Consciousness and Cognition 132 (2025) 103887

Contents lists available at ScienceDirect

Consciousness and Cognition

journal homepage: www.elsevier.com/locate/yccog

Registered Report Stage II

Social cues for experimenter incompetence influence choice blindness

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ARTICLE INFO

Keywords:
Choice blindness
Social cue context
Experimenter competence
Confabulation

ABSTRACT

Choice blindness refers to a surprising blind spot we have about choices made only seconds ago. After making a choice between two items, observers presented with the unchosen item may fail to report the incongruence, and even provide justifications for a choice they did not make. Here, we show that this effect is modulated by participant's perception of the reliability of the environment. In three experiments, we introduced cues about the competence or incompetence of experimenters, either during or before the traditional choice blindness phase. When manifest reliability of the experimenter decreased, participants were more likely to report the mismatch between the chosen item and the item presented to them. Our results reinforce the notion that choice blindness is a context-dependent phenomenon, permeable to social cues in the context of psychological experiments. Dataset and the analysis scripts are available at the Open Science Foundation at: https://osf.io/ht769/.

1. Introduction

Choice blindness (Johansson, Hall, Sikström & Olsson, 2005) refers to people's inability to consciously monitor their decisions, and access the reasons motivating their own choices. In the original version of the choice blindness protocol, Johansson et al. (2005) asked participants to select which of two pictures of faces they preferred. Then, the researcher handed participants the selected face and asked them to justify their decision. In critical trials the researcher, with a sleight of hand, handed back the non-selected face. Only 13 % of the participants detected the card change (Johansson et al., 2005; Johansson, Hall, Tärning, Sikström & Chater, 2014). Even more surprisingly, the remaining 87 % readily volunteered justifications for a decision that they had not originally made.

The dominant understanding of this inability to track the outcome and the true reasons for the original choice is that it reveals an introspective blindspot. According to a tradition that dates back at least to Nisbett and Wilson (1977), cognitive processes themselves are opaque to introspection, and we gain conscious access only to the subjective by- and end- products of our decisions. Thus can we be

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https://doi.org/10.1016/j.concog.2025.103887
Received 27 November 2024; Received in revised form 13 May 2025; Accepted 13 May 2025
Available online 16 May 2025
1053-8100/

aware of the *amount of effort* invested in a decision (Naccache, Dehaene, Cohen, Habert, Guichat-Gomez, Galanaud & Willer, 2005) or its *duration* (Corallo, Sackur, Dehaene & Sigman, 2008; Miller, Vieweg, Kruize & McLea, 2010). We can also express our *confidence* in a decision (Peirce & Jastrow, 1884), as the subjective adequacy between the end-product and the intention of the decision. But we cannot access the processes of the decision itself (Reyes & Sackur, 2014, 2017). Blindness to one's own choice fits in with this tradition, as it seems to be a remarkable case of obliviousness to the consequences of a personal decision combined with some propensity to confabulate the reasons for said decision.

Since the first report of choice blindness by Johansson et al. (2005), researchers have been delineating its domain of application both in terms of contexts (*legal*: Sagana, Sauerland & Merckelbach, 2013; *economic*: McLaughlin & Somerville, 2013; *clinical*: Aardema, Johansson, Hall, Paradisis, Zidani & Roberts, 2014; *ethical*: Hall, Johansson & Strandberg, 2012; and *political*: Hall, Strandberg, Pärnamets, Lind, Tärning, & Johansson, 2013), and in terms of the sensory modalities wherein it can occur (*chemical senses*: Hall, Johansson, Tärning, Sikström & Deutgen, 2010; *audition*: Lind, Hall, Breidegard, Balkenius & Johansson, 2014; and *touch*: Steenfeldt-Kristensen & Thornton, 2013) and studies have shown that choice blindness is not just a laboratory phenomenon but also occurs outside the lab (Hall, Johansson, Tärning, Sikström & Deutgen, 2010; Sagana, Sauerland, & Merckelbach, 2013).

In parallel, some effort has been devoted to discovering the causes of choice blindness. Stimulus-level factors (exposure duration, similarity between the two faces) seem to moderate the effect (Johansson et al., 2005), whereas the self-relevance of the stimuli does not (Sauerland, Sagana, Otgaar & Broers, 2014, but see Somerville & McGowan, 2016). Individual differences in traits such as social desirability and compliance have consistently failed as explanatory factors (Sauerland, Sagana & Otgaar, 2013, Sagana, Sauerland & Merckelbach, 2014, Sauerland, Sagana, Siegmann, Heiligers, Merckelbach & Jenkins, 2016; Strandberg, Sivén, Hall, Johansson & Pärnamets, 2018). Thus, choice blindness stands out as a robust and general effect without obvious explanations. Yet, there is some evidence that higher confidence in the decision (Sauerland et al., 2016) predicts higher detection rates in a choice blindness protocol. This suggests that in order to account for choice blindness, one would need to factor in the internal uncertainty of the initial choice. Although hidden to participants, the main task in a choice blindness paradigm is, in fact, a match-to-sample task. It thus relies on a perceptual decision, much like in change detection tasks, where it is known that the ability to detect changes depends on the task relevance of the feature that is changed, at the time of the change (e.g. Triesch, Ballard, Hayhoe, & Sullivan, 2003). The current thinking in the field of perceptual decision-making emphasizes the role of attention and expectations (Summerfield & Egner, 2009), and these ideas have been formalized in the Bayesian framework (Cf., Vogel, Pärnamets, Hall, & Johansson, 2023), according to which perception is the optimal combination of prior information with sensory inputs (Kersten, Mamassian & Yuille, 2004). Recently, this framework has been extended to decisions in social contexts (Toelch & Dolan, 2015). Indeed, when participants are instructed to make a decision, they would need to combine their internal noisy signal with the information dispensed by others, if relevant. In particular, Toelch and Dolan (2015) revisited the classical Asch (1956) experiment, in which observers sometimes state that a visual line is shorter than another (although it clearly is not) after hearing confederates making such (incorrect) statements. This seemingly irrational conformity could be in fact optimal in the sense that, having a strong prior on the group's accuracy, participants rationally follow the group's decision rather than their own senses.

