No Matter of Trust: Retrieval of Observationally Acquired Stimulus-response Bindings Occurs in Interactions with Both Trustworthy and Untrustworthy Partners

Kira Franke[1], Nicole Keller[1], Klaus Rothermund[1], Bernhard Hommel[2], and Carina G. Giesen[3]

[1] Department of General Psychology II, Friedrich-Schiller University Jena, Jena, Germany

[2] Department of Psychology, Shandong Normal University, Jinan, Shandong Province, China

[3] Department of Psychology, Faculty of Health, Health and Medical University Erfurt, Erfurt, Germany

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ORCID

Kira Franke 0000-0002-3038-2826

Bernhard Hommel 0000-0003-4731-5125

Klaus Rothermund 0000-0002-9350-5272

Carina G. Giesen 0000-0002-2395-4435

Correspondence concerning this article should be addressed to Kira Franke, Friedrich-Schiller University Jena, Department of Psychology, General Psychology II, Am Steiger 3, Haus 1, 07743 Jena, Germany. Email: kira.luisa.franke@uni-jena.de. Telephone: ++49(0)3641/9-45114

Abstract

Recent findings show that simply observing how another person responds to a stimulus is sufficient to create stimulus-response (SR) episodes, which can later be retrieved from memory to guide one's own actions. However, this only occurs if the observed person is socially relevant for the observer. This social relevance can result from task demands (e.g., cooperation or competition) or the relationship between the interacting people. An essential component of successful social relationships, which should therefore also increase the social relevance of another person, is trust. In two online experiments we investigated whether trusting versus distrusting an interaction partner modulates observationally acquired SR binding and retrieval (oSRBR) effects. Trust was manipulated by a variation of the Investment Game. Interaction partners behaved either trustworthily or untrustworthily by keeping or violating a previous promise. After that, participants performed an online interactive color classification task to assess oSRBR effects. Both experiments yielded successful manipulation checks. Significant oSRBR effects emerged, but these were not modulated by trust. We discuss potential reasons underlying these findings, particularly whether and how distrust may also have facilitated the occurrence of oSRBR effects.

Keywords: stimulus-response binding, event files, observational learning, trust, online interactions

Executing a response in close temporal proximity to a stimulus is known to result in the creation of a retrievable stimulus-response (SR) binding or event file (Hommel et al., 2001, for overviews see Frings et al., 2020; 2024). Of particular interest for our purposes, people create such an event file even when the response is carried out by another person they merely observe (Franke et al., 2025; Giesen et al., 2014; 2017; 2018; 2021; Giesen & Rothermund, 2022). Subsequent repetition of the same stimulus triggers the retrieval of this observationally acquired SR binding, which will reactivate the previously observed response. This will impact on the observer's performance and facilitate responding if the retrieved response is compatible with the required response in the current situation, but interfere with responding if retrieved and required response are incompatible. Statistically, retrieval of observationally acquired SR bindings is therefore reflected by an interaction of stimulus relation and response compatibility.

Importantly, the emergence of such observationally acquired SR binding and retrieval (oSRBR) effects depends on the relationship between observer and observed person. In previous studies, oSRBR effects only occurred when there was some form of interdependence between interaction partners, either because they were instructed to cooperate or compete (Giesen et al., 2014) or because they were in a romantic relationship with each other (Giesen et al., 2018). In contrast, oSRBR effects were absent when interaction partners worked independently and were strangers. This implies that interaction partners need to be perceived as socially relevant for binding and retrieval by observation to occur. The modulating influence of social relevance might be attributed to a weighing process as assumed by the intentional weighing principle (Memelink & Hommel, 2013): Stimulus- and response features of actions executed by a relevant interaction partner might be attended more than those of an irrelevant partner. This could lead to these features receiving stronger activation, making it more likely for them to be integrated in an event file and/or to be retrieved later on (Giesen, 2024).

A similar influence of social variables can also be found in joint action research. Generally, co-representation of another person's actions as indicated by interference effects in the joint Simon task (Sebanz et al., 2003) is stronger if the relationship between co-actors is positive or cooperative, while co-representation is reduced or absent for negative or competitive relationships (e.g., Hommel et al., 2009; Iani et al., 2011; Kuhbandner et al., 2010; Quintard et al., 2020; Shafaei et al., 2020, but see also Ruys & Aarts, 2010). The modulating influence of the relationship is typically attributed to increased overlap between the mental representations of one's own and the other's actions due to greater perceived similarity with the co-actor in positive or cooperative compared to negative relationships (Dolk et al., 2014; Hommel et al., 2009).

Trust and social relationships

Trust is generally considered a basic social emotion that is essential for building successful social relationships. It is positively linked to relationship quality, both in private (Campbell et al., 2010; Eckstein & Cohen, 1998; Mikulincer, 1998) and work or business contexts (Hunt et al., 2011; van Toder, 2016). Trust is, however, also a somewhat opaque concept due to the multitude of its definitions (Lewis & Weigert, 1985; Mayer et al., 1995; Rotter, 1971; Simpson, 2007). In our study, we followed the definition by Mayer et al. (1995), who describe trust as "the willingness of a party to be vulnerable to the actions of another party based on the expectation that the other will perform a particular action important to the trustor, irrespective of the ability to monitor or control that other party". Whether people engage in trusting behavior crucially depends on how trustworthy the other person is perceived (Chang et al., 2010; Hale et al., 2018; Tingley, 2014; van der Biest et al., 2020). Trustworthiness is evaluated in less than 40ms upon meeting a novel person using facial information (Oosterhof & Todorov, 2008; Todorov et al., 2015) and then continuously updated based on the person's behavior (Chang et al., 2010).

Despite plausible theorizing (Hommel & Colzato, 2015), it has never directly been investigated whether trust influences co-representation of another person's actions or retrieval of observationally acquired SR bindings. However, there are several findings that encourage this idea: First, people are more likely to follow the advice of a trustworthy compared to an untrustworthy instructor (Hale et al., 2018; van der Biest et al., 2020), implying that information originating from someone perceived as trustworthy are considered more relevant. Second, there is evidence that people automatically follow the gaze of trustworthy, but not of untrustworthy looking faces (Ding et al., 2024; Süßenbach & Schönbrodt, 2014; but see also King et al., 2011; Strachan et al., 2017), which suggests that trust may also impact on automatic, unconscious processes. Third, trust is closely associated with cooperation (Balliet & van Lange, 2013; Chang et al., 2010; Gambetta, 1988) and positive relationships (Campbell et al., 2010; Eckstein & Cohen, 1998; Hunt et al., 2011), two factors that increase co-representation (Hommel et al., 2009; Iani et al., 2011; Kuhbandner et al., 2010; Quintard et al., 2020; Ruys & Aarts, 2010; Shafaei et al., 2020) and oSRBR effects (Giesen et al., 2014; 2018). Therefore, it seems likely that trust in the person one is interacting with influences retrieval of observationally acquired SR bindings in a similar way.

