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The Role of Social Identity in Memory Integration

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Master's Thesis (30 hp)
Spring 2021

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Abstract

An adaptive memory system has to support both the encoding of individual episodes as well as generalization and inference across several episodes. The latter of these two goals is achieved by memory integration in which a memory representation is updated to accommodate new information. However, the circumstances leading to either separate encoding or generalization are still unknown. Building on literature implicating social identity in memory processes, this investigation for the first time examined social identity as a moderating factor in that trade-off. In Experiment 1, a common memory integration paradigm was adapted to investigate the influence of ingroup and outgroup source personas that presented one of two overlapping episodes on memory integration. While finding the expected ingroup advantage for memory of individual episodes, there was a surprising outgroup advantage for inferences across episodes. Experiment 2 investigated the influence of social identity in an implicit memory integration paradigm to examine whether the value attached to social identity symbols spreads to neutral stimuli through memory integration. While this mechanism was not evident on a group level, first signs of a behavioral correlate were found in that the extent of decision bias partly depended on the importance of the social identity and the evaluation of the social symbols. In conclusion, this investigation underlines the importance of considering social identity in memory integration and offers implications for the literature on collective memory as well as political discourse.

Keywords: memory integration, social contagion, memory conformity, social identity, inference, episodic memory

On 20 January 2021, newly elected President Biden said these sentences at the beginning of his inaugural address: “A cry for racial justice some 400 years in the making moves us. The dream of justice for all will be deferred no longer.” (Biden, 2021) The symbol of the dream in the context of racial justice may evoke the echo of Martin Luther King’s renowned speech held in 1963. In fact, Biden mentioned his name later. But if we were to quiz somebody who had missed this explicit mention on which historical figure Biden relates to – would she be able to name King? Would her ability to do so depend on whether she shared a social identity such as partisanship with Biden? And if she admired King, would she also see Biden in a more positive light, by mere association?

Answering the first of those three questions, her ability to make the connection would depend on memory integration, a process in which pre-existing memories are updated in the light of new related information. This process exemplifies the flexibility of memory that we rely on in our daily lives as it supports spatial navigation, decision making, and creativity (Schlichting & Preston, 2015). In this case, the speeches of King and Biden would share the symbol of the dream as an overlap, which could result in both episodes being stored in an integrated memory representation.

However, overlap in episodes does not inevitably lead to memory integration. Instead, similar episodes can also be stored separately from each other in a process of pattern separation. However, which exact factors lead to memory integration or separation are still largely unknown (Brunec et al., 2020). Although research has established that social identity modulates both the encoding of new information (e.g., Jeon et al., 2021) and transformation of existing memories (e.g., Andrews & Rapp, 2014), no study so far has investigated its influence on memory integration. Filling this void, this investigation contributes to the understanding of this crucial memory process and can thus answer the second question concerning whether memory integration is facilitated when sharing the social identity of the source of the information.

The last question asked whether Biden could benefit from a shared representation with King. Prior studies have suggested this possibility by creating paradigms in which the value attached to items can spread to neutral objects through memory integration (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019). This process can underly decision making when no prior experience with the choices is available, in which case related items could lend their value to these new options, creating seemingly ungrounded preferences. While prior work has employed monetary values to study the phenomenon, this investigation makes the first attempt to investigate a value spread based on social identity symbols.

Thereby, this strand of research is aligned with real-world circumstances with omnipresent social identity symbols and allows a closer look at the inter-individual variability that has marked prior data.

This absolutely novel connection of memory integration and social identity has the potential to elucidate the fundamental trade-off between memory for single episodes and generalization across events. Furthermore, by introducing memory integration to this social arena, the results allow conclusions concerning the formation of collective memory and political discourse in general.

Memory Integration

Episodic memory supports the recollection of single events from an individual's past and enables humans to mentally travel in time, picture themselves in past situations, and retrieve details from them (Tulving, 2002). It therefore stores specific characteristics of individual experiences, such as Biden's inaugural address, where it could enable individuals to recall topics from the speech or details about the context in which it was delivered. Keeping this representation separate from similar events, such as other speeches by Biden, is important to avoid misattributions and memory errors.

However, in other situations, isolated memories for individual episodes may not be beneficial. For instance, if we want to discuss Biden's general qualities as a speaker or extract overarching themes, generalized representations that take all his speeches into account would be needed. These generalizations should represent commonalities over a class of events and thus allow us to build knowledge over the statistical structure of the environment (Kumaran et al., 2016).

Therefore, there are two divergent goals for the memory system when dealing with similar input: remembering specifics and extracting generalities (O'Reilly & Norman, 2002). According to Complementary Learning Systems theory, this duality between episodic memory and knowledge structures is reflected in neuroanatomy. While the neocortex is believed to be tasked with slowly learning generalities, the hippocampus rapidly encodes separate representations for each episode (O'Reilly & Norman, 2002). More recently, however, evidence has been amounted showing that the hippocampus does not exclusively support episodic memory (Nadel & Hardt, 2011), but is also involved in generalization across episodes (Kumaran et al., 2016; Schapiro et al., 2017). If the same structure pursues both coding schemes (Duncan & Schlichting, 2018), the question arises which of them is pursued in response to similarity between episodes under which circumstances. This question is especially pressing since each coding scheme has its distinct advantages and downsides.