Along these lines, we reasoned that low detection rates in choice blindness paradigms might reflect the combination of internal uncertainty concerning the choice, with the prior belief that in the context of this experiment, substitutions are not expected to occur. In other terms, we put the choice blindness paradigm in the social context of the psychology experiment (Orne, 1962), where participants have a strong prior concerning the infallibility of the experimenters, and a low motivation for openly questioning their competence. In fact, we note that Johansson and colleagues (2005) already suggested that choice blindness would disappear if participants were to expect a change and instructed to detect it: "Had our experiment been framed as an explicit detection task, we have no doubt that most participants would have been able to spot the manipulations immediately." Our goal in the present study is to examine how the choice blindness phenomenon, that is, the failure to spontaneously report choice substitutions introduced by the experimenter, may depend on participants' expectations regarding the reliability of experimenters. Specifically, we predicted that attenuating the cues related to the reliability of experimenters (*Cf.* Penner, Hawkins, Dertke, Spector & Stone, 1973; Kidd, Palmeri & Aslin,

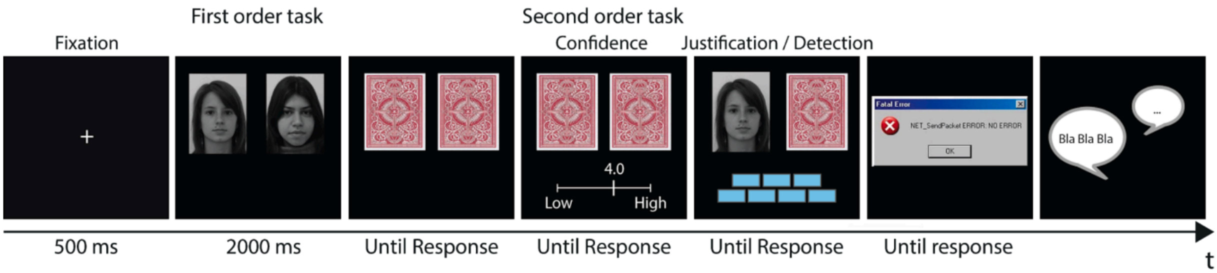

Fig. 1. Structure of the task in Experiment 1. Two faces are shown to participants and they are asked for their preference. Immediately afterward, they report their confidence on a visual analog scale. The face chosen is shown again so they can justify their decision with 7 predefined characteristics. A detection button (*I chose the other one*) is also presented. In the manipulated trials (7, 10 and 14) participants are asked to justify the other face than the one originally chosen. In Experiment 1, between trials 8 and 9, a Windows OS fictitious error appears, so as to simulate an error in the experiment code. All participants asked the experimenter for help. At that point, the scripted interaction with the confederate researcher begins.

2013) would increase detection rates. A similar prediction was put forward in the study by Sagana, Sauerland, and Merckelbach (2018), in which participants took part in an eyewitness identification procedure and received an explicit warning. In the pre-warning condition, the instruction that errors might occur during the procedure was presented before the manipulated identification outcome. In contrast, in the post-warning condition, this instruction was provided after the outcome. Interestingly, the authors did not find significant effects of warning type on detection rates, but they did find that pre-warning instructions may lead to faster detection responses.

In our studies, we examine again the issue of how information regarding the experimental context may influence choice blindness. We explore a diverse set of situations, in which participants were not explicitly told that errors may occur in the experimental procedure but instead received contextual cues regarding the reliability or competence of the experimenters. We hypothesized that such cues would affect the likelihood of participants detecting a change.

To test this hypothesis, in Experiment 1, we inserted an unexpected error screen in the protocol (Fig. 1), as an anchor for an interaction between participants and the experimenter, during which the experimenter appeared either competent or incompetent when solving the technical problem. In Experiments 2 and 3, we strictly adhered to the traditional choice blindness paradigm while implementing a more remote manipulation of competence, by manipulating prior expectations about the reliability of experimenters, before the start of the experiment. In Experiment 2, the interaction was with a confederate participant in the waiting room, with the intention of generating either high or low expectations regarding the overall professionalism of the laboratory, without any allusion to definite experiments. In Experiment 3 which was conducted online, we manipulated the welcome message that participants read prior to the instructions, to convey high or low expectations regarding the competence of the experimenter.

2. Experiment 1

2.1. Materials and Methods

2.1.1. Participants

77 university students without training in experimental psychology participated in the experiment. There were 26 participants in the competent condition (age: M = 22.2, SD = 2.8, 10 women), 27 participants in the incompetent condition (age: M = 21.5, SD = 1.7, 16 women) and 24 participants in a control group (age: M = 21.6, SD = 2.1, 13 women). Note that a prior study by Petitmengin et al. (2013) found that detection rates in the choice blindness paradigm can vary between 33 % and 80 % depending on the specific report procedure used by experimenters. A power analysis (conducted with G*Power v3.1) indicated that, to detect the difference in detection rates reported by Petitmengin et al. (2013) between conditions with and without an elicitation interview (80 % vs. 33 %, respectively), an estimated sample size of 50 participants would be required. This was calculated using the reported value of Cramér's V2 = 0.35, assuming that Cohen's w is approximately equal to V^2 such that $w \approx \sqrt{V^2}$, with 95 % statistical power and a 5 % alpha error rate.

Informed consent forms were signed and the guidelines of the Code of Ethics of the World Medical Association and the Declaration of Helsinki were completely fulfilled. Thus, the University ethics committee approved the experiment. At the end of the experiment, 200 USD were raffled among participants, as compensation for their work. Initially, participants were informed that the aim of the experiment was to investigate facial preferences. Students from the Faculty of Psychology were not selected. Once the experiment was finished, the participants were informally probed regarding the choice blindness protocol. No participant reported having knowledge of the experimental protocol nor of the literature around the choice blindness phenomenon. Participants were fully debriefed regarding the real aim of the experiment. They were also probed for suspicion with respect to the fake error and the interaction with the confederate-researcher. No participant reported noticing the presence of a confederate, or suspicion regarding the error screen. Dataset and the analysis scripts are available at the Open Science Foundation at: https://osf.io/ht769/?view_only=5b8fce39872342f0a3b87f6b9ebac8b4.

2.1.2. Stimuli and procedure

Stimuli consisted of 15 pairs of images representing women's faces, presented on a uniform black background. The faces were obtained from the FEI Face Database, free of copyright for research use (https://osf.io/ht769/). These 15 pairs were selected from pilot testing with 30 university students, where participants were presented with 50 pairs and rated the similarity between the two faces for each pair. We then selected the 15 pairs with the lowest similarity ratings on average (see Table SM-I in the Supplementary Material, for the final selection of all pairs of faces). None of the survey participants was selected for the later experiments. Only the pairs ranked in the category of low similarity were selected for the critical trials. A CRT screen was used (size 17″, resolution of 1024 X 768, refresh rate of 60 Hz, viewing distance ~55 cm). The experiment was run on standard desktop computers running Microsoft Windows (see Johansson et al., 2007; Pärnamets, et al., 2015, for computer-based CB procedures).

Regarding the experimental task, after a fixation point (500 *ms*), two faces were shown for 2000 *ms*. After this, the backs of the cards were shown. Then, participants were asked – without time pressure – to click on the card they preferred. Once the response was made, participants were asked – without time pressure – to report a confidence judgment (*Please, indicate the level of confidence in your preference?*) on a scale from 0 (completely random) to 6 (certain). The selected card was then shown a second time, together with 7 buttons ("*Mouth*", "*Eyes*", "*Nose*", "*Shape*", "*Proportions*", "*Accessories*", and "*I chose the other card*") that participants had to use to justify their decision (*Please, justify your preference by selecting one of these buttons*). Note that no specific reason for the presence of the "detection" button (i.e. "*I chose the other card*") was mentioned to participants. After participants selected one justification by clicking on the corresponding button, the experiment continued with the following trial for a total of 15 trials. Critically, in some trials (trials 7, 10 & 14) the non-selected card was shown. Thus, in these critical trials, participants could either provide a justification for the face that they had not originally chosen, or select the detection button ("*I chose the other card*"), indicating awareness of the experimental manipulation.

Our critical manipulation was as follows. Between trials 8 and 9, a fictitious "Windows error" screen appeared, so as to simulate an error in the experiment code which would trigger a message from the computer operating system. Importantly, at no point where participants forewarned about this alleged bug. All participants left the experimental booth and asked the investigator for help. A confederate researcher (professional actor: 30-year-old man, without knowledge of psychology, without knowledge about the choice blindness effect, nor about the operational hypothesis) intervened to solve the issue, and behaved so as to create either the Competent or the Incompetent conditions, in a scripted fashion. In the Competent condition, the "experimenter" stated his competence with respect to solving the error, and behaved as a self-assured person. In the Incompetent condition, the "experimenter" did the opposite and notably stated that he did not have sufficient skills to deal with this kind of error (see SM-II in the Supplementary Material for the dialog script). In both conditions, after 3 min of interaction, the confederate researcher typed a command, so that the experiment resumed its normal course. We also included a Control condition with no error message, and consequently no intervention of the "experimenter".

2.1.3. Statistical analysis

Detection responses (i.e. whether participants selected "I chose the other card" or not) were analyzed using generalized linear mixed models (GLMM, *binomial* probability distribution, with *logit* link function, fit by maximum likelihood). We also ran multiple and independent linear mixed models (LMMs) on each other measurement of interest (detection-RTs, preference-RTs, and confidence). In these regressions, we considered experimental groups (competent, control & incompetent), and trial rank for the critical trials (before the manipulation vs. after the manipulation, that is, trial 7 vs. trials 10 and 14), as well as their interaction, as fixed factors. Participants' intercepts were included as a random effect. Statistical analyses were performed with the statistical software R, using the *lme4* package (Bates et al., 2015), and using unstructured variance–covariance components for the LMM and GLMM analyses. Results reported did not differ by modifying covariance structures. Given the covariance type, we implemented the *Satterthwaite* method to estimate degrees of freedom. To compare detection rates across groups, we also used one-sided χ^2 tests with the hypothesis that detection rates would be higher in the incompetent group than in the control, in higher in the control than in the competent group. As a measure of effect size in logistic regression models, odds ratios (OR) were reported.

2.2. Results

2.2.1. Detection & detection-RTs

Fig. 2 illustrates detection rates in the 3 critical trials in the 3 experimental conditions. Overall, only one in five substitutions were detected, but this rate was different across conditions: participants in the competent condition detected less substitutions (14 %) than in the control (18 %) condition, who in turn detected fewer substitutions than in the incompetent condition (28 %). Critically, we predicted that our 3 groups would behave similarly on critical trial 7 that occurred before the experimental manipulation, but differently on critical trials 10 and 14 that occurred after the experimental manipulation. We therefore conducted a binomial GLMM regression on detection, with experimental group (competent, incompetent, control), trial rank (before vs. after the manipulation, i.e. trial 7 vs. 10 and 14), and their interactions as fixed effects. Results indicated a main effect of experimental group ($b = 0.87$, $SE = 0.36$, $z = 2.45$, $p = 0.01$, OR = 2.40), no main effect of trial rank ($b = 1.37$, $SE = 1.15$, $z = 1.19$, $p = 0.23$, OR = 3.93), and most critically an

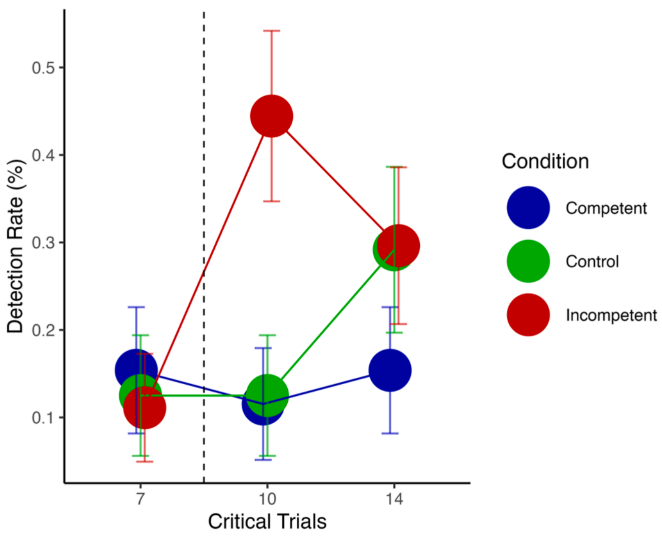

Fig. 2. Detection rates, calculated for each experimental group and critical trial (7, 10 & 14) for Experiment 1. The dashed line notes the insertion of the experimental manipulation (i.e., windows error) which occurs between trials 8 and 9. Error bars show the group means ± 1 standard error (*SE*).

interaction between these two factors (b = -1.11, SE = 0.54, z = -2.04, p = 0.041, OR = 0.33). Separate tests further indicated that detection rates did not differ between groups before the “Windows error” screen and the interaction with the experimenter/actor (critical trial 7: χ^2 (2, n = 77) = 0.22, p = 0.89; Competent = 15 %, Incompetent = 11 %, Control = 13 %), but differed immediately after the manipulation (critical trial 10: χ^2 (2, n = 77) = 10.31, p = 0.006; Competent = 11 %, Incompetent = 44 %, Control = 13 %), with higher detection rates for the incompetent condition compared to the competent condition (χ^2 (1, n = 53) = 7.07, p = 0.004) and to the control condition (χ^2 (1, n = 51) = 6.25, p = 0.006), whereas the detection rates for the competent condition were not lower than in the control conditions (χ^2 (1, n = 50) = 0.01, p = 0.46) of critical trial 10. Note also that at critical trial 14, the difference between conditions was no longer significant (χ^2 (2, n = 77) = 1.82, p = 0.40; Competent = 15 %, Incompetent = 30 %, Control = 29 %). Finally, for completeness we examined detection rates on non-critical trials, which were always below 5 % and did not differ between the 3 groups, either overall across all non-critical trials (χ^2 (2) = 1.51, p = 0.47; Competent = 1.6 %, Incompetent = 2.5 %, Control = 3.1 %), or when focusing on non-critical trials occurring after the experimental manipulation (χ^2 (2) = 0.30, p = 0.86; Competent = 1.5 %, Incompetent = 2.2 %, Control = 2.5 %).

2.2.2. Preference-RTs & confidence

In terms of preference-RTs, LMM analysis showed no significant interaction effect between trial rank (before vs. after the manipulation) and experimental condition (b = -0.62, SE = 0.76, t = -0.82, p = 0.41), with non-significant main effects (p’s > 0.37). See Table SM-III in the Supplementary Material for the full results. Regarding confidence, the LMM analysis indicated only a main effect on critical trial rank (F(1, 145.8) = 13.2, p = 0.0008, d = 0.06, mean confidence: $\text{Trial}_7 = 4.4$; $\text{Trial}_{10} = 4.6$; $\text{Trial}_{14} = 3.8$), but no significant effect of experimental group or of the interaction (all ps > 0.97). Thus, confidence does not appear to be affected by our competence manipulation.

2.3. Discussion

This experiment confirmed our hypothesis: detection rates increased when the experimenter was perceived as less competent. Introducing the “Windows error” screen after trial 8 generated a situation of uncertainty, which was interpreted differently depending on the subsequent interaction with the experimenter. When the experimenter appeared reliable when solving the bug, detection rates after the bug were no different than in the control condition. However, when the experimenter seemed incompetent, they significantly increased. We note that this increase is most salient immediately after the manipulation, and that detection rates seem to be converging over time between the different conditions. Interestingly, confidence judgments did not track internal differences between experimental groups. In sum, our results suggest that our effect directly impacts on the choice blindness detection rate through a manipulation of prior expectations related to the reliability of the experimental context. In order to corroborate these results, in Experiment 2 we modulated the participants’ expectations with a more remote manipulation, so as to adhere more closely to the original choice blindness paradigm, without the disrupting event.

3. Experiment 2

3.1. Materials and methods

3.1.1. Participants

41 university students participated in a choice blindness protocol (age: M = 22, SD = 2.1; 24 women), with 21 participants for the competent group (age: M = 21.5, SD = 2.4; 7 women) and 20 participants for the incompetent group (age: M = 22.2, SD = 1.8; 10 women). Other criteria related to the selection of participants followed what was described for Experiment 1.

3.1.2. Stimuli and procedure

Stimuli were identical to those used in Experiment 1. In Experiment 2 however, the procedure did not rely on a “Windows error” screen during the experiment. Instead, we manipulated social cues about the reliability of the experimenter in the waiting room: When participants arrived at the laboratory, a confederate participant (the same actor that participated in Experiment 1) played the part of another participant of the experiment, striking up a conversation with the real participant. The interaction with the confederate participant aimed at generating either low or high expectations regarding the reliability of the experimentation team (Competent vs. Incompetent expectations) by mentioning the reputation of the research laboratory, the quality of the scientific work, and the expertise of the researchers (see SM-II in the Supplementary Material for the dialog script). In both conditions the actor maintained the duration of the interaction between a minimum of 3 min and a maximum of 6 min. At the end of this interaction, the real participant entered the test cabin and performed the traditional choice blindness protocol, identical in all details with the protocol of the Control condition of Experiment 1 (i.e. using 3 critical trials, and no “Windows error” screen). During debriefing, no participant reported suspicion concerning the interaction with the confederate prior to the experiment.

3.1.3. Statistical analysis

As was done for Experiment 1, we ran a *binomial* GLMM on Detection scores in critical trials, and LMMs for each measurement of interest (detection-RTs, preference-RTs and confidence). These analyses considered the different critical trials (7, 10 & 14), experimental groups (competent & incompetent) and their interaction as fixed effects, and we added an intercept for each participant as a random effect. As in Experiment 1, we report odds ratio for these regressions, and we conducted χ^2 tests separately for the different critical trials.

3.2. Results

3.2.1. Detection & detection RTs

In this experiment the overall detection was 14 % on critical trials. As can be seen on Fig. 3, in the first critical trial participants detected more substitutions in the incompetent group than in the competent group (detection rates: 25 % vs. 0 % at critical Trial 7), and the difference between the two groups declined afterwards (20 % vs. 9.5 % at critical Trial 10; 15 % vs. 14.3 % at critical Trial 14). Consistent with this, our GLMM analysis indicated that detection rates varied across conditions, reflecting a significant interaction between the experimental group and critical trials (b = -1.18, SE = 0.54, z = -2.16, p = 0.0311, OR = 0.31). There was also a main effect of experimental group (b = 14.6, SE = 7.07, z = 2.08, p = 0.0379, OR = 2.4*10^6[1]) and a main effect of critical trials (b = 2.03, SE = 0.98, z = 2.06, p = 0.04, OR = 7.58). Importantly, we conducted separate tests for the difference between the incompetent and competent groups in the different critical trials, which confirmed a lower detection rate for the competent group for critical Trial 7 (χ^2 (1, n = 41) = 5.97, p = 0.007), but not for critical Trial 10 (χ^2 (1, n = 41) = 0.90, p = 0.17) and critical Trial 14 (χ^2 (1, n = 41) = 0.004, p = 0.47).

For completeness, we examined detection rates on non-critical trials as well. These were overall very low (1.6 %), but slightly higher in the incompetent group (Competent = 0.4 %, Incompetent = 2.9 %; χ^2 (1) = 4.88, p = 0.014), suggesting that participants adopted a more liberal criterion to report choice substitutions in this condition. Finally, the analysis (LMM) on the response times associated with detection (*Detection-RTs*) revealed a significant interaction between critical trial and experimental group (F(1, 80.0) = 3.99, p = 0.0236, d = 0.04) with no main effects. However, a deeper inspection of this interaction did not indicate an increase in Detection-RTs in the competent condition (*FET:* p = 0.0712), nor in the incompetent condition (*FET:* p = 0.2603).

3.2.2. Preference-RTs & confidence

As for Experiment 1, preference-based response times (Preference-RTs) showed no significant effects or interactions between experimental groups and critical trials (b = -0.62, SE = 0.31, t = -1.99, p = 0.05), and no significant main effects (p's > 0.05). Regarding confidence in the perceptual choice, similar LMM analyses revealed no significant interactions involving the fixed effects (b = -0.01, SE = 0.07, t = -0.10, p = 0.92), nor any significant main effects (p's > 0.67).

3.3. Discussion

In Experiment 2 we replicate our findings from Experiment 1, in that participants are more likely to detect the substitution of the card when the context induces an expectation that the competence of experimenters is low. Furthermore, in Experiment 2 we implemented a traditional choice blindness experiment without introducing a disruptive error in the middle of the task, as the interaction with our actor occurred before the start of the experiment. In this setting, we find that the effect of the contextual manipulation is short-lived, as detection rates between the competent and incompetent contexts converge over 15 trials. Indeed, the effect is only observed in critical trial 7, with no differences detected in the other critical trials. To further replicate and generalize our findings, we implemented a third, online experiment with a larger sample size, where the experimental manipulation is introduced in the welcome message text prior to instructions.

4. Experiment 3

4.1. Material and methods

4.1.1. Participants

442 subjects (age: M = 28.03, SD = 8.57, 254 women) agreed to participate in an online experiment where a choice blindness protocol was implemented. Participants were recruited through Prolific Academic and received payment for their participation (USD $1.88 for approximately 15 min). All subjects read and accepted an informed consent approved by the University ethics committee before starting the experiment. We applied an online pre-screening for selecting participants who needed to have normal or corrected to normal vision and be fluent English speakers. As in Experiment 1, we tested three different experimental conditions: competent (n = 143), incompetent (n = 150) and control (n = 149).

4.1.2. Stimuli, procedure & statistical analysis

The experiment was conducted online using Qualtrics analytics. Stimuli were identical to those used in Experiment 2. The experimental manipulations in Experiment 3 consisted in different cover stories that appeared in the welcome message just after the

[1] The odds ratio (OR) reported for the effect of experimental condition in Experiment 2 should be interpreted with caution. The data exhibit signs of quasi-complete separation, as detection events are infrequent within the relevant predictor. Specifically, participants in the competent condition detected changes in 5 out of 63 trials, while those in the incompetent condition detected changes in 12 out of 60 trials. This results in a highly imbalanced outcome, with most responses being non-detections. The small sample size in Experiment 2 likely contributed to this sparsity and the resulting inflation of the OR. This limitation is addressed in Experiment 3, which includes a larger sample and more balanced detection rates.