The present study

We were interested whether (dis-)trust in the person one is interacting with influences oSRBR effects. To induce trust versus distrust, participants played a Trust Game, a modified version of the Investment Game (Berg et al., 1995) with their putative interaction partner. The goal of this game was to gain as many points as possible to get an extra reward. This could only be achieved if both players cooperated. In the high trust condition, the partner behaved fairly and shared points with the participant, while in the low trust condition, the partner kept everything to themself despite promising to share. Then participants performed the online version of the observational SR binding task (Giesen & Rothermund, 2022) with the same person to assess oSRBR effects. We expected oSRBR effects to occur only if interaction partners had behaved trustworthily during the Trust Game. In turn, oSRBR effects should be absent if partners had behaved untrustworthily before. To anticipate results, Experiment 1 yielded robust oSRBR effects that were not modulated significantly by trust, despite successful manipulation checks. However, descriptively, effects were larger in the high trust compared to the low trust condition. Additionally, many participants indicated that they did not believe their partner was human, which may have affected results (cf. Giesen & Rothermund, 2022). Because of this and the relatively low statistical power due to small sample size in Experiment 1, we conducted a higher-powered Experiment 2, which incorporated a few minor changes to make the interaction appear more realistic.

Method

Preregistration, open access, and ethics vote

Both experiments were preregistered online prior to data collection (Experiment 1: https://aspredicted.org/995qz.pdf, Experiment 2: https://osf.io/hbrpw/?view_only=9f7021ad2bc6453cac70eb385d807dba). All experimental files, data and analyses scripts are available on the Open Science Framework (https://osf.io/9yj3a/).

All experiments were in accordance with the Ethical standards of the Institute of Psychology of the University of Jena and the Declaration of Helsinki. For Experiment 1, ethical approval was granted by the Ethics Committee of University of Jena (FSV 22/031).

Participants

To estimate the required sample size, we ran an *a priori* power analysis in G*Power 3.1 based on the effect size that Giesen and Rothermund (2022) reported for modulations of oSRBR effects (d_z=.40). To detect an effect of this size with a statistical power of 1-β = .80 and an alpha level of α = 0.05 in a one-tailed independent-samples *t*-test, a total of n = 156 (78 per trust condition) participants are needed.

As Experiment 1 was conducted as part of a bachelor thesis, the data collection had to take place within a limited time period. For this reason, the recruited sample size deviated from the required sample size calculated in the a priori power analysis. In total, 72 participants took part in Experiment 1. These were recruited via an e-mail distribution list of the University of Jena (n = 46) and Prolific Academic (n = 23, https://www.prolific.com/). One participant had to be excluded due to excessive error rates (> 25% in the memory test), another one because they did not pass the practice block. A third participant was excluded because they participated a second time after experiencing technical problems during their first participation, enabling them to figure out the deceptive nature of the trust manipulation. Thus, data of n = 69 participants were analyzed (47 female, 20 male, 2 diverse; M_{age} = 24.5 years, SD_{age} = 7.3).

For Experiment 2, 173 new participants were recruited at Prolific Academic. Four participants had to be excluded due to excessive error rates (>25% in the memory test), another four did not pass the practice block. Nine more participants were excluded because no other participant was available or they did not exchange any messages during the chat phase, four because they did not send any points during the first round of the Trust Game, undermining the trust manipulation. This means that data of n = 154 participants were analyzed (60 female, 92 male, 2 diverse; M_{age} = 28.7 years, SD_{age} = 6.8).

In Experiment 1, all participants were native German speakers; for Experiment 2 we recruited both native German and native English-speaking participants[1]. Participants on Prolific could only take part in any of the experiments if they had never previously participated in online experiments on oSRBR effects and if they conducted the experiments on a notebook or desktop computer. Additionally, for Experiment 1 they had to use Windows 10 as an operating system; for Experiment 2, participants were prescreened to be between 18 and 45 years old.

[1] As stated in the preregistration, we first recruited German-speaking participants for Experiment 2. However, this was harder than expected since we always needed at least two participants to be active at the same time (see Procedure). Since there are many more Prolific users that are native English speakers than native German speakers, we then decided to finish the data collection with English native speakers, translating the instructions, stimuli, and messages of the online experiment to English. Exploratory analyses revealed that native language did not affect our results in any meaningful way.

Both experiments had a medium duration of 33 minutes. Students of the University of Jena received partial course credit for their participation in Experiment 1. Prolific users received a financial reward (Experiment 1: 5.25 £, Experiment 2: 6.00 £) for taking part in the study.

Design

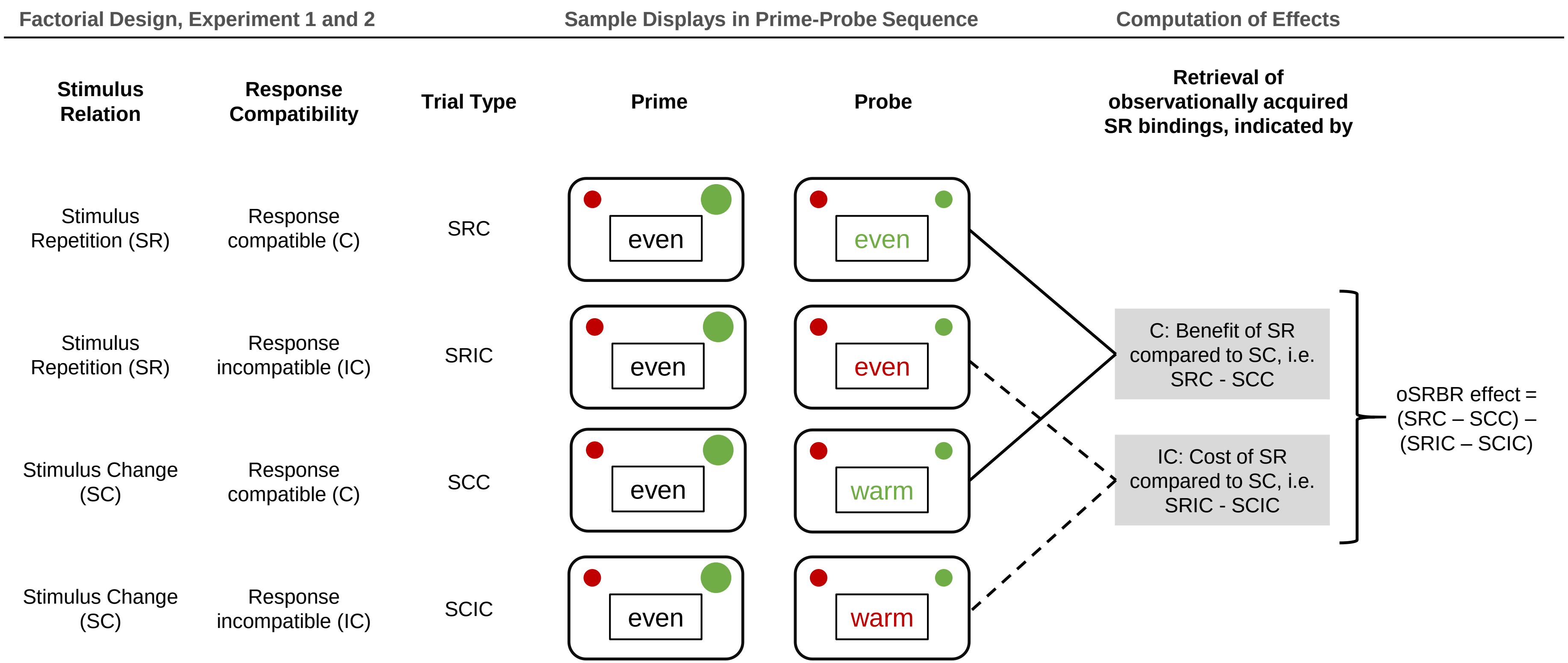
Both experiments comprised a 2 x 2 x 2 mixed factors design with two within-subject factors (stimulus relation and response compatibility), and one between-subject factor (trust condition). The between factor was manipulated by means of the Trust Game: Participants were randomly assigned to play with an interaction partner that behaved either in a trustworthy (high trust) or an untrustworthy way (low trust). Stimulus relation was manipulated by either repeating or changing the word stimulus from prime to probe in the observational SR binding task (50% stimulus repetitions, 50% stimulus change). Response compatibility was manipulated by requiring participants to perform a color categorization response in the probe trial that was either compatible to the response observed in the prime trial in 50% of all prime-probe sequences (compatible response, e.g. red-red) or incompatible with the observed prime response in 50% of all prime-probe sequences (incompatible response, e.g. green-red). See Figure 1 for examples for each combination of the within-factors. The dependent variable of interest was response time (RT) in the probe trials of the observational SR binding task.

Apparatus and stimuli

Experiment 1 was programmed with E-Prime 3 and converted for online data collection with E-Prime Go 1.0. Experiment 2 was programmed and hosted online using Gorilla Experiment Builder (www.gorilla.sc; Anwyl-Irvine et al., 2020), as the software allows chat interactions between two real participants. The studies were conducted on either a

Figure 1

Sample displays of color categorization task for Experiment 1 and 2



Note. Observed responses in prime trial are represented by the larger button, i.e. in all of the sample prime display the observed response is green. For illustrative purposes, prime stimulus and observed response were integrated into one sample screen. In the experiments they were not displayed simultaneously, but consecutively (see Procedure). Stimuli are not drawn to scale, foreground and background colors are inverted.

desktop computer or notebook. As stimuli we used 25 neutral, mono- or disyllabic adjectives (e.g. warm, slow, even), that were either in German for German participants or in English for English participants. Stimuli were presented either in white (RGB: 255, 255, 255), red (RGB: 255, 0, 0) or green (RGB: 0, 255, 0) font on a black background (RGB: 0, 0, 0). The font size was 53px in Experiment 1 and 30px in Experiment 2.

Procedure

Unless mentioned otherwise, Experiments 1 and 2 followed the same procedure. At the start of each experiment, demographic information was collected and participants gave their informed consent to take part in the study; otherwise, the study was terminated. Next, participants were informed that they would now be connected with another participant and that they would interact with this person during the entire experiment. Actually, all interactions except for the chat in Experiment 2 (see below) were scripted and pre-programmed. However, since Giesen and Rothermund (2022) found that oSRBR effects only emerged when participants believed they were interacting with another human, we included the following measures to ensure participants believed their interaction partner was another person: In Experiment 1, after waiting for a few seconds, participants were supposedly connected with their interaction partner and received information about the partner’s name and age. Participants were also asked to send their partner a welcome message. After sending the message, participants received a message from their alleged interaction partner. As several participants stated in the post-experimental questions that they believed these messages to be scripted in Experiment 1, we decided to include a brief interaction with a real participant at the start of Experiment 2: Participants waited up to 5 minutes to be connected to another participant. If nobody was available during that time, the experiment was terminated and participants received a partial compensation of £0.75. In case of a successful match, the two participants then had three minutes to exchange text messages in a chat in real time. After the chat, they were disconnected from each other without their knowledge. To maintain the illusion for participants that they were still interacting with the other person throughout the rest of the experiment, participants occasionally had to wait for their partner to finish reading instructions or executing responses.

Next, participants played three rounds of a Trust Game with their putative interaction partner. This game served to manipulate participants' trust in their interaction partner and was based on the Investment Game developed by Berg et al. (1995). Participants were randomly assigned to either the high trust (Experiment 1: n = 31, Experiment 2: n = 79) or the low trust condition (Experiment 1: n = 38, Experiment 2: n = 75). Participants were told in the beginning that the four players that collected the highest amount of points in the game would receive an Amazon voucher of 5€ (5£ for British participants). For reasons of fairness, however, the vouchers were raffled among all participants after the data collection concluded. At the start of each round, participants received 10 points and could either decide to keep these or to send a portion of the points of their choice to their interaction partner. In this game, trust is operationalized by the amount of points that a player chooses to invest in their partner. The points that were sent to the partner were then quadrupled. Participants were told that their interaction partner could then decide how many of the resulting points they wanted to send back to them.

Each round worked as follows: At the start of each round, players were asked to declare how they intended to behave in the current round towards their interaction partner. In Experiment 1, participants could write a message to their partner. It was suggested to participants that their partner was writing a message to them at the same time. Participants received their partner's message after sending their own. In both conditions, the interaction partner would always promise to send half of the points they received back to the participants. In Experiment 2, participants had to select the message that best described their intentions for the current round from five predetermined messages. After submitting their choice, they were presented the message their partner had supposedly chosen, which always stated that the interaction partner would return half of the points they received to the participant. Then, participants choose the number of points to send to their interaction partner, which was then multiplied by four. In the high trust condition, the interaction partner would always share with the participant and return half of the points back to them. In the low trust condition, the interaction partner would break their promise and keep all of the points for themselves. Then, the next round started. Participants played three rounds in total. In the low trust condition, the partner would also apologize for breaking their promise after round 1 and 2.

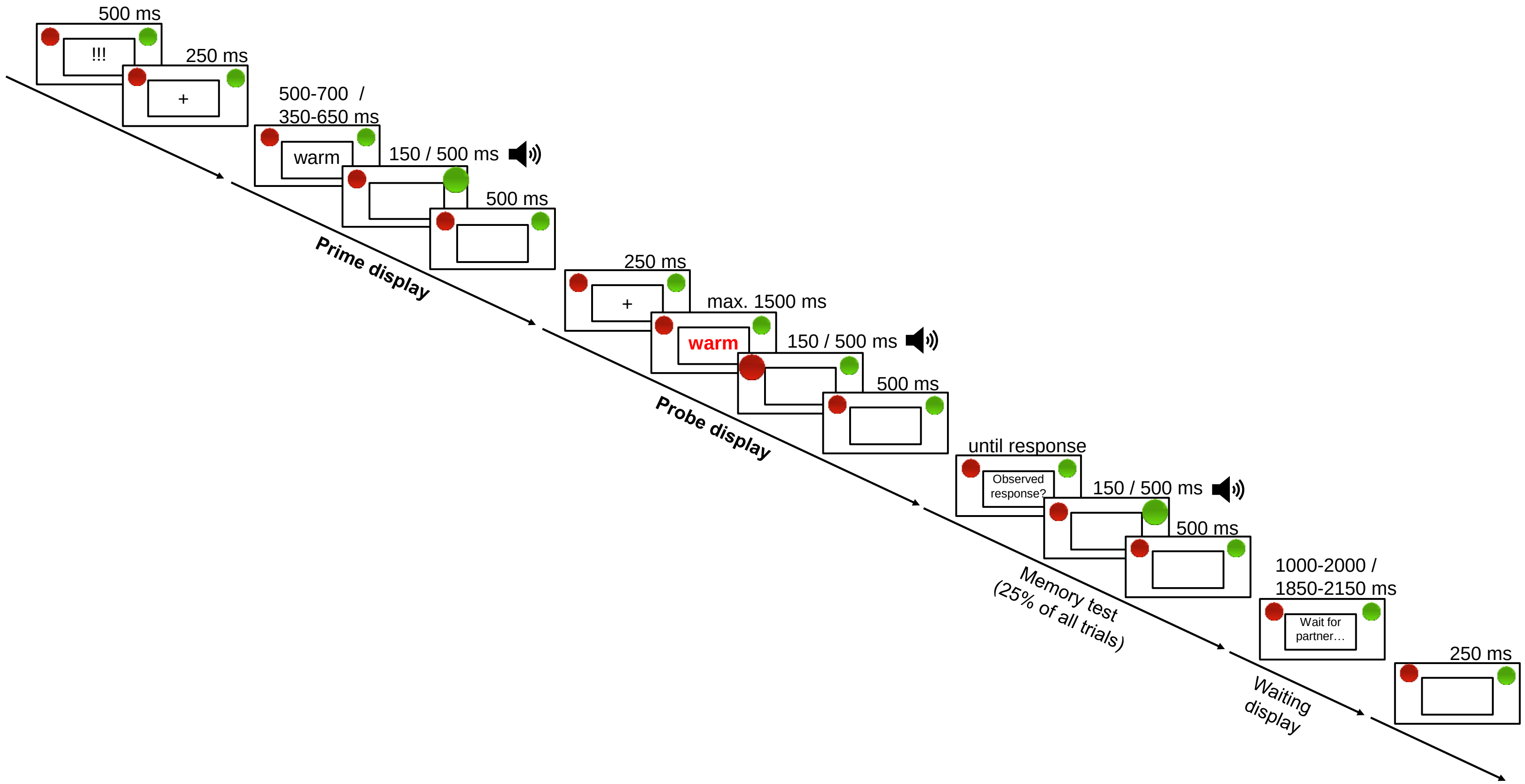
After finishing the Trust Game, participants performed the online version of the observational SR binding task developed by Giesen and Rothermund (2022). This sequential priming paradigm was used to assess oSRBR effects. Participants were informed that in the following task they would take turns with their interaction partner to categorize words based on the color they were presented in. If participants saw a word in red or green font, they had to respond as fast and accurately as possible by pressing the corresponding key (‘A’ for red, ‘L’ for green). If the word appeared in white, it was the interaction partner’s turn to respond. In both cases the response given was simulated by a virtual red or green button lighting up in the upper right or left corner of the screen (see Figure 2). Participants were instructed to carefully observe and memorize their partner’s responses, as they would be asked to remember them in occasional memory tests. The instructions were followed by a brief instruction check. Participants were asked to remember which key corresponded to which color. If they did not answer with 100% accuracy, they had to read the instructions again.

After that, there was a brief practice block of 24 prime-probe sequences (Experiment 1: 8 prime-probe sequences for participants recruited via Prolific). The practice block was repeated if participants made too many errors in the color categorization task (> 20% errors) or responded slower than 1000 ms too often (slow response in more than 50% of all trials). In Experiment 2, participants needed to have less than 25% errors in the color categorization task, slow responses in less than 25% of all trials and an error rate of less than 25% in the memory test to pass the practice block. If participants did not meet these performance criteria after several tries (Experiment 1: 4 tries allowed, 3 for Prolific users; Experiment 2: 2 tries allowed), the experiment was terminated.