Generalization across episodes and building of knowledge is pursued by memory integration where prior episodes are updated with new and related information from a current episode (Schlichting & Preston, 2015). This integrated representation allows making inferences about new and not directly observed relationships of elements (Shohamy & Wagner, 2008). For instance, in associative inference tasks, two elements from separate episodes (A and C) can be linked because both are related to a third element (B) that connects both episodes with each other (Preston et al., 2004; Zeithamova & Preston, 2010). Referring to the initial example, in a first episode (for instance in a history class), the relation between Martin Luther King (A) and the symbol of a dream (B) is encoded. During the 2021 inauguration, the dream (B) is paired with Joe Biden (C). When memory integration occurs, the overlap in the episodes (B, the dream) leads to both episodes being stored in integrated representations, enabling rapid connection if the relationship between A and C has to be inferred, even though these two items have never been encountered together.

According to the account of integrative encoding, memory integration occurs when a related prior experience (AB) is being reactivated during the encoding of a new episode (BC). This happens due to the shared element of both experiences (B), which may serve as a retrieval cue for the past episode (Gershman et al., 2013; Schlichting & Preston, 2015; Zeithamova et al., 2012; Zeithamova & Bowman, 2020). Therefore, the extent of reactivation of the first episode during encoding of the second one predicts subsequent inference performance, enabled by memory integration (Zeithamova et al., 2012).

Crucially, reactivation of the prior episode reduces the likelihood of memory interference (Chanales et al., 2019). Interference between memories occurs at retrieval when the target memory has to compete with other memories that are similar enough to also be activated (Anderson, 2003). Intuitively, it is easy to remember unique events such as Biden's inauguration, but difficult to remember events that often happen in a similar way, such as all of Biden's campaign speeches. If the old memory trace is reactivated during the encoding of related information, however, interference is reduced and competition is replaced by harmonization through integration (Chanales et al., 2019; Long & Kuhl, 2019). Therefore, integration reduces the risk of forgetting the older memory after encoding a new and similar episode (Kuhl et al., 2010; van Kesteren et al., 2018).

While memory integration and the inferences it supports are largely beneficial, supporting navigation, decision making, and creativity by allowing to flexibly combine past episodes (Schlichting & Preston, 2015), the very characteristics that are so valuable can also lead to memory errors and inaccuracies. If an older episode is updated with new related information, unique aspects of each episode can be misattributed to the wrong event (Varga et al., 2019). This higher prevalence of source memory errors as well as a decrease in episodic specificity has been shown to occur only after successful inference, and can therefore be considered to be a direct consequence of memory integration (Carpenter & Schacter, 2017, 2018b). Therefore, higher flexibility and lower episodic specificity are two sides of the same coin (Schacter & Addis, 2007).

Pattern separation is the other potential reaction to similarity in episodes in which the new content is not integrated, but instead stored separately (Banino et al., 2016). Thus, this encoding of single events preserves the hallmark of episodic memory (Yassa & Stark, 2011), supporting the possibility of mental time travel to specific events. The consequence of pattern separation is lower flexibility as shared features across episodes are not directly connected, as well as a higher risk of interference upon retrieval. Since the several similar episodes were not integrated, they can all be activated by the same retrieval cue, making interference likely (Chanales et al., 2019; Long & Kuhl, 2019). On the other hand, the loss of episodic specificity and overgeneralizations are avoided (Duncan & Schlichting, 2018).

While pattern separation and memory integration serve different goals, they are not mutually exclusive. If separate representations are formed initially, they may become integrated on-demand when inference is necessary (Banino et al., 2016; Carpenter & Schacter, 2017). Therefore, generalization and separation can be employed to differing degrees and changes can occur over time (Zeithamova & Bowman, 2020).

Regardless of the specific downsides and advantages of memory integration and pattern separation, a simple question about these two processes remains. When new episodes share commonalities with prior representations, when is this information integrated and when is it separated? Anatomically, different subfields of the hippocampus have been associated with either process (Duncan & Schlichting, 2018; Schapiro et al., 2017; Schlichting et al., 2014; Zeithamova et al., 2016) and the degree of reinstatement of the prior episode predicts memory integration (Chanales et al., 2019). Behaviorally, however, the circumstances favoring each coding scheme are still largely unknown (Brunec et al., 2020). For instance, dependences on task demands (Richter et al., 2016), as well as a shared context of similar items (Libby et al., 2019) have been documented, but the general understanding of the circumstances is still sparse (see Brunec et al., 2020 for a review). To increase the understanding of factors leading to either coding scheme, this investigation suggested and evaluated social identity as a possible predictor. A large body of prior research elevated social identity to the status of a sizeable influence on individual memory, making an involvement in this fundamental trade-off in memory likely.