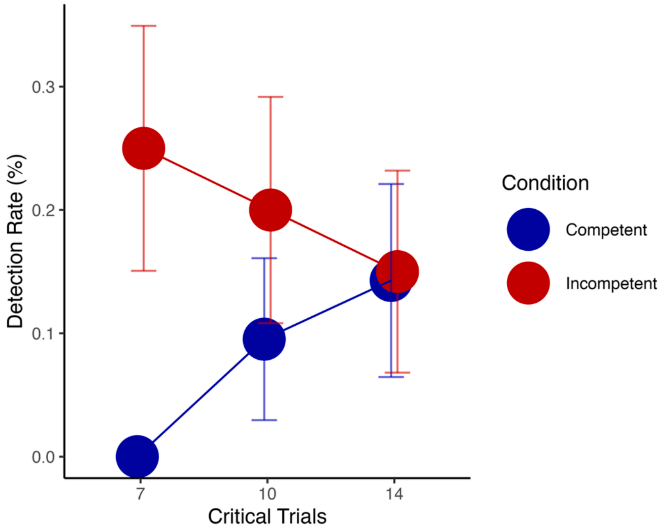

Fig. 3. Detection rates, calculated for each experimental group and critical trials (7, 10 & 14) for Experiment 2. In this experiment, the manipulation occurs before the experiment begins. Error bars show the group means ± 1 standard error (*SE*).

informed consent and before task instructions. This welcome message could go in three directions, aimed at projecting high, medium (control) or low expectations regarding the competence of the experimental team, by referring to the status of the experimenters (students vs. professors) and to their prior experience with running experiments (see SM-II Supplementary Material for the selected material). We ran a pre-test on 90 online participants to test 9 different sentences for each of the three contexts, asking participants to rate the level of competence that each sentence reflected. According to the results in this pre-test, we selected the best and the worst sentences to build the text for competent and incompetent conditions (see SM-IV Supplementary Material for all the tested sentences used in pilot study). After subjects read the welcome message, the choice blindness procedure was conducted as in Experiments 1 and 2. As suggested by an anonymous reviewer, we also reformulated the detection option from "*I chose the other card*" to "*I actually prefer the other face*" (as in Pärnamets, et al., 2015) which might be less suggestive of possible choice substitutions occurring in the experiment. The other options remained the same as in Experiments 1 and 2.

Our analysis strategy was identical to Experiments 1 and 2. Again, we were interested in detection rates in critical trials, and in how they might vary as a function of experimental groups, trial number (7, 10 and 14), and their interaction. We did not analyze response times as this variable was not available for each decision in the online platform.

4.2. Results

4.2.1. Detection

Detection rates are illustrated in Fig. 4. A binomial GLMM analysis, with detection as outcome and group (competent, incompetent, control), critical trials (7, 10 and 14) and their interaction as predictors, revealed a significant main effect of group ($b = 1.01$, $SE = 0.46$, $z = 2.21$, $p = 0.027$, OR $= 2.74$), a significant main effect of critical trials ($b = 0.29$, $SE = 0.09$, $z = 3.36$, $p < 0.001$, OR $= 1.34$), and a significant interaction between these two factors ($b = -0.09$, $SE = 0.04$, $z = -2.23$, $p = 0.026$, OR $= 0.92$). Further analyses confirmed significantly lower detection rates in competent than in the incompetent group for critical trial 7 (χ^2 $(1, n = 293) = 3.21$, $p = 0.037$), but not for critical trial 10 ($p = 0.35$) and 14 ($p = 0.78$). In other words, participants were less likely to detect the substitution in the competent than in the incompetent condition, at the beginning of the series of trials, and this effect vanished over trials much like in Experiments 1 and 2. For completeness, we examined detection rates in non-critical trials, and found, as in Experiment 2, that participants made slightly more false alarms in the incompetent group than in the competent group (Competent = 2.4 %, Incompetent = 3.9 %; χ^2 $(1) = 6.46$, $p = 0.006$), again consistent with the idea that they adopted a more liberal criterion to report choice substitutions in this experimental condition.

4.2.2. Confidence

As before, we also checked the confidence level in the perceptual choice, using a LMM analysis. This analysis revealed a significant main effect of critical trial ($b = -0.103$ $SE = 0.030$, $t = -3.33$, $p < 0.001$) with confidence decreasing over critical trials (from 4.96 in trial 7 to 4.83 in trial 10 and 4.33 in trial 14), with no main effect of condition ($p = 0.61$), and no interaction ($p = 0.78$). A limitation of Experiment 3 is that no analyses were conducted on preference RTs due to data loss.

4.3. Discussion

Results of Experiment 3 replicated the findings obtained in Experiment 2. Again, we implemented a traditional choice blindness

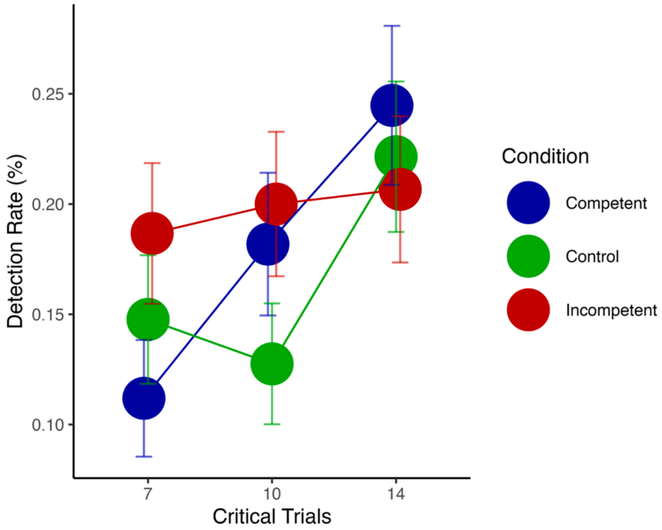

Fig. 4. Detection rates calculated for each experimental group and critical trials (7, 10 & 14) in Experiment 3. In this experiment, the manipulation was presented as cover stories regarding the experimental team just before the experiment started. Error bars show the group means ± 1 standard error (*SE*).

protocol, without a disruptive error that appeared during the task, but with a manipulation taking place before the experimental task itself. Again, we found that when expectations were biased towards lower competence, participants were more able to detect the substitution, particularly during the first critical trial. We considered the results of this experiment as further evidence that contextual information about the reliability of experimenters can modulate choice blindness. We note also that the manipulation of reliability cues was however more subtle in Experiment 3 than in Experiment 2. Indeed, whereas in Experiment 2 a person was physically present and delivered an explicit message about competence directly to the participant, in Experiment 3 we used an online setting, and participants did not receive a direct indication about the reliability of the experimenter, but only a written welcoming message which was not mentioning reliability explicitly. Thus, the replication of our main finding despite such changes in the procedure provides strong evidence for the robustness and generalizability of this observation that choice blindness can be modulated by contextual cues about the reliability of the experiment. However, a potential limitation of our online experiment in Experiment 3 is the absence of attentional checks and instruction comprehension questions. Future studies should carefully consider how to implement such checks without disrupting detection rates or drawing participants' attention to the cover story manipulation.

5. General discussion

Just seconds after making a choice between two pictures, people may fail to notice that they are given the picture they did not choose. They are also surprised to be subject to such a "choice blindness" (Johansson et al., 2005). This failure in our decision-making process is related to other phenomena in which observers overlook clear events in the visual scene and instead access informational content that is 'filled in' according to their expectations (Neisser & Becklen, 1975; Mack & Rock, 1998, Simons & Chabris, 1999; de Gardelle, Sackur & Kouider, 2009). It also figures prominently in reminders that introspection is fragile (Dehaene, 2014) or in more general accounts of the mind as being "shallow" (Chater, 2018). Here, we examine how choice blindness may be modulated by the social context surrounding the choice. We focus on the laboratory version of the choice blindness phenomenon, and more specifically, on expectations regarding the reliability of the experimenters.

We show that choice blindness decreases when experimenters appear incompetent, either because they indicate themselves being incompetent when having to deal with a bug in the experiment's code (Experiment 1), or because a third party tells the participant that experimenters are not reliable in this laboratory (Experiment 2), or because some aspects in the presentation of the experiment point towards the lack of experience of experimenters (Experiment 3). The means through which these experimental manipulations impact participants' beliefs are certainly different, but in all three cases, choice substitutions were detected more frequently when cues were provided towards the incompetence or unreliability of experimenters. Our results are obtained across a variety of settings (online or in the laboratory) and with different types of cues (from the experimenter themselves or from a third party), which suggest they would be generalizable to other situations. The possibility that cues regarding experimenter's competence may even account for some of the variability in detection rates observed in the literature opens an exciting avenue for further research.[2]

We acknowledge that in the present setting, these manipulations only acted as modulators of the choice blindness phenomenon, and did not completely erase the phenomenon: even in the incompetent condition, detection rates are below 50 %. Future experiments might explore alternative scenarios, which may trigger larger modulations. For instance, while we have only considered cues indicating that experimenters might make honest mistakes, one could instead inform participants that experimenters sometimes resort to magic tricks and deception to study attention and perception. More generally, we believe that by taking into account the social context of the experiment, one could realize that choice blindness might be modulated by participants' expectations about experimenters in many ways (Pärnamets et al., 2020). Of course, in different settings, and in particular in non-laboratory realizations of choice blindness, other social cues might be of relevance too. Clearly, more research is needed to investigate this issue.

We also note a potential limitation of the present work, in that one might wonder whether the incompetent conditions in our empirical studies are truly realistic (and by extension whether choice blindness truly is fragile and context-dependent). In response to this concern, we would argue that many experimenters have experienced first-hand awkward moments in front of participants, as they had to urgently fix or simply cancel an experiment because of technical difficulties (e.g. a bug in the code, a network issue, an unanticipated operating system update, etc.). Online experiments created by researchers may not always be presented in the most professional and polished manner. After all, experimenters are humans and humans make mistakes. Thus, we would argue that in both laboratory and online experiments, participants can reasonably encounter conditions or experimenters who are not infallible, which makes our incompetent condition a plausible and realistic scenario. However, we acknowledge that the lack of an empirical measure to address this question constitutes a limitation of the present work, which should be addressed in future investigations.

To account for the role of social context in choice blindness, we could rely on a framework reminiscent of classical Signal Detection Theory (Green & Swets, 1974): even though detection of substitutions was never instructed as a task to participants, we could suppose that a substitution causes a noisy "error signal" that needs to reach a certain threshold so as to trigger a report. Below this threshold, the error would remain unreported. In this framework, the perceived competence of the experimenters would plausibly influence the setting of the threshold. This line of interpretation is a special case of a more general Bayesian framework for social influence (Toelch & Dolan, 2015), where the detection criterion would depend on a prior for there being a substitution and a cost for reporting it. Adopting this framework, we speculate that our manipulation of social cues for competence have mainly impacted the prior probability of errors (although we cannot exclude the possibility that it also impacted the cost of reporting the error). Pushing further, we could even consider that the implicit task would be for participants to ascribe the error either to the experimenters or to themselves.

Within this framework, our results even suggest that, by default, participants assume that experimenters make no mistake. Indeed, in Experiment 1, detection rates take a relatively low value in all conditions before the manipulation, and then once the cue towards competence or incompetence is delivered, they only increase in the incompetence condition, whereas they remain low in the competent and control condition. Note that it is also reasonable to assume that such beliefs regarding the reliability of experimenters are updated with experience. This would probably explain the increase in detection rate with the rank of the manipulated trial, a phenomenon that is well documented in the Choice Blindness literature (see for instance Taya et al., 2014). In our three experiments, we observe that detection rates for the competent and incompetent conditions converge over time. This is consistent with the idea that participants' experience, over time, would override their prior belief derived from their past history and from our experimental manipulation. However, we note that in Experiments 1 and 2 detection rates *decreased* for the incompetent condition as the experiment progressed. We speculate that this may reflect disengagement from participants in these situations of incompetence. Investigating, in various environments, the dynamics of choice blindness over time would constitute an interesting perspective for future research.

The main message from the present work is that choice blindness does not involve only a failure at the level of the participant's individual cognition, but also some form of social influence, as has been suggested in the literature (Pärnamets et al., 2020). This brings a host of factors and variables worth investigating, and we would like to highlight possible perspectives for future work that the present study opens. First, one could try to disentangle the effect of the perceived reliability of the specific experimenter or research environment, from a general priming effect about incompetence or unreliability that would not be attached to the research environment itself. Here, we manipulated cues that related to specific aspects of the experimental situation, and thus we naturally assumed that the reduction of choice blindness in the presence of such cues was due to a specific form of unreliability, but we cannot exclude that our experimental manipulations affected participants' behavior through a more generic channel. Second, as it has been shown that individuals' responses to social influence is quite variable (even when this influence produces its effects at the level of the perception of sensory stimuli, see e.g. Anlló, Watanabe, Sackur, & de Gardelle, 2022), and as we anticipate that detection of an unexpected substitution in choice blindness paradigms should be no exception, we can therefore predict that the effect of social cues on choice

[2] Individual differences may offer a valuable direction for future research on choice blindness (CB). Indeed, an anonymous reviewer suggested examining whether age or gender interacted with our experimental conditions. We explored this in Experiment 3 and found no significant interaction between gender and condition nor between age and condition (all p's > 0.24) Interestingly, we observed a significant main effect of age on detection trials ($b = -0.32$, $SE = 0.14$, $p = 0.03$, $OR = 0.72$), although the explained variance was minimal ($r^2 = 0.009$). The trend suggests that younger participants were slightly more likely to detect changes than older ones—a counterintuitive result, given previous findings (e.g., Law et al., 2017; Sauerland et al., 2014) suggesting that younger individuals, due to less mature cognitive development, tend to perform worse in detection tasks compared to older adults.

blindness would be more pronounced in individuals who are more susceptible to social influence (Strandberg et al., 2019). Finally, as much as the presence of cues regarding the reliability of the experimental environment may influence choice blindness, one could investigate more generally whether such cues may affect other types of cognitive processes as well. One natural candidate is the sense of agency, that is the perception of one's own control over our actions and their consequences on the external world. Indeed, we speculate that one reason for participants' inability to notice choice substitutions is the lack of agency in the traditional choice blindness situation. Therefore, we speculate that cues towards incompetence might increase the sense of agency in participants. If this is so, we predict that other cognitive processes that depend on agency (feedback processing, introspection, emotional responses, and so on) would also be impacted by cues relative to experimenters' reliability.

The idea that participants' behavior is in part controlled by their construal of the experimental situation is classical in psychology (Orne, 1962). A telling example may be found in the reinterpretation of perseverations in Piaget's A not-B task in the "natural pedagogy" framework (Csibra & Gegerly, 2009): babies' failures to perform the task as intended by the experimenters may not mainly be due to their ineffective notion of object permanence, but rather it may result from their interpretation of the situation as a learning context (see also Rose & Blank, 1974). Similarly, we suggest here that non-detections in choice blindness may in part depend on participants' actively taking into account the social *a priori* that the laboratory is no place for errors: by default, as seen in our neutral conditions, participants behave as if they trust the experimenters.

6 Consent to participate

Informed consent was obtained from all individual participants included in the study.

7 Consent to publication

The authors affirm that the human research participants provided informed consent for the publication of the data and further statistical analysis with group-level description (such as means and standard deviations), including the data shown in Figs. 2–4.

8 Availability of data and material

Dataset are available at the Open Science Foundation at: https://osf.io/ht769/.

9 Code availability

Code scripts are available at the Open Science Foundation at: https://osf.io/ht769/.

CRediT authorship contribution statement

Nicolás Marchant: Writing – review & editing, Writing – original draft, Visualization, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Gorka Navarrete:** Software, Resources, Investigation. **Vincent de Gardelle:** Writing – original draft, Validation, Supervision, Methodology, Formal analysis, Data curation. **Jaime R. Silva:** Supervision. **Jérôme Sackur:** Writing – review & editing, Writing – original draft, Supervision, Methodology, Funding acquisition, Formal analysis, Conceptualization. **Gabriel Reyes:** Writing – review & editing, Writing – original draft, Supervision, Project administration, Methodology, Funding acquisition, Formal analysis, Conceptualization.

Ethics approval

Approval was obtained from the ethics committee of University Austral de Chile (Exp. 1 and 2) and Universidad Adolfo Ibáñez (Exp. 3). The procedures used in this study adhere to the tenets of the Declaration of Helsinki and the World Medical Association.

Funding

This research was supported by the Fund for Science and Technology (Fondo de Desarrollo Científico y Técnológico, FONDECYT Chile) Grant N° 1221785 (GR), and by the Agence Nationale de la Recherche (France) Grant N° ANR-10-IDEX-0001-02 (JS).

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.concog.2025.103887.

Data availability

Dataset and the analysis scripts are available at the Open Science Foundation at: https://osf.io/ht769/.

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