Once the practice block was completed successfully, the main block started. The interaction partner responded during prime trials (which therefore reflect observation trials), whereas participants responded during probe trials. At the start of each of the main block's 128 prime-probe sequences (see Figure 2), a ready signal (!!!, 500ms) was presented centrally on the screen. Then the prime trial began with a fixation cross (250ms), followed by the appearance of a white word. This word remained on screen for a variable duration (Experiment 1: 500-700 ms, Experiment 2: 350-650 ms) which reflected the time the putative interaction partner needed to make their response. Once the word disappeared, the partner's response was simulated by a red or green response button lighting up in one of the upper corners of the screen. This impression was created by first showing a larger picture of one of the buttons for 500ms while a clicking sound was played. Then the button was shown in its standard size for 500ms. In case a participant responded in the prime trial, error feedback was shown ("WRONG PERSON!", 1000ms). Next, the probe trial also started with a fixation cross (250ms), followed by the appearance of a word. This word was presented either in red or green font and remained centrally on the screen either until the participant responded by pressing either the 'A' or the 'L' key or until 1500ms had passed. The executed response was then simulated by the corresponding button lighting up in the same way as described for the prime trial. If participants responded incorrectly or failed to respond in time, error feedback was given (1000ms, "WRONG KEY!", "Respond faster!"). In 25% of all prime-probe sequences the probe trial was followed by a memory test. The memory test required participants to press the key that corresponded to the response they had observed in the previous prime trial. After responding, the corresponding response button lit up, as described for the prime and probe trials. If the response was incorrect, error feedback was shown ("INCORRECT!", 1000ms). All sequences ended with a blank screen (250ms). After every 32 prime-probe sequences there was a break and participants received a short feedback on

Figure 2

Example of a prime-probe sequence in the observational SR binding task.



Note. Stimuli are not drawn to scale. For illustrative purposes, foreground and background colors are inverted. Stimuli in boldface were presented in red/green; stimuli in normal face were presented in white. If two different durations are indicated for a screen, the first one refers to Experiment 1 and the second one to Experiment 2.

their performance (% errors in the color categorization task, % errors in the memory test, % slow responses).

When the observational SR binding task was completed, participants were asked on-screen to write down what they thought the study was about and whether they had noticed anything unusual. As a manipulation check, they were then asked to rate via mouse click on a 4-point Likert scale how trustworthy, cooperative, and reliable they thought their interaction partner was (1 = untrustworthy/not cooperative/not reliable, 5 = neutral, 9 = very realistic). After answering all questions, participants were fully debriefed[2] and could leave an email address if they wanted to trustworthy/cooperative/reliable). Finally, participants were asked to rate via mouse click on a 9-point Likert scale how realistic they perceived the interaction (1 = very unrealistic; 5 = neutral, 9 = very realistic). After answering all questions, participants were fully debriefed and could leave an email address if they wanted to take part in the raffle of the Amazon vouchers.

Data preparation

Statistical analyses were performed with R (Version 4.1.2). Bayes Factors were computed with JASP (Version .14.1.0)

For our manipulation checks, participants' post-experimental ratings were compared as a function of trust condition using independent sample *t*-tests (see Table 1). To investigate how trust in the interaction partner developed over the three rounds of the Trust Game in both groups, we conducted a 3 (round in the Trust Game: 1 vs. 2 vs. 3) x 2 (trust condition: low trust vs. high trust) mixed factors ANOVA with points sent to the interaction partner as a dependent variable. Further, we conducted two types of follow-up tests using *t*-tests: First, we compared the points sent in each individual round between high and low trust condition.

[2] In Experiment 1, participants recruited with the email distribution list were not debriefed immediately upon completing the experiment but later via email. This was done because other similar studies used the same email distribution list for recruitment at the time of the data collection.

Second, we tested whether the number of points sent to the interaction partner differed between the round of the Trust Game within the trust conditions (see Table 1).

Prior to all analyses, probe responses were discarded because of erroneous responses in the color classification task (Experiment 1: 2.1%, Experiment 2: 1.4%) and errors in the memory test (Experiment 1: 5.0%, overall: 1.3%; Experiment 2: 4.3%, overall: 1.0%). Probe responses faster than 200ms or slower than 1.5 interquartile ranges above the 75th percentile of the individual RT distribution were regarded as outliers (Tukey, 1977) and were excluded (Experiment 1: 3.8%, Experiment 2: 3.6%). Mean probe RTs for the conditions of the factorial design are presented in Table 2. For both experiments, we computed effect scores for oSRBR effects reflecting the stimulus relation x response compatibility interaction for each participant. This score is calculated by subtracting the performance costs of stimulus repetitions compared to stimulus changes in incompatible trials from the performance benefit of stimulus repetitions vs. changes in compatible trials (see Figure 1 for formula and examples). Positive values on this score reflect a pattern indicative of the presence of oSRBR effects.

Results

Manipulation checks

Trust manipulation

Behavioral data from the Trust Game shows that, in both experiments, the amount of points sent to the interaction partner remained constant or increased with each round in the high trust condition. In contrast, in the low trust condition participants sent less points with each round, reflected in a significant trust condition x round interaction (Table 1). This pattern

Table 1

Descriptive results of manipulation checks as well as inferential statistics.

		High trust	Low trust			
Trust ratings		*M*	*M*	*t*	*df*	*p*
Experiment 1	Trustworthy	3.2	1.2	11.0***	67	<.001
	Cooperative	3.9	1.2	29.7***	67	<.001
	Reliable	4.0	1.1	45.4***	67	<.001
	Trust Score	3.7	1.1	28.7***	67	<.001
Experiment 2	Trustworthy	3.8	1.3	29.3***	152	<.001
	Cooperative	3.9	1.5	25.2***	152	<.001
	Reliable	3.9	1.2	35.9***	152	<.001
	Trust Score	3.9	1.4	37.1***	152	<.001
Trust Game: points sent per round		*M*	*M*	*t*	*df*	*p*
Experiment 1	Round 1	7.4	8.0_a	0.92	67	.361
	Round 2	7.8	4.9_a	3.40**	67	.001
	Round 3	7.4	2.8_a	5.78***	67	<.001
Experiment 2	Round 1	8.8_b	8.5_c	0.91	152	.364
	Round 2	9.1	5.3_c	7.32***	152	<.001
	Round 3	9.5_b	3.3_c	12.2***	152	<.001
Trust Game: ANOVA results		*df*1	*df*2	*F*	*p*	η_p^2
Experiment 1	Trust (T)	1	67	12.3**	.001	.16
	Round (R)	2	134	30.8***	<.001	.31
	R x T	2	134	26.7***	<.001	.29
Experiment 2	T	1	152	88.5***	<.001	.37
	R	2	304	37.4***	<.001	.20
	R x T	2	304	64.6***	<.001	.30
		High Trust	**Low Trust**			
Memory test performance (error rate)		*M*	*M*	*t*	*df*	*p*
Experiment 1		5.9	4.3	1.36	67	.179
Experiment 2		3.5	5.1	2.01*	152	.046
Realism of interaction		*M*	*M*	*t*	*df*	*p*
Experiment 1		3.6	3.0	1.33	67	.190
Experiment 2		7.1	5.9	3.87***	152	<.001