Memory Integration and Decision Making

The relevance of memory integration for behavior cannot be overestimated. Apart from navigation, inferential reasoning, and creativity (Schlichting & Preston, 2015), memory integration can also underly decision making and explain ungrounded preferences. Humans often have to make decisions between objects without having prior experiences with the available choices. A small line of research suggests that value may spread between objects through memory integration. Therefore, if neutral objects are integrated with positively or negatively colored items, this value may also be transferred to the neutral object (Carpenter & Schacter, 2018a; Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019).

To explore this possibility, the associative inference paradigm was adapted (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012). In a first step, participants implicitly learned associations between pictures (A) and fractals[1] (B). In the next phase, the same fractals (B) were paired with a symbol of value (C), either a monetary reward or no reward (baseline). Unsurprisingly, participants were more likely to choose rewarded fractals over non-rewarded fractals, an indication of direct learning. The outcome of interest, however, was if participants would also prefer those pictures that were themselves never paired with monetary rewards but were coupled with a fractal that was itself rewarded.

This decision bias was not observed on a group level, but the data was marked by substantial inter-individual variance which suggested that some participants did consistently choose the indirectly rewarded option. Neuroimaging indicated that the amount of reactivation of the picture (A) during encoding of the fractal-money association (BC) predicted later decision bias. This finding implicates integrative encoding in the process, clearly aligning it with the account of memory integration underlying generalization through the reinstatement of the previous episode (Chanales et al., 2019; Long & Kuhl, 2019). These integrated associations can then support a spread of value between items (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012).

Capitalizing on the observation that avoiding loss has a bigger impact on behavior than seeking reward (Tversky & Kahnemann, 1991), Yngve (2019) extended the paradigm by adding a condition of money loss to the previously used reward and baseline conditions. However, no significant decision bias in which participants avoided loss items was found. Furthermore, individual tendency for loss aversion, measured with a gambling task, did not

[1] Fractals are complex stimuli that are made up of several superimposed repetitive patterns. In the studies discussed here, they have a circular form and thus bear resemblance to complex mandalas.

correlate with that decision bias (Yngve, 2019).

While the aforementioned studies used an implicit design in which each association was presented several times for encoding, Carpenter and Schacter (2018a) explicitly instructed participants to integrate across episodes and presented each pair only once. Furthermore, instead of directly using element C as the reward or loss symbol, they presented overlapping pairs of stimuli (AB and BC) in which half of the A-items were associated with a monetary gain. In this design, they found a decision bias, in that participants preferentially chose indirectly rewarded items, but exclusively after successful inference (AC) in an explicit memory test. This finding directly relates to the discussion about temporal factors in memory integration (Zeithamova & Bowman, 2020) and suggests that episodes are first coded separately and integrated only upon demand in this design (Carpenter & Schacter, 2018a).

As part of the exploration of social identity in memory integration, this work investigated if a value spread can be triggered by social identity symbols such as party logos. Not only does this manipulation mirror circumstances outside of the laboratory more closely, but the possibility to measure the value attached to these logos may increase the understanding of the circumstances in which a decision bias may arise.

Social Identity in Memory

In this work, social identity was introduced to memory integration designs for the first time. Thus far, the implicit assumption in this literature has been that cognition and behavior are governed only by an individual's personal identity. In contrast to this, in their influential Social Identity Theory, Tajfel and Turner (1979) argued that behavior and cognition can also be fully or partly influenced by social identities. Social identities are categories that characterize an individual both via similarities with other members of a category as well as via differences to those not part of that group (Hornsey, 2008; Tajfel & Turner, 1979; Turner et al., 1994). According to Social Identity Theory, the main function of these categories is to derive positive self-esteem for the individual from the perceived high status of their ingroup (Tajfel & Turner, 1979).

One of several mechanisms to uphold a high status to derive positive self-esteem is the intergroup bias. This bias can be observed in decisions that favor other ingroup members, even if membership in meaningless groups is randomly assigned within a minimal group paradigm (Hewstone et al., 2002; Hornsey, 2008; Tajfel et al., 1971). Furthermore, and critically for the present investigation, the intergroup bias also leads to more trust (Balliet et al., 2014) and a more positive evaluation (Mullen et al., 1992) of other ingroup members. Both effects have been shown for partisanship (Carlin & Love, 2013; Hernandez & Sarge, 2020), which is the focus of this investigation. Social identity can thus act as a shortcut for attributing trustworthiness and liking when no other information is available. Therefore, in this work, effects of trustworthiness and liking are assumed to indirectly lend support for potential effects of social identity as well.

Social Identity in the Encoding of New Memories

With regards to the strong and consistent ingroup bias, it is not surprising that memory is also affected by social identity. For instance, tapping it can enhance the encoding of new information. Specifically, words are remembered better if they are encoded in reference to one's ingroup, for instance by deