes demonstrated by the enhancement of performance.

Many models of cognitive control deem control processes to be inherently related to consciousness, particularly to the conscious awareness of one's intentions (Jacoby, 1991; Norman & Shallice, 1986). Empirical research called into question this putative link between control and consciousness by showing that control processes can be activated by unconscious information about conflicting stimuli (Dehaene, Lau, & Kouider, 2017; Kunde, Reuss & Kiesel, 2012; Lau, 2011; van Gaal, De Lange, & Cohen, 2012; van Gaal, Lamme & Ridderinkhof, 2010). Nonetheless, these studies did not challenge the premise that cognitive control is accompanied by the conscious feeling of effort and volition, and argued merely that the processing of conflict does not reach consciousness (Lau, 2011). Therefore, it is still unexplored whether the association between conflict and control holds when people experience involuntariness, the lack of intentions, regarding their own behavior.

Building on the *higher-order thought* (HOT) theories of consciousness (Lau & Rosenthal, 2011; Rosenthal, 1986, 2002), control processes can be labelled as unconscious, if the person who is acting cannot report on her intentions concerning the behaviour in question. HOT theories differentiate between mental states that reflect the external world (first-order mental states) and mental states about other mental states (second- or even higher HOTs). The theory postulates that one has to have a second order thought about the first order mental state to be aware of the first order state; therefore, having an intention, but lacking a HOT about it

makes the intention unconscious. Possessing an inaccurate HOT about the intention can also indicate unconscious control, but it is not a necessary requirement. Here, we refer to involuntary, and so unconscious, control as a behavior derived from a first-order intent to act (e.g., with content “arm rise!”) that is either accompanied by a HOT about the lack of that intention (e.g., with content “I do not intend to raise my arm”) or is not accompanied by any HOT regarding the intention.

Hypnotic responding can be understood as an instance of unconscious control, as its defining feature is the feeling of involuntariness that accompanies a usually mundane act such as raising one’s arm or hearing the tune of Happy Birthday (Weitzenhoffer, 1974, 1980; Terhune, Cleeremans, Raz & Lynn, 2017). Although theories of hypnosis share the assumption that a hypnotic response feels like it is happening by itself, not all of them concur with the claim that the response is constructed by cognitive control processes. For instance, the *response expectancy theory* claims that expecting a behavior to happen can induce that particular behavior and so suggestions can be implemented without the involvement of intentional executive systems (Kirsch, 1985, 1997). Consequently, the theory assumes that behaviors following hypnotic suggestions feel involuntarily since they lack the intention and are driven by the expectation itself. This theory can partially explain the effect of suggestions, however, it has been demonstrated that expectations cannot explain much variance in behavior (Benham, Woody, Wilson, & Nash, 2006), which may be due to measure unreliability but also leaves space for other, cognitive ability based models. Several theories, such as the sociocognitive (Coney & Kirsch, 1999; Spanos, 1986) and dissociation theories (Bowers, 1990; Hilgard, 1977, 1991; Kihlstrom, 1985), emphasize that the subjects of hypnotic suggestions play an active role in creating the hypnotic response. By synthesizing these theories, the *cold control* theory offers a solution to how a strategic and intentional behavior can be sensed as involuntary. It posits that the key to responding hypnotically is to alter one’s monitoring of one’s own intentions, thus, the feeling of involuntariness arises from possessing inaccurate HOTs about the intentions (e.g., “I am not intending to raise my arm”) and not from the lack of intentions (Barnier, Dienes & Mitchell, 2008; Dienes, 2012; Dienes & Perner, 2007). Hence, cold control theory postulates that a hypnotic response should be labelled as unconscious control. Although cold control theory provides a general explanation that suggestions are implemented by the involvement of executive functions, the mechanism by which control processes are activated, when the intentions are unconscious, is still unclear.

The application of the *word blindness* suggestion (Raz, Shapiro, Fan, & Posner, 2002; this term was coined by Parris, Dienes, & Hodgson, 2012) offers a unique opportunity to investigate how unconscious control can be activated. The word blindness suggestion is an instruction for highly suggestible people (henceforth “highs”) that the words (i.e., stimuli of the Stroop task) will appear as a meaningless foreign script on the screen. The effect has been replicated by several independent research groups (Augustinova & Ferrand, 2012; Casiglia et al., 2010; Parris, Dienes, & Hodgson, 2012; Raz, Kirsch, Pollard, & Nitkin-Kaner, 2006), even with slightly modified wording (Zahedi et al., 2017), and it has been shown to be a reliable tool to halve the Stroop interference effect, as measured by the response time (RT) difference between incongruent and neutral trials (Parris, Dienes, & Hodgson, 2013; See Table 1. for the

results of a meta-analysis). Importantly, this reduced interference is not a consequence of participants slowing down on neutral trials in the suggestion conditions compared to the no suggestion conditions but it is the result of speeded responses on incongruent trials, making it unlikely that the enhancement of performance is faked (MacLeod, 2011). These studies attest to the robustness of the word blindness effect, however, the debate on the underlying mechanisms of the suggestion remained unsettled between two classes of models. Distinguishing between these two competing models has broader implications than a mere understanding of the underlying mechanism of the word blindness effect. This issue is closely related to the main question of the current paper about the role of conflict in the activation of unconscious control.¹

The de-automatisation of reading model posits that the process of reading is not obligatory and it can be impaired or even hindered in a top-down way, by influencing the neural processing of input words (Raz et al., 2002, 2006; Raz & Campbell, 2011). Specifically, it assumes that the reduction of the Stroop interference effect, when the suggestion is active, is simply the result of a general impairing of the visual stimuli by which subjects cannot employ information about the meaning of the words, thereby, they experience congruent and incongruent trials as identical. Several studies provide evidence consistent with this model. First, it was demonstrated that the word blindness effect arises even when visual accommodation (e.g., visual blurring) and the usage of attentional strategies (e.g., looking away) are prevented (Raz et al., 2003) suggesting that a simple optical explanation cannot account for the phenomenon. Second, by applying the combination of neuroimaging methods (fMRI and EEG), Raz, Fan and Posner (2005) found that the suggestion mitigated the activity of the occipital cortex underscoring the idea that early visual information processing and not the operation of the visual sensory organs can be modulated by the application of the word blindness suggestion. In addition, a recent study using EEG showed evidence for increased theta-band and beta-band activity in the frontal-midline region in response to the activation of the word blindness suggestion, implying the increased involvement of top-down, cognitive control processes (Zahedi et al., 2017). This finding is in accord with the explanation of the de-automatisation model that the suggestion dampens early visual processing by a top-down mechanism. Nevertheless, these data are also in line with other cognitive control based models.

The response competition model proposes that the word blindness suggestion takes its effect at a later stage of the process, namely, at the level of response competition resolution (Augustinova & Ferrand, 2012; Parris, Dienes, & Hodgson, 2013). This model assumes that upstream information processing (e.g., semantic processing) remains mainly untouched by the suggestion and so word reading is not de-automatised. Hence, conflicts between word meaning and color can and will arise. According to the response competition driven model of word blindness suggestion effect, the resultant response competition is key in the application of the suggestion. In accord with the prediction that conflict activates conscious cognitive control

¹ Of note, the current study was not design to test the assumption of cold control theory (and several other theories of hypnosis) that responses to suggestions feel involuntary. We take this assumption for granted based on the abundant evidence from the field of hypnosis (e.g., Kihlstrom, 2008) and relying on another study demonstrating that highs and mediums report decreased control over the meaningfulness of the words in a suggestion condition compared to a no suggestion condition (Palfi, Parris, McLatchie, Kekecs & Dienes, 2018).

processes as portrayed in the conflict-monitoring model (Botvinick et al., 2001; Botvinick, Cohen, & Carter, 2004), the response competition account argues that response competition triggers control processes required for the suggestion to take its effect. This is because the presence of response competition leads to an enhanced recruitment of control permitting the word blindness suggestion effect. Without response competition, for example when trials involve only semantic-associative conflict, the suggestion is not triggered, and the extra control permitted by the suggestion does not take effect.

This model is also in accordance with the finding that cognitive control related brain activity increases in the suggestion conditions compared to no suggestion conditions (e.g., Zahedi et al., 2011), as response selection is a primary function of cognitive control (Miller & Cohen, 2001). A handful of behavioral studies have also provided evidence supporting this model. First, Augustinova and Ferrand (2012) compared the effect of the suggestion on a traditional and a semantic version of the Stroop task (Neely & Kahan, 2001). The interference in the later version of the task is entirely the product of semantic factors (incongruent trials consist of words that are highly associated with colors but their font color does not match with it, such as *sky* presented in *green*) and it reduces conflict derived from response competition. Their results revealed that the word blindness suggestion could not reduce the interference effect on the semantic Stroop task, whereas it could mitigate the interference effect on the standard Stroop task, implying that not visual information processing but response competition is affected by the suggestion. Second, Parris, Dienes, and Hodgson (2013) analysed the distribution of the RTs of their earlier study by applying the ex-Gaussian approach² and their results were in line with the view that the enhanced resolution of response competition is the underlying mechanism of the word blindness suggestion.

At root, the two portrayed models of the word blindness suggestion have an opposing stance on how the suggestion itself, more generally unconscious control, is activated. The de-automatisation of reading model proposes that the suggestion can reduce Stroop interference by preventing the conflict from happening: if the meaning of a word is not processed then the conflict between the meaning and the color cannot occur. Consequently, the model assumes that conflict does not play an essential role in the activation of the suggestion. Alternatively, the response competition model suggests that conflict between the meaning and the color still emerges and the increased activation of control processes permits the influence of the suggestion. The latter position intimates that the word blindness suggestion effect operates by somehow piggy-backing on existing control mechanisms.

In order to disentangle these two models, we produced two experimental designs with altering levels of conflict from one block to another. Specifically, we manipulated the proportion of incongruent trials in the experimental blocks. By increasing the proportion of incongruent trials one can raise the level of response conflict (e.g. Logan & Zbrodoff, 1979).

² The ex-Gaussian approach focuses on the distribution of the RTs and one of its parameters (μ , the mean of the Gaussian distribution) is known to be a useful tool for indexing response conflict (Steinhauser & Hübner, 2009; cf. White, Risko & Besner, 2016). The ex-Gaussian function helps better exploit RT data and reveal effects which would remain hidden for the traditional analysis process (i.e., the exclusion of trials with prolonged RTs and aggregation of RT data in each condition can result in information loss for positively skewed distributions).

When the proportion of incongruent trials is high, response competition occurs more frequently increasing the recruitment of control processes; under such conditions, the Stroop effect is reduced. When the proportion of incongruent trials is low, response competition occurs less frequently reducing the activation of control processes; under such conditions the Stroop effect is increased. If the word blindness suggestion effect is the result of top-down control over input words (as per the wording of the suggestion and Raz and colleagues' conclusions) the proportion of incongruent trials should not affect the presence or magnitude of the effect. Alternatively, if the word blindness suggestion effect is dependent on the operation of mechanisms that control response competition, the effect should be larger when the proportion of incongruent trials is high (due to the enhanced activation of control processes).

We devised two experiments (Experiment 1A and 1B) with varying proportion of incongruent trials to allow for analyses exploring the minimum amount of conflict that is indispensable to the activation of control. However, the main analyses proposed in the paper are based on the merged dataset of these two experiments to maximise the sensitivity of the core test. Namely, whether the word blindness effect is stronger in a high than in a low incongruence proportion block as predicted by the response competition model, or it is equally efficient in the two blocks as predicted by the de-automatisation of reading model. To supplement the analyses of the objective measures (RTs), we also assessed the conscious experience of word meaningfulness of the participants, and the expectations of the participants about seeing the words as meaningless characters by employing subjective reports. We aim to discriminate between two accounts (de-automatisation of reading vs. response competition) of the word blindness suggestion effect. If the response competition model is supported, the experiment addresses a key question raised earlier about the role of consciousness in cognitive control: When intentions are unconscious, what are the mechanisms by which control is triggered?

Methods

Participants

Experiment 1A. We recruited 23 (21 females, $M = 19.13$, $SD = 0.81$) highs from the subject pool of the University of Sussex to participate in our experiment. The hypnotisability of these students was measured with the Sussex Waterloo Scale of Hypnotisability (SWASH; Lush, Moga, McLatchie & Dienes, 2018) on group screening session at the university. This scale is a modified version of the WSGC (Bowers, 1993), it is adjusted to be ideal for screenings with larger groups of people and it contains items for each suggestion assessing the subjective experiences of the participants. Students, whose hypnotisability score is in the top fifteenth percentile based on their composite score (arithmetic mean of the subjective and objective scores), were recruited via email to attend the experiment (this threshold is comparable with those of employed by other researchers in the field; e.g., Anlló, Becchio, & Sackur, 2017; Barnier & McConkey, 2004). Based on the preregistration of the project (see details below), the stopping rule was to cease recruitment when the Bayes factor of the critical test (see details below) reached good enough sensitivity (either below 1/3 or above 3) or when had run 20 participants. All participants were informed about the nature of the study and all of

them signed the consent form before participation. After finishing the experiment, the participants were debriefed and received a payment of £7 or course credit. The Ethical Committee of the University of Sussex (Sciences & Technology C-REC) approved the study.

Experiment 1B. Fourteen highly suggestible students (10 females, $M = 19.00$, $SD = 0.78$) participated in the experiment. The stopping rule was to run as many participants we responded in the following term (less than 1A because previously run participants could not be run again.) The steps of the recruitment and incentivization of the participants were the same as those of Experiment 1A.

Stimuli and apparatus

Experiment 1A. The stimulus set of the experiment closely followed those of the stimuli used by Raz et al. (2002). The stimuli contained 4 color words (RED, BLUE, GREEN, YELLOW) and 4 neutral non-color words (LOT, SHIP, KNIFE, FLOWER). The displayed color of the color words can be red, blue, green or yellow depending on the congruency type of the current trial. Each neutral word can be only presented in the colors to whose name it is not matched for length. For instance, the word LOT can be coloured in blue, green and yellow, but not in red. The words were written in upper-case font and their vertical size was 0.6 cm (visual angle of 0.5° from 65 cm) and their horizontal size varied between 1.3 and 2.4 cm (visual angles of 1.146° - 2.115°). The stimuli were presented against a white background on a computer screen with a resolution of 1280x1024. The participants were able to indicate their answers about the displayed color of the words by pressing one of the following buttons on the keyboard: “V”, “B”, “N”, “M”. The Stroop task part of the experiment was run in Opensesame (Mathôt, Schreij, & Theeuwes, 2012).

Experiment 1B. The stimulus set and the apparatus matched those of Experiment 1A.

Design

Experiment 1A. We employed a $2 \times 2 \times 3$ within subjects design with independent variables: (i) presence of the word blindness suggestion (present vs. absent), (ii) proportion of incongruent trials in the current block (PI 10% vs. PI 80% where PI refers to the proportion of incongruent trials or simply proportion incongruence) and (iii) congruency type of the current trial (congruent vs. incongruent vs. neutral). The percentage of the neutral trials was held constant in all blocks (10%).

Experiment 1B. This experiment had a $2 \times 3 \times 3$ within subjects design. Two independent variables remained identical to those of Experiment 1A, namely, the presence of the word blindness suggestion and the congruency type of the current trial. We added two new levels to the the variable of the proportion of the incongruent trials in the current block and removed the PI80% block (levels of proportion incongruence block: PI 10%, PI 33%, PI 45%). The first level of the variable was identical to that of the PI 10% block of Experiment 1A. The PI 33% block had equal proportion of all three congruency types of the trials, which is considered to be the default for studies investigating the word blindness effect (e.g., Raz et al., 2002; Augustinova & Ferrand, 2012; Parris, Dienes, & Hodgson, 2012). The PI 45% block had

45% congruent and 10% neutral trials so that the proportion of neutral trials remained constant compared to the blocks of the previous experiment.

Procedure

Experiment 1A. Data collection was conducted in a small experimental room with the experimenter present. After providing informed consent, the participants were told that they will receive a hypnotic induction and a suggestion and that during the experiment they will be asked to report their subjective experiences and expectations about the effect of the suggestion. Next, they were informed that they will play a computer game where their task is to indicate the ink color of the word on the computer screen. They were asked to lay their left middle finger on V, left index finger on B, right index finger on N and right middle finger on M. The corresponding buttons of red, blue, green and yellow will be V, B, N and M, respectively. The buttons on the keyboard were neither color labelled nor marked. The importance of speed and accuracy were equally highlighted in the instructions ("Please respond as quickly and as accurately as you can."). The participants were asked to focus on a black fixation cross that appeared in the middle of the screen and was replaced by a colored word after 500ms. They had 2000ms to indicate their answers, and after pressing one of the buttons, a feedback indicating the correctness of their last answer appeared on the screen for 500ms (the word `CORRECT` or `INCORRECT` in black color). The interstimulus interval was 2000ms.

Figure 1 shows the process of the administration of Experiment 1A. As a first step, the participants received 36 practice trials (12 trials from each congruency type). Performance was not assessed in the training session and all participants continued the experiment after finishing the practice block by receiving a standard hypnotic induction. The hypnotic induction was followed by a single question to measure the `depth` of hypnosis (Hilgard & Tart, 1966; "How deeply hypnotised are you?"; 0 – normal state, 1 – relaxed, 2 – hypnotised, 3 – deeply hypnotised). Next, we introduced the word blindness suggestion of Raz et al. (2002). To check the effect of the suggestion, participants were asked to rate the meaninglessness of a presented colored word on a 4 point Likert scale ("How strongly do you experience the word as meaningless?"; 1 – completely clear, 2 – little unclear, 3 – unclear, 4 – completely unclear). Participants answering 3 or 4 received the deactivation of the suggestion and the de-induction, while participants choosing 1 or 2 heard the following script from the experimenter before the deactivation of the suggestion and the de-induction to help them achieve the full potential of their response to the suggestion:

"Notice how as you look at the word on the screen, you can look at it with the meaning fading to the background of your mind. We have found even when people consciously experience some meaning after this suggestion, they still process the words differently at a deeper level. You know you are capable of not reading meaning fully, remember how you have zoned out while reading a book." (Raz et al., 2002)

After the de-induction, participants were asked again to report the depth of hypnosis they currently experience. Next, the participants engaged in four Stroop blocks: they received both types of blocks (PI 80% vs. PI 10%) with and without the suggestion. A clap activated and a double clap deactivated the suggestion. In each block, they received 120 trials. The order

of the trials within the blocks was randomly generated and the order of the four blocks was counterbalanced between the participants. Before each Stroop block, the participants were asked to rate on a 5 point Likert scale how strongly they expected that the words on the screen would be meaningless. After each block, participants were asked to report their subjective experience about the effect of the word blindness suggestion by indicating the percentages of the trials with clear and meaningful words. The subjective experience of meaningfulness was assessed with four items each time. In addition, we also asked them to indicate their experience about the depth of hypnosis they experienced during the last Stroop block. For the exact questions and choice options see Appendix A. These questions and the options were read out by the experimenter and were provided on a paper for the participants. Finally, the participants were thanked for attending and debriefed.

Figure 1. Flowchart depicting the four versions in which Experiment 1A was conducted. The participants were randomly assigned to one of the four versions. The clap was used as a cue to activate the word blindness suggestion and the double clap was used to deactivate the suggestion. The proportions of the incongruent, neutral and congruent trials in the PI80% and PI10% Stroop blocks were 80%, 10%, 10% and 10%, 10%, 80%, respectively.

Experiment 1B. The procedure closely followed that of Experiment 1A with a difference of having three no-suggestion and three suggestion blocks instead of two and two,

respectively. The PI 10% block was presented as the third in both of the suggestion and no suggestion conditions and the order of the other two blocks were counterbalanced.

Data analysis

Statistical analyses. We conducted the data transformation and ran all analyses with the statistical software R 3.3.1 (R Core Team, 2016). The R markdown script about data processing and the analyses can be retrieved from osf.io/pk2st. We had a within-subjects design and, therefore, we simplified all of our tests to direct comparison of two conditions so that they could be conducted by running paired t-tests or simple linear regressions. We define Stroop interference as the RT difference between incongruent and neutral trials, and Stroop effect as the RT difference between incongruent and congruent trials. To explore the relationship between the RTs, the subjective reports and expectations, we ran linear regressions and tested the slopes against zero. In addition to the p-values, we calculated Bayes factors (Bs) and used them as a basis of decision making about the hypotheses. Moreover, to explore the extent to which the participants experienced being in a hypnotic state during the Stroop task, we conducted parameter estimation and reported the raw effect sizes (condition means) with the 95% Credibility Intervals (CIs). We calculated the 95% CIs by assuming a uniform prior distribution, thus, the values of the 95% CIs are numerically identical to the 95% Confidence Intervals.

Bayes factor. We used the Dienes and McLatchie (2018) calculator to compute the Bayes factor and extended it so that it can model the predictions of the alternative hypotheses (H1s) with a uniform distribution (See the R script of the calculator in Appendix B.). We employed t-likelihood function for each analysis and we set the degrees of freedom of the theory to 10^{10} so that the distribution of the likelihood function was comparable with a normal distribution. We report Bs as evidence for H1 over H0 in the following formats depending on the model of H1: $B_{H(0, X)}$ or $B_{U[0, X]}$. The H in the subscript indicates that we used a half-normal distribution and the U indicates that we used a uniform distribution to model the predictions of the alternative hypotheses. For the half-normal models, the first character within the brackets indicates that the mode of the distribution was zero and X stands for the SD of the distribution, which varied among the analyses. For the uniform models, the first value within the brackets is the lower limit of the distribution, which was zero in all cases, and X marks the upper limit, which differed across the analyses.

We used half-normal models for the outcome neutral and crucial tests (i.e., analyses of RTs) as its application fits the scientific intuition that small effect sizes are more probable than large effect sizes; and we employed uniform distributions for the supporting and exploratory tests (all labelled as exploratory analyses in the preregistration) as we had no prior information about these questions. Nonetheless, a small effect size is more probable than a large one (Dienes, 2017), thus, we recalculated the Bs based on uniform distributions to test the robustness of their conclusions to the shape of the distribution modelling the predictions of H1 (See Supplementary Materials for results). To determine the parameters of the models, we used two strategies throughout the analyses. When we had estimates of effect sizes from the relevant literature, we applied those as the SDs of the half-normal models. For instance, for the analyses of the Stroop interference, the Stroop and the Suggestion effects (difference in the reduction of

the interference effect between no suggestion and suggestion conditions), we informed our models based on the meta-analysis of Parris et al. (2013, Table 1) who summarized the results of 7 studies on the word blindness effect. We used the means of the Stroop interference, the Stroop and the Suggestion effects, which are 62ms, 90ms and 64ms, respectively. In the absence of prior information about the expected effect size, we derived the parameters of the models from the maximum effect size (Dienes, 2014). For the analysis with half-normal models, we took the half of the maximum effect size as the SD of the distribution. For instance, we halved the SD that we used to test the Stroop effect and employed it as the expected effect size for the test of the proportion congruency effect (that is, the Stroop effect is expected to be smaller in the blocks with higher proportion of incongruent trials). For the analyses with uniform models, we took the maximum effect size as the upper limit and zero as the lower limit of the distribution. For example, to test the effect of suggestion on the self-report scales, we simply used the maximum value of the scale as the upper limit, and the minimum value of the scale as the lower limit of the uniform distribution. For the regression analyses, the maximum of the possible effect size was the perfect linear relationship between the examined variables (from the minimum of each scale to the maximum, e.g., 100/4 for the test of the subjective measures and expectations: the ratio of scales heuristic), therefore, we used 100/4 as the upper and 0 as the lower limit of the model of H1s.

To distinguish good enough and insensitive evidence, we used the convention suggested by Jeffreys (1961), which is 3 for H1; and thus, by symmetry, 1/3 for H0. We also employed the evidence labels used by Lee & Wagenmakers (2013) to further differentiate between moderate ($3 < B < 10$ or $1/10 < B < 1/3$) and strong evidence ($B > 10$ or $B < 1/10$). Finally, as the value of a B depends on the features of the distribution we choose to model the predictions of a hypothesis (e.g., the SD of the half normal distribution; Rouder, Morey, Verhage, Province & Wagenmakers, 2016; Rouder, Morey & Wagenmakers, 2016), we report Bayesian robustness regions (RR) to assess the robustness of the conclusions of the acquired B s to the value of the applied SDs. The robustness regions are notated as RR[x1, x2] in which x1 is the smallest SD of the model that would produce a qualitatively identical conclusion as the chosen SD (e.g., if the B was greater than 3 then the B calculated with x1 equals to or is slightly greater than 3), whereas x2 is the largest SD that would bring us to the same conclusion.

Implementation of the preregistration

The protocol of Experiment 1A and the analysis plan was preregistered here <https://osf.io/7ma4t/>. At a handful of steps, however, we deviated from this plan for either theoretical or practical reasons. First, consider our main hypothesis, which aimed to test whether a minimum amount of conflict is required to trigger the word blindness suggestion. We planned to use the extent of the Stroop effect as the index of conflict. However, based on a more thorough review of the literature (see introduction), we chose the proportion of incongruent trials as the index of conflict. Thus, the minimal required conflict hypothesis predicts that the suggestion should be more efficient in the high incongruence proportion blocks compared to the low incongruence proportion blocks. In addition, we tested the efficiency of the suggestion on the ability to reduce the Stroop interference effect rather than the Stroop effect as Parris et al. (2013) demonstrated that the suggestion affects the interference

component of the Stroop effect most frequently and the facilitation component (mean RT of neutral trials – mean RT of congruent trials) less often. We also desisted from the usage of ratio scores (e.g., Stroop effect would have equalled to the congruent RTs divided by the incongruent RTs) to calculate the extent of the Stroop effect and employed the same procedure as other studies in the field of the word blindness effect, namely, the difference scores (e.g., Stroop effect equals to the difference of incongruent and congruent RTs, Stroop interference equals to the difference of incongruent and neutral RTs). Supplementary Materials presents the results with ratio scores; the pattern and conclusions are the same; in light of this, to aid the comparison of our results to other studies of the word blindness effect, we present the results of the difference score analyses here, and ratio scores in the Supplementary Materials. Second, we included an extra outcome neutral test to examine whether the manipulation of the proportion of incongruent trials successfully influenced the extent of the Stroop interference and Stroop effects in the different blocks (we expect larger effects in blocks with smaller incongruence proportions).

The materials and procedure of Experiment 1A closely followed of those that were determined in the preregistration. Although Experiment 1B was not preregistered, the materials and procedure of the experiment were identical to those of Experiment 1A apart from the fact that we introduced two new blocks (33% and 45% incongruence proportion) and we excluded the 80% incongruence proportion block. The logic and steps of the analyses of Experiment 1B also closely followed those of Experiment 1A.

Results

Data transformation

Following Raz et al's (2002) procedure, we excluded trials with errors and missed trials (8.09% and 8.30% in Experiment 1A and B, respectively). In addition, trials with response times (RTs) 3 standard deviations either above or below the grand mean were considered as outliers and omitted from all the further analyses (1.45% and 1.45% in Experiment 1A and B, respectively). In order to directly test our main hypothesis (three-way interaction of interference, incongruence proportion of the block and suggestion), we computed variables of difference scores. We calculated the extent of the Stroop interference (mean RT of Incongruent trials - mean RT of Neutral trials) in each block. Next, to maximize the statistical power of our crucial test, we collapsed the data of Experiment 1A and 1B.³ To achieve this, first, we merged the blocks of PI 33% and PI 45% of Experiment 1B by calculating the average of the RTs in each block, so that both experiments contained only two blocks; one, in which the proportion of incongruent trials is deemed to be high (PI 80% block in Experiment 1A, and the merged block in Experiment 1B; henceforth, referred to as high incongruence proportion) and one in which it is considered low (PI 10% block in each experiment; henceforth, referred to as low incongruence proportion). We decided to produce one block from the blocks of PI 33% and PI 45% as the former one is the most frequently used design in word blindness studies and the

³ This step was not preregistered and we present the separate analysis of Experiment 1A and B in the Supplementary Materials ("Unmerged dataset" section). The pattern of the results are in accord with the pattern showed in the main paper but more tests are insensitive.

latter one had the highest proportion of incongruent trials in Experiment 1B, thus, we expected to find evidence for the word blindness effect in both of them. In addition, we compared the PI 33% and PI 45% blocks in terms of extent of the Stroop interference ($t(13) = 0.87$, $p = .400$, $M_{\text{diff}} = 20$ ms, $d_z = 0.24$, $B_{H(0, 31)} = 1.17$, $RR[0, 309]$) and the word blindness suggestion effects ($t(13) = 1.18$, $p = .257$, $M_{\text{diff}} = 34$ ms, $d_z = 0.32$, $B_{H(0, 64)} = 1.28$, $RR[0, 307]$) and we found no evidence for the difference in any of these cases. Finally, we collapsed the data of the two experiments so that the final dataset included the answers of 37 participants and the block variable had two levels: low and high incongruence proportions. Supplementary materials include the analyses of Experiment 1A and 1B; the pattern of the results is the same but more tests are insensitive.

Outcome neutral tests: Was there a Stroop interference effect and did the proportion of incongruent trials influence the extent of Stroop interference?

First, we confirmed that the latencies of the incongruent trials were the longest ($M = 887$ ms, $SD = 124$) followed by the neutral trials ($M = 790$ ms, $SD = 111$) and the RTs of the congruent trials were the shortest ($M = 707$ ms, $SD = 104$); see also panel A of Figure 2. There were Stroop interference ($t(36) = 8.58$, $p < .001$, $M_{\text{diff}} = 96$ ms, $d_z = 1.41$, $B_{H(0, 62)} = 9.79 \times 10^7$, $RR[5, 3.35 \times 10^4]$) and Stroop effects ($t(36) = 15.81$, $p < .001$, $M_{\text{diff}} = 180$ ms, $d_z = 2.60$, $B_{H(0, 90)} = 1.59 \times 10^{15}$, $RR[6, 6.49 \times 10^4]$). Furthermore, the extent of the Stroop interference effect was influenced by the type of the block ($t(36) = 1.81$, $p = .078$, $M_{\text{diff}} = 34$ ms, $d_z = 0.30$, $B_{H(0, 31)} = 3.14$, $RR[23, 39]$). Panel B of Figure 2 shows that the interference effect was larger in the low incongruence block ($M = 113$, $SD = 107$) than in the high incongruence block ($M = 80$, $SD = 66$). There was an interaction of the Stroop effect and the type of the block ($t(36) = 5.41$, $p < .001$, $M_{\text{diff}} = 87$ ms, $d_z = 0.89$, $B_{H(0, 45)} = 7.88 \times 10^3$, $RR[6, 2.73 \times 10^4]$). As panel C of Figure 3 depicts, the extent of the Stroop effect was larger in the low incongruence ($M = 223$, $SD = 106$) than in the high incongruence block ($M = 136$, $SD = 56$).

Crucial test: Is the suggestion more effective in the high incongruence than in the low incongruence block?

We examined the main question of interest by comparing the extent to which the suggestion reduced the interference effect in the high incongruence and low incongruence blocks. The results showed supporting evidence for the effect of the block ($t(36) = 2.05$, $p = .047$, $M_{\text{diff}} = 54$ ms, $d_z = 0.34$, $B_{H(0, 64)} = 4.21$, $RR[21, 116]$) as the interference was diminished more strongly by the suggestion in the high incongruence block ($M = 49$ ms, $SD = 107$) than in the low incongruence block one ($M = -5$ ms, $SD = 152$). Separate analysis of the two blocks revealed that the suggestion worked only in the high incongruence block ($t(36) = 2.77$, $p = .009$, $M_{\text{diff}} = 49$ ms, $d_z = 0.46$, $B_{H(0, 64)} = 14.54$, $RR[10, 420]$). There was moderate evidence that the suggestion did not lower the interference in the low incongruence block ($t(36) = -0.20$, $p = .841$, $M_{\text{diff}} = -5$ ms, $d_z = 0.03$, $B_{H(0, 64)} = 0.32$, $RR[61, \infty]$). Panel D of Figure 2 shows a violin plot of the word blindness effect broken down by the type of the proportion incongruence block. For an exploratory analysis on the influence of the suggestion on the facilitation effect (mean RT of neutral trials - mean RT of congruent trials) see the Supplementary Materials. Table 1 depicts the descriptive statistics broken down by suggestion conditions and proportion incongruence blocks.

Figure 3. Panel A represents the distribution of the RTs broken down by the congruency type of the Stroop trials indicating the size of the Stroop interference (difference of incongruent and neutral condition) and the Stroop effects (difference of incongruent and congruent conditions). Panel B and C depicts the extent of the Stroop interference and Stroop effects, respectively, separately for the high and low proportion incongruence blocks. Panel D shows the distribution of the extent of the word blindness effect in the high and low proportion incongruence blocks separately. The black dots on the plots indicate the average response of a particular participant.

Table 1.

Descriptive Statistics of the Response Times and Self-report Measures Broken Down by Suggestion Conditions and Proportion Incongruence Blocks

Category	Item (scale)	Low incongruence		High incongruence	
		No Suggestion	Suggestion	No Suggestion	Suggestion
Response times (RTs)	Incongruent (ms)	930 (184)	872 (174)	909 (153)	834 (145)
	Neutral (ms)	819 (140)	756 (133)	805 (141)	781 (136)
	Congruent (ms)	687 (114)	669 (94)	759 (151)	713 (120)

Expectations	Expecting the words to be meaningless (0-4)	0.32 (0.63)	1.35 (1.16)	0.34 (0.66)	1.08 (0.76)
Subjective experience of meaningfulness	Q1 (%)	85 (22)	62 (30)	85 (20)	55 (29)
	Q2 (%)	12 (20)	29 (25)	12 (21)	35 (27)
	Q3 (%)	14 (22)	33 (27)	17 (23)	40 (29)
	Q4 (%)	13 (23)	32 (29)	13 (18)	32 (27)
Depth of hypnosis	Experienced depth of hypnosis (0-3)	0.41 (0.64)	0.91 (0.74)	0.74 (0.44)	1.03 (0.80)

Note. The Standard Deviations (SD) of the means are shown within the brackets. Q1 is a reversed item compared to the other three subjective experience measures.

Supporting test 1: Does the suggestion, the type of the block and their interaction influence subjects' experiences of word meaningfulness?

The participants experienced more meaningless words in the suggestion than in the no suggestion condition for all four items (all Bs $> 1.58 \times 10^3$). There was moderate evidence for the absence of the main effect of block for all items (all Bs < 0.23). Finally, the test of the interaction between the type of the block and the suggestion showed moderate evidence for the null for Q2, Q3 and Q4 (Bs < 0.27) and indicated data insensitivity for Q1 (B equals to 0.50). Table 2 summarises the results of the statistical analyses.

Table 2.

Results of the t-tests Investigating the Effect of Suggestion, Block and their Interaction on Subjective responses of meaningfulness

Item	Predictor	Statistics					
		M_{diff} (%)	d_z	t	p	$B_{U[0, 100]}$	RR
Q1	Suggestion	27	0.96	5.87	$< .001$	2.85×10^4	$3, 7.95 \times 10^4$
	Block	4	0.21	1.26	.216	0.15	45, ∞
	Suggestion*Block	8	0.29	1.76	.087	0.50	16, 149
Q2	Suggestion	20	0.82	5.00	$< .001$	1.68×10^3	$3, 5.04 \times 10^4$
	Block	3	0.26	1.56	.128	0.16	49, ∞
	Suggestion*Block	6	0.25	1.50	.142	0.27	80, ∞
Q3	Suggestion	21	0.73	4.42	$< .001$	3.60×10^2	$4, 1.20 \times 10^4$
	Block	4	0.26	1.59	.122	0.23	68, ∞
	Suggestion*Block	4	0.14	0.86	.400	0.13	41, ∞
Q4	Suggestion	19	0.72	4.38	$< .001$	2.93×10^2	$3, 9.76 \times 10^3$
	Block	0	-0.01	-0.09	.930	0.03	10, ∞
	Suggestion*Block	0	0.02	0.10	.925	0.06	19, ∞

Supporting test 2: Does the suggestion affect subjects' expectations?

The participants expected to have a stronger experience of word meaninglessness in the suggestion condition ($M = 1.22$, $SD = 0.82$) than in the no suggestion condition ($M = 0.33$, $SD = 0.54$), ($t(36) = 5.48$, $p < .001$, $M_{diff} = 0.89$, $d_z = 0.90$, $B_{U[0,4]} = 7.71 * 10^3$, $RR[0.11, 2.51 * 10^3]$).

Exploration 1: Is there any relationship between expectations and the subjective experiences of the suggestion?

In order to explore whether the expectations to experience the words as meaningless characters can predict participants' subjective experience of meaninglessness, we conducted linear regression analyses. We computed the raw regression slope in the suggestion and no suggestion conditions separately as well as the slope of the difference scores (Expectation_{Suggestion} - Expectation_{No Suggestion} as a predictor and Subjective experience_{Suggestion} - Subjective experience_{No Suggestion} as an outcome). In the no suggestion condition, the slope differed from zero for all items (all B s > 5.56 and all b s > 12.28). In the suggestion condition there was strong evidence for Q1, Q2 and Q3 (B s > 68.66 and b s > 15.43) and the results were insensitive for Q4 ($B = 0.87$, $b = 5.84$). Lastly, the analysis of the difference scores revealed a positive relationship between expectations and subjective experiences for all four items (B s > 63.00 and b s > 13.24). Table 3 depicts the results of the regression analyses.

Table 3.

Results of the Regression Models Predicting the Experience of Word Meaninglessness Based on the Subjects' Expectations

Item	Condition	Statistics					
		b (%)	β	t	p	$B_{U[0, 25]}$	RR
Q1	No Suggestion	27	0.79	7.53	$< .001$	$4.34 * 10^6$	$3, 8.7 * 10^4$
	Suggestion	20	0.60	4.41	$< .001$	$1.17 * 10^3$	$3, 1.11 * 10^4$
	Difference	20	0.71	5.93	$< .001$	$9.34 * 10^4$	$3, 5.97 * 10^4$
Q2	No Suggestion	12	0.35	2.19	.035	5.56	7, 47
	Suggestion	17	0.58	4.19	$< .001$	$6.28 * 10^2$	$3, 5.39 * 10^3$
	Difference	14	0.59	4.35	$< .001$	$8.14 * 10^2$	$3, 6.79 * 10^3$
Q3	No Suggestion	20	0.53	3.70	$< .001$	$1.72 * 10^2$	$4, 1.76 * 10^3$
	Suggestion	15	0.49	3.35	.002	68.66	4, 585
	Difference	20	0.67	5.36	$< .001$	$1.76 * 10^4$	$3, 5.45 * 10^4$
Q4	No Suggestion	15	0.42	2.73	.010	17.14	5, 148
	Suggestion	6	0.19	3.30	.26	0.87	0, 65
	Difference	13	0.50	3.40	.002	68.69	3, 573

Note. The raw slopes are indicated by b , whereas the standardised effect sizes are indicated by β .

Exploration 2: Is there any relationship between the objective responses and the expectations of the participants?

Next, we investigated the link between expectations and objective responses, specifically, the extent of the interference effect. Similar to the previous analysis, we calculated the raw regression slopes in the suggestion and in the no suggestion conditions as well as for their interaction by running the analysis on the difference scores. The test of the slopes against zero

revealed insensitive evidence in all cases: for the no suggestion condition ($t(36) = 1.20$, $p = .237$, $b = 37$ ms, $\beta = 0.20$, $B_{U[0, 24]} = 1.47$, $RR[0, 429]$); for the suggestion condition ($t(36) = 0.81$, $p = .422$, $b = 11$ ms, $\beta = 0.14$, $B_{U(0, 24)} = 0.42$, $RR[0, 30]$); and for the interaction of the conditions ($t(36) = 1.41$, $p = .168$, $b = 25$ ms, $\beta = 0.23$, $B_{U[0, 24]} = 1.99$, $RR[0, 328]$).

Exploration 3: Is there any relationship between the subjective and objective responses of the participants?

The logic of the analysis of the question whether the subjective reports of meaninglessness can predict the extent of the Stroop interference effect in the different conditions closely followed that of Exploration 1 and 2. The regression slopes in the no suggestion condition suggest that seeing more words as meaningless characters predict faster RTs (note that positive b s indicate reduction in RTs owing to the increase of the experience of meaninglessness), whereas the slopes in the suggestion condition and the slopes calculated from the difference scores depicted mixed results. The statistical analyses showed that the evidence is not enough in any case to come to a decision whether the experience of word meaninglessness can predict RTs. Table 4 summarises the results of the regression models.

Table 4.

Results of the Regression Models Predicting Response Times Based on Subjects' Experience of Word Meaninglessness

Item	Condition	Statistics					
		b (ms)	β	t	p	$B_{U[0, 1]}$	RR
Q1	No Suggestion	-0.12	-0.02	-0.13	.896	0.78	0, 3.17
	Suggestion	-0.55	-0.22	-1.31	.200	0.25	0.74, ∞
	Difference	-0.29	-0.08	-0.47	0.64	0.53	0, 1.73
Q2	No Suggestion	0.17	-0.03	-0.19	.851	0.91	0, 3.96
	Suggestion	0.04	-0.01	-0.09	.931	0.61	0, 1.93
	Difference	0.23	0.05	0.31	.760	0.92	0, 3.65
Q3	No Suggestion	0.98	0.20	1.21	.234	1.68	0, 11.36
	Suggestion	-0.32	-0.12	0.71	.480	0.35	0, 1.04
	Difference	0.30	0.08	0.49	.630	0.97	0, 3.60
Q4	No Suggestion	0.69	0.13	-0.78	.442	1.26	0, 7.08
	Suggestion	-0.20	-0.07	-0.43	.670	0.42	0, 1.27
	Difference	-0.32	-0.08	-0.47	.643	0.58	0, 1.83

Note. The raw slopes are indicated by b , whereas the standardised effect sizes are indicated by β .

Discussion

In the current study, we manipulated the proportion of incongruent Stroop trials across blocks, in order to influence the level of conflict and so to test whether the activation of the word blindness suggestion requires a minimum level of conflict. Our results revealed that highly suggestible people can reduce the Stroop interference effect (respond to the suggestion) to a greater extent in a Stroop task with high levels of conflict (high incongruence block), compared to a Stroop task with low levels of conflict (low incongruence block). This finding supports the

response competition model (Augustinova & Ferrand, 2012; Parris, Dienes, & Hodgson, 2013) of the word blindness suggestion, over the de-automatisation of reading model (Raz et al., 2002, 2006; Raz & Campbell, 2011). The former model predicts that a minimum amount of conflict is necessary for the activation of the suggestion, whereas the latter assumes that the operation of the suggestion is independent of the registered conflict.

Given the assumption of cold control theory (Barnier, Dienes & Mitchell, 2008; Dienes, 2012; Dienes & Perner, 2007) that the sole difference between hypnotic and non-hypnotic responding is the form of the accompanying HOT of intending, our findings imply that conflict plays a crucial role in the activation of unconscious control. This indicates that the conflict-monitoring model (Botvinick et al., 2001; Botvinick, Cohen, & Carter, 2004) might be generalizable for unconscious control and the awareness of the intention may not be a requisite of this monitoring process. Coinciding with this view, Parris, Dienes, & Hodgson (2012) found that the word blindness suggestion is subject to the same time constraints as the CSE (Egner, Ely & Grinband, 2010), namely, shorter Response-Stimulus Interval (RSI, 500 ms) results in a stronger suggestion effect (stronger reduction in the interference effect) than a long RSI (3500 ms). To test the similarity of the mechanisms underlying the activation of regular control processes and the hypnotic response, they tested whether the congruency type of a trial can modulate the efficiency of the suggestion on a subsequent trial. They found no evidence for the idea that the suggestion reduces interference more strongly after an incongruent trial than after a neutral or congruent one. Nevertheless, the analysis of the traditional CSE effect revealed no evidence in either direction, implying that the design itself was not sufficient to test the effect of preceding conflict on the activation of control. Indeed, it has been demonstrated that in Stroop tasks that employ four colors and color words the emergence of a CSE effect is unlikely (Puccioni & Valessi, 2012). Future research could shed more light on whether hypnotic responding and so unconscious control can be triggered by recently registered conflict as well or it is only subject to the relative frequency of conflicting trials.

The application of multiple experimental manipulations that affect the extent of the Stroop interference effect in tandem (i.e., the proportion of incongruent trials within a block and the word blindness suggestion) can necessitate a different analysis plan than a simple calculation of difference scores. Blocks with low proportion of incongruent trials tend to have a greater baseline of Stroop interference than blocks with high proportion of incongruent trials (PCE). Therefore a three-way interaction with difference scores can be found even when the suggestion, for instance, halves the interference effect in both of the blocks. In this case, a difference score analysis would give the false impression that the suggestion is more effective in the low incongruence than in the high incongruence block. Using ratio rather than difference scores overcomes this issue as the test of the ratio scores would only be sensitive if the suggestion is proportionately more effective in one block compared to the other (e.g., interference reduces to its half in one block and to its quarter in the other block). Nonetheless, it worked out in our case that the ratio score analysis yielded the same conclusion as the difference score analysis, because the largest differences occurred for the smallest baselines.. Thus, we reported the classical difference score analysis in the main paper so that our results can be directly contrasted to other studies of the word blindness effect and the dependent

variable can be unambiguously interpreted (See the Supplementary Materials for the analysis of the ratio scores). Choosing one analysis specification over another can increase the researchers' degrees of freedom exacerbating the rate of false-positive findings (Simmons, Nelson & Simonsohn, 2011). Multiverse analyses (Steegen, Tuerlinckx, Gelman & Vanpaemel, 2016) and specification curves (Simonsohn, Simmons & Nelson, 2015) can reduce the possibility of selective reporting and enhance the credibility of reported findings through an increased transparency of the consequences of the analytic choices.

The participants' subjective reports of word meaningfulness differed in the suggestion and no suggestion conditions but, interestingly, they did not discriminate between the high and low conflict environments, implying that the subjective experience of meaningfulness is unrelated to the level of Stroop interference (see also Parris, Dienes & Hodgson, 2012), which further corroborates the response competition model. Nonetheless, these results proved to be dependent on the shape of the distribution used to model the predictions of H1, and the evidence for the null is insensitive when a plausible model favouring small effect sizes is contrasted to H0 (Table S2, Supplementary Materials). Moreover, the direct tests of the link between the subjective experiences of meaningfulness and the objective scores of the Stroop interference reductions were all insensitive (with both uniform and half-Cauchy distributions) precluding any robust conclusion about the relation between the subjective experience of meaningless and Stroop reduction. Demonstrating support for the null hypothesis has proven to be challenging for experimental psychologists as the classical procedure of null hypothesis significance testing cannot provide evidence for the null even for a relatively large p-values (Fisher, 1935, Royal, 1997). Despite this, non-significant tests are still frequently used to support claims about the absence of effects (Aczel et al., 2018). The application of the Bayes factor overcomes this issue by allowing us to assess the relative strength of evidence for two competing models (Dienes, 2014, 2016), however, B depends on the shape and parameters of the distribution chosen to represent the predictions of H1 and H0 (Dienes, 2017; Rouder et al., 2016; Rouder, Morey & Wagenmakers, 2016). For instance, putting too much weight on large effect sizes when modelling the predictions of H1 could result in evidence for H0, but it is evidence against only a vague model of H1, which precludes inference about theories (when there is information for the theory to be more precise). Thus, to make theoretically-motivated inferences based on B s, one needs to ensure that the predictions of the theories under comparison are represented fairly by the models. This is the main reason why we refrain from drawing specific conclusions based on the B s calculated with uniform distribution as a model of H1s.

The impact of expectations

The response expectancy theory (Kirsch, 1985, 1997, Kirsch & Lynn, 1997; Lynn, 1997), emphasise the causal role of expectancies in the activation and modulation of responses (e.g., subjective experiences, behaviors) to suggestions. Our results are in line with this theory insofar as participants expected to see more meaningless words in the suggestion condition, compared to the no suggestion condition. Moreover, the difference of the expectations strongly predicted the difference of the subjective experiences of meaningfulness between suggestion and no suggestion conditions (for all four items), implying that the participants were able to produce the anticipated subjective responses.

The evidence for the relationship between the expectations to see meaningless words and the capacity to reduce the interference effect was not decisive, since all regression analyses were insensitive. Nonetheless, it is important to note that our expectancy measures assessed word meaningfulness and not the experience of increased control. This difference might prove to be crucial as the current study provided further evidence for the notion that enhanced control over response competition, and not the de-automatisation of reading, is responsible for the reduction of the interference effect. Thus, future studies should consider including a new item measuring the expected ease of naming the color of the words during the Stroop task. Acquiring sensitive data here would help us understand whether placebo-suggestions and imaginative suggestions can both modulate behavior by influencing expectancies (Kirsch, 1985, 1999). For instance, a recent study demonstrated that the Stroop interference measured as the difference between the accuracy on incongruent and neutral trials can be modulated by a placebo-suggestion (persuading people that in the placebo condition the accuracy of their color discrimination will be enhanced), whereas an imaginative suggestion (a suggestion to imagine the ability of having an improved color perception) worded akin to the placebo-suggestion did not modulate the Stroop interference (Magalhães De Saldanha da Gama, Slama, Caspar, Gevers, & Cleeremans, 2013). These results seemingly count against the response expectancy theory, however, the participants in this study were not screened for hypnotisability and so only a fraction of them were highly suggestible; and it has been shown that only highs and mediums can respond to imaginative suggestions that help them enhance the resolution of response competition (Parris & Dienes, 2013). Therefore, additional inquiry is needed to address whether expectancies are reliable predictors of responses (i.e., performance on the Stroop task) not just to placebo-suggestions but to imaginative suggestions as well.

Limitations

It is to be noted that the current study used a specific sample, namely highly suggestible individuals, and so the finding that unconscious control can be triggered by conflict may not be generalizable to non-highs. In addition, we applied a single interference task and suggestion, which may also restrict the generalizability of our findings and so future research could aim to implement conceptual replications of our study by employing suggestions claimed to reduce the Flanker compatibility (Iani, Ricci, Gherri, & Rubichi, 2006), the Simon (Iani, Ricci, Baroni, & Rubichi, 2009) or the McGurk effects (Lifshitz, Bonn, Fischer, Kashem, & Raz, 2013).

The most distinctive feature of a hypnotic response is the experience of involuntariness (Weitzenhoffer, 1980) and this sensation is also the key element of an unconscious control process (Dienes, 2012; Dienes & Perner, 2007). Although in the presented experiments, we did not gauge directly whether our participants were aware of their intentions to impair the meaningfulness of the words, in another study of ours, we have demonstrated that highs report reduced control over how meaningful the words appear to them when the word blindness suggestion was activated compared to the case when it was not (Palfi, Parris, McLatchie, Kekecs & Dienes, 2018). This finding underscores the idea that the implementation of this suggestion is accompanied by an inaccurate HOT about the intention and it is in accord with the interpretation of hypnotic responses to the word blindness suggestion as unconscious control processes. Future studies using (post)hypnotic suggestions as tools to examine

unconscious control processes should consider collecting subjective reports measuring the extent to which participants experience involuntariness during the execution of the hypnotic response (e.g., Polito, Barnier, & Woody, 2013; Polito, Barnier, Woody, & Connors, 2014).

Conclusion

In sum, the moderating effect of conflict on the operation of the word blindness suggestion demonstrates compelling evidence for the notion that the suggestion takes its effect on response competition rather than on semantic processing. This finding supports also the idea that conflict plays a crucial role in the activation of the word blindness suggestion by enhancing the recruitment of control processes. Hence, given the assumption that hypnotic responding is intentional and it is accompanied by the feeling of involuntariness, this study is the first to indicate that cognitive control with and without the awareness of intentions can be activated by the very same process, namely, the monitoring and registration of conflict as outlined in the conflict-monitoring theory (Botvinick et al., 2001; Botvinick, Cohen, & Carter, 2004). This explanation is in line with the view about the strategic nature of hypnotic responding (Spanos & Barber, 1974; Comey & Kirsch, 1999; Hilgard, 1977; Spanos, 1986) as it depicts a model in which the extent of the exerted control is determined by the demands of the environment, namely, by the difficulty of the current proportion incongruence block. Of interest is that the present results suggest that the suggestion does not operate according to the instructions in the suggestion itself. Participants are processing the words to the level of meaning acquisition and even if they report to perceive them as words of a foreign language or as gibberish, this experience of meaninglessness is not a consequence of a deteriorated reading ability. Instead, participants seem to understand the desired outcome and unconsciously simulate that outcome by operating over existing control mechanisms conferring a level of control in the Stroop task difficult to achieve by conscious means.

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Appendix A. Items of Experiment 1A and 1B

Expectations

How strongly do you expect to experience the words as at least somewhat meaningless?

1. I know the meaning of the words on the screen will be completely clear to me
2. I am guessing that the meaning of the words on the screen will in some way be unclear
3. I am fairly sure that the meaning of the words on the screen will in some way be unclear
4. I am almost certain that the meaning of the words on the screen will in some way be unclear
5. I am certain that the meaning of the words on the screen will in some way be unclear

Subjective experience questions

On what percentage of the trials:

1. Was the meaning of the words on the screen completely clear to you?
2. Were you aware of only an unclear meaning of the words on the screen?
3. Were you just aware of the colour and had no idea of what script the words were written in?
4. Were the words on the screen written in a clear yet completely meaningless script?

Depth of hypnosis scale

How deeply hypnotised were you during that game (Stroop task)?

1. Normal state
2. Relaxed
3. Hypnotized
4. Deeply hypnotized

Appendix B. R script to calculate the Bayes factor

The following R script integrates the scripts of the Dienes and McLatchie (2018) and the Dienes (2008) Bayes factor calculators into a single R function that can calculate B_s with normal and t likelihood function; and it can model the predictions of H_1 with either a uniform, a normal, a t or a Cauchy distribution. Moreover, the R script allows to calculate B_s with one-tailed models that have a non-zero mode.

```
Bf<-function(sd, obtained, dfdata = 1, likelihood = c("normal", "t"), modeloftheory=
c("normal","t","cauchy", "uniform"),lower =0, upper=1, modeoftheory = 0, scaleoftheory = 1,
dftheory = 1, tail = 2)
{
  if(likelihood=="normal"){
    dfdata=10^10
  }
  if(modeloftheory=="normal"){
    dftheory = 10^10
  } else if(modeloftheory=="cauchy"){
    dftheory = 1
  }
  area <- 0
  normarea <- 0
  if(modeloftheory=="uniform"){
    theta <- lower
    range <- upper - lower
    incr <- range / 2000
    for (A in -1000:1000){
      theta <- theta + incr
      dist_theta <- 1 / range
      height <- dist_theta * dt((obtained-theta)/sd, df=dfdata)
      area <- area + height * incr
    }
    LikelihoodTheory <- area
  }else{
    theta <- modeoftheory - 10 * scaleoftheory
    incr <- scaleoftheory/200
    for (A in -2000:2000){
      theta <- theta + incr
      dist_theta <- dt((theta-modeoftheory)/scaleoftheory, df=dftheory)
      if(identical(tail, 1)){
        if (theta <= modeoftheory){
          dist_theta <- 0
        } else {
          dist_theta <- dist_theta * 2
        }
      }
    }
  }
}
```

```

    }
  }
  height <- dist_theta * dt((obtained-theta)/sd, df = dfdata)
  area <- area + height * incr
  normarea <- normarea + dist_theta*incr
}
LikelihoodTheory <- area/normarea
}
Likelihoodnull <- dt(obtained/sd, df = dfdata)
BayesFactor <- LikelihoodTheory/Likelihoodnull
BayesFactor
}

```