Note. Means for the Trust Game with the same subscripts within the same column differ at $p < .05$. Trust Score = (Trustworthy + Cooperative + Reliable)/3. * $p < .05$. ** $p < .01$. *** $p < .001$

Table 2

Mean RT probe performance (SD) in the observational SR binding paradigm

		High trust		Low trust	
		C	IC	C	IC
Experiment 1	Stimulus repetition (SR)	456 (60)	456 (60)	484 (69)	476 (66)
	Stimulus change (SC)	463 (59)	451 (58)	488 (69)	474 (64)
	Δ SC - SR	7* [3.2]	-5 [3.6]	4 [3.0]	-2 [2.9]
	S x R interaction score	12** [3.9]		6 [4.3]	
Experiment 2	Stimulus repetition (SR)	494 (67)	498 (70)	521 (77)	520 (80)
	Stimulus change (SC)	502 (75)	496 (72)	532 (86)	519 (77)
	Δ SC - SR	8** [2.5]	-2 [2.4]	11*** [3.1]	-1 [2.6]
	S x R interaction score	10** [3.4]		12** [4.0]	
Joint analysis	Stimulus repetition (SR)	483 (67)	486 (70)	508 (76)	505 (78)
	Stimulus change (SC)	491 (72)	483 (71)	517 (83)	504 (76)
	Δ SC - SR	8*** [2.0]	-3 [2.0]	9*** [2.3]	-1 [2.0]
	S x R interaction score	11*** [2.7]		10** [3.0]	

Note. C = compatible probe response, IC= incompatible probe response. S x R interaction score = $(\Delta \text{ SC - SR})_{C}$ - $(\Delta \text{ SC - SR})_{IC}$. Positive values reflect the standard pattern of oSRBR effects, i.e. performance benefits for stimulus repetition vs. stimulus change in compatible probe trials, and performance costs for stimulus repetition vs. stimulus change in incompatible probe trials. Standard error of the mean in brackets. * $p < .05$. ** $p < .01$. *** $p < .001$. Asterisks denote that effects significantly differ from zero.

is consistent with the development of distrust for the partner.

Interaction partners were perceived as significantly more trustworthy, reliable, and cooperative in the high trust condition than in the low trust condition in both studies (Table 1). We also calculated the mean of the three ratings to get a single score for how the partner was perceived (Cronbach's α = .95 in Exp. 1, Cronbach's α = .97 in Exp. 2). This trust score was significantly higher in the high trust than in the low trust condition (Table 1), indicating that the trust manipulation was successful.

Memory test

We compared participants' average error rates in the memory tests as a function of trust condition to ensure that both groups adequately attended to and memorized observed prime responses. In Experiment 1, there was no significant difference between trust conditions. However, in Experiment 2, error rates were significantly higher in the low trust compared to the high trust condition (Table 1). This implies that participants in the low trust condition might have been less motivated to observe their partner's responses.

Realism of interaction

In Experiment 1, participants in both trust conditions perceived the interaction as rather unrealistic. In Experiment 2, the interaction was perceived as rather realistic overall. However, ratings were significantly lower in the low trust condition compared to the high trust condition (Table 1).

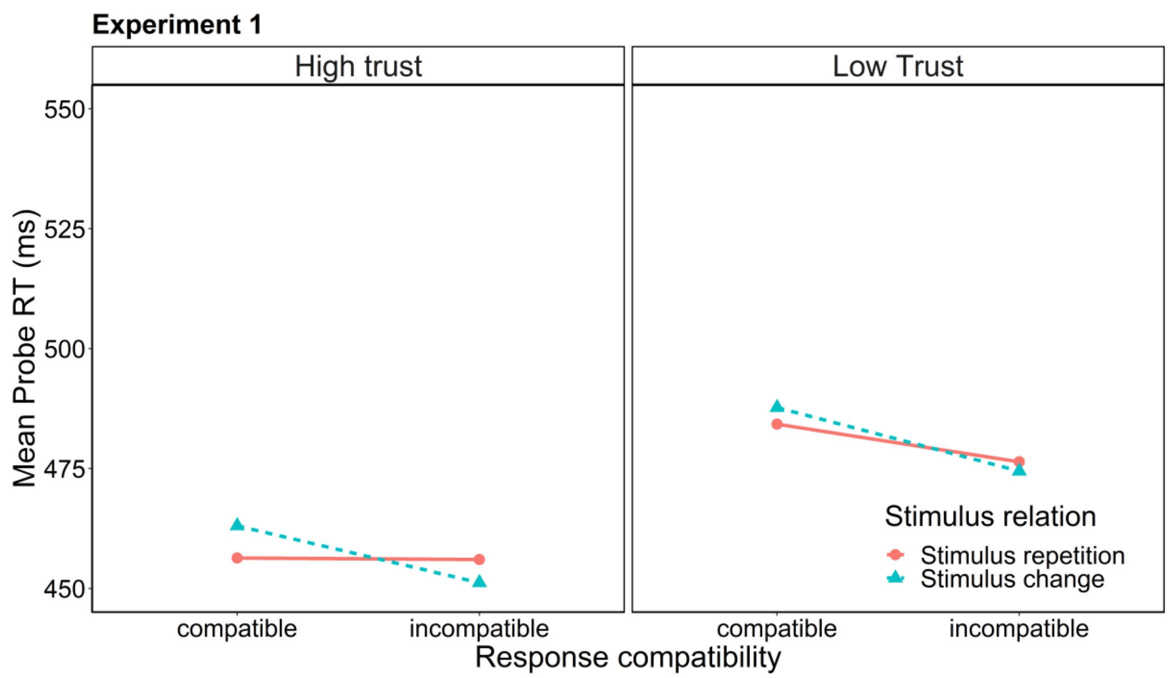
Probe performance

To test our directional hypothesis, effect scores for retrieval of observationally acquired SR bindings were compared between trust conditions using a one-tailed, independent-sample *t*-test. For both experiments, the *t*-test was not significant (Experiment 1: $t(67)=1.04$, $p=.151$, $d=0.25$, $BF_{01}=1.53$; Experiment 2: $t(152)=0.41$, $p=.658$, $d=0.07$, $BF_{01}=7.67$). Contrary to our expectations, effect scores were not significantly larger in the high trust (Experiment 1: $M_{SxR}=11.6$ ms, Experiment 2: $M_{SxR}=10.1$ ms) than in the low trust condition (Experiment 1: $M_{SxR}=5.4$ ms, Experiment 2: $M_{SxR}=12.2$ ms; see Table 2, Figure 3). Follow-up tests showed that effect scores only differed significantly from zero in the high trust condition in Experiment 1 ($t(30)=2.98$, $p=.006$, $d=0.54$, $BF_{10}=7.29$; low trust: $t(37)=1.26$, $p=.216$, $d=0.20$, $BF_{10}=0.36$), which would be in line with our hypothesis. However, in Experiment 2, effect scores differed significantly from zero in both conditions (high trust:

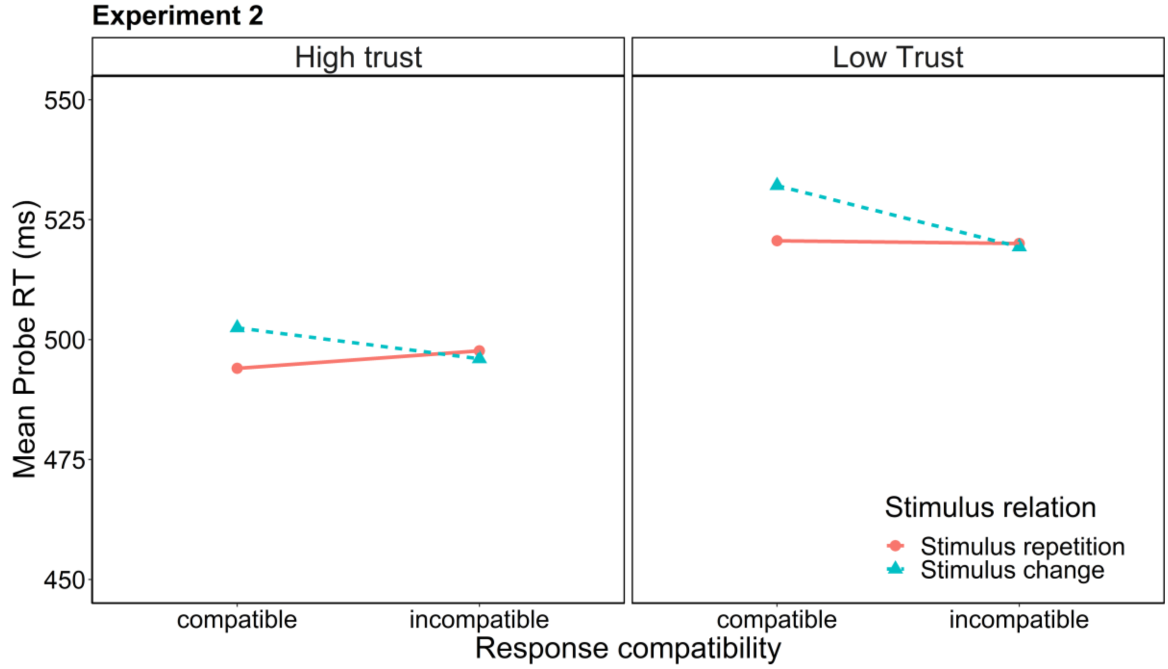
Figure 3

Probe performance (RT) as a function of stimulus relation, response compatibility, and trust condition (a) in Experiment 1 and (b) in Experiment 2

(a)



(b)



$t(78)=2.96$, $p=.004$, $d=0.33$, $BF_{10}=6.82$; low trust: $t(74)=3.05$, $p=.003$, $d=0.35$, $BF_{10}=8.84$).

As the descriptive pattern was in the expected direction in Experiment 1, we ran a joint analysis to exclude the possibility that our tests did not reach significance due to insufficient power. A 2 (trust condition) x 2 (Experiment) factorial ANOVA revealed a significant overall oSRBR effect, $t(222)=5.06$, $p<.001$, but no significant difference between high and low trust condition as the main effect of condition was not significant, $F(1,219)=0.45$, $p=.504$, $\eta^2_p<.01$, $BF_{01}=6.78$. All other effects did not reach significance. Exploratory follow-up tests on the joint data set confirmed that effect scores differed significantly from zero in both trust conditions (high trust: M_{SxR}=10.5 ms, $t(109)=3.93$, $p<.001$, $d=0.37$, BF_{10} = 120.62; low trust: M_{SxR}=9.9 ms, $t(112)=3.28$, $p=.001$, $d=0.31$, $BF_{10}=15.69$). Taken together, these results indicate that observationally acquired SR bindings were always retrieved, independently of whether interaction partners were perceived as trustworthy or not.

Discussion

We examined whether trust in an interaction partner influences retrieval of observationally acquired SR bindings. For both experiments, manipulation checks showed that trust or distrust was successfully induced by the Trust Game. However, this did not impact retrieval of observationally acquired SR bindings: Contrary to what we expected, we did not only find significant oSRBR effects when participants trusted their interaction partner, but also when they did not trust them.

The emergence of oSRBR effects in the low trust condition is particularly surprising as we did not expect that participants would retrieve responses that they had observed in untrustworthy participants. This assumption was based on trust being an important element of positive (Campbell et al., 2010; Eckstein & Cohen, 1998; Hunt et al., 2011) and cooperative (Balliet & van Lange, 2013; Chang et al., 2010; Gambetta, 1988) relationships, and the typically stronger co-representation of another person's actions in positive than in negative relationships (Hommel et al., 2009; Iani et al., 2011; Kuhbandner et al., 2010). However, while for stronger co-representation as measured by the joint Simon effect it may be necessary for the relationship between co-actors to be positive, there is some evidence that this might be different for retrieval of observationally acquired SR bindings: Giesen et al. (2014) found significant oSRBR effects when participants competed for an extra reward, but not when they worked independently, suggesting that negative interdependence also facilitates observational SR binding and retrieval. This differing modulatory influence of negative interdependence/relationships may be accounted for by important structural differences between the joint Simon task used in some of the previous studies and the observational SR binding task used here. In the joint Simon task, two co-actors perform one task together, with each one being responsible for one response alternative. This requires participants to discriminate at each trial whose turn it is to respond. This discrimination is assumed to be more difficult the more the representations of oneself and the other overlap (Dolk et al., 2014; Sebanz et al., 2003). In contrast, in the observational SR binding task, there is no need to differentiate between one's own and the other's response, since participants always respond in turns. Here the impact of the other's actions on one's own performance results from retrieval of observed actions, so the decisive factor is the presence and strength of binding and retrieval processes. It is assumed that actions executed by relevant others receive more attention, leading to a stronger activation of the features of stimuli and responses related to this action (Giesen, 2024), which should benefit both binding and retrieval (Logan, 1988; Moeller & Frings, 2014). While self-other overlap should be selectively increased by the positive relationship implied by trusting one's interaction partner and not by distrust, for attentional processes influencing the strength of binding and retrieval processes it may not matter as much if the other person is considered relevant for positive or negative reasons. Hence, it may be social relevance that counts but not the particular reason for this relevance.

However, to explain our results, this implies that participants perceived their untrustworthy interaction partner just as socially relevant as a trustworthy partner. While we originally did not expect this, it may indeed be plausible to assume this. Especially if someone previously behaved in a way that goes against one's own interests, as was the case for untrustworthy partners in our Trust Game, one might be motivated to pay even more attention to this person's actions in the future. There is indeed research supporting the view that information originating from a source considered untrustworthy are processed particularly deeply. For instance, research on so-called cheater detection (Cosmides & Tooby, 1989, 1992) speculates that memory for cheaters is enhanced (Buchner et al., 2009). Importantly for binding and retrieval processes, this means that the information coming from an untrustworthy person are attended to and processed. Finding that participants with an untrustworthy partner responded significantly slower overall compared to those with a trustworthy partner would fit with the idea that participants attended more to untrustworthy interaction partners.

An alternative simple explanation for the absence of a modulation of oSRBR effects by trust would be that the trust associated with the partner in the Trust Game did not transfer to the observational SR binding task. However, we consider this to be unlikely, because participants with an untrustworthy partner responded significantly slower overall compared to those with a trustworthy partner (see Table S1; Table 2 for means). This implies that the degree of trust induced in the Trust Game did affect their performance in the observational SR binding task. Furthermore, manipulation checks were collected after the observational SR binding task and yielded significant effects in the expected direction.

Limitations

We did not include a neutral condition in which participants neither trusted nor distrusted their partner. It might thus be the case that participants attended more strongly to both, trustworthy as well as untrustworthy interaction partners. In the former case, this would result from benevolence; in the latter case, this would result from a heightened need for austerity towards the interaction partner. Therefore, we cannot tell for sure whether both trust conditions increased oSRBR effects, that would otherwise be absent in a neutral condition. Looking at previous research, what comes closest to a neutral condition can be found in the study by Giesen and Rothermund (2022): Here the belief that the interaction partner was human was sufficient for oSRBR effects to occur without any further social relevance manipulation. However, given that trustworthiness is one of the first assessments made upon meeting an unknown person (Todorov et al., 2015) and research indicating that people tend to trust others by default if they have no reason not to (Berg et al., 1995; Légal et al., 2012; McKnight et al., 1998), it seems unlikely that this condition was truly neutral regarding trust in the interaction partner. Thus, even in this case it cannot be excluded that trust facilitated the emergence of oSRBR effects.

Further, it is possible that the online nature of our study affected our results. So far, all evidence of an influence of social relevance on oSRBR effects comes from studies in the lab, where two people interacted with each other face-to-face (Giesen et al., 2014; Giesen et al., 2018). Although Giesen and Rothermund (2022) found a modulation of oSRBR effects by animacy belief in online interactions, it remains unclear whether these effects are affected by further social manipulations in interactions between two humans (for a more elaborate discussion see Franke et al., 2025). The question whether other’s actions are represented and used for one’s own action regulation the same way in an online context as they are in face-to-face interactions is an issue that should be systematically investigated in future research.

Conclusions

In summary, in both of our experiments, (dis-)trust in the interaction partner was successfully induced by a Trust Game. However, trust did not modulate oSRBR effects. We consider it most likely that, contrary to our expectations, participants in the low trust condition also regarded their interaction partner as socially relevant, leading them to attend to their responses sufficiently for oSRBR effects to occur. For future research, it would be interesting to further investigate the impact of social relevance originating from a negative relationship on retrieval of observational SR bindings. Also, future studies need to ensure that they use a control condition in which the partner is truly considered irrelevant in order to find a modulation.

Contributions

Contributed to conception and design: CGG, KF

Acquisition of data: NK, KF

Analysis and interpretation of data: KF, CGG, NK

Drafted the article: KF

Revised the article: KF, CGG, KR, BH

Approved submitted version for publication: KF, CGG, KR, BH, NK

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Ethics statement

All experiments were in accordance with the Ethical standards of the Institute of Psychology of the University of Jena and the Declaration of Helsinki. For Experiment 1 ethical approval was granted by the Ethics Committee of the University of Jena (FSV 22/031).

Competing interests

We have no competing interests to disclose.

Data accessibility

All data, analysis scripts, and experimental files are available on this paper's project page on the Open Science Framework (https://osf.io/9yj3a/).

Open practices statement

Data, analysis scripts and all materials will be made available after initial acceptance of the paper (link for review: https://osf.io/9yj3a/?view_only=05763ec937ba48e88cf197d3c2f497b4). Both experiments were preregistered (Experiment 1: https://aspredicted.org/995qz.pdf, Experiment 2: https://osf.io/hbrpw/?view_only=9f7021ad2bc6453cac70eb385d807dba).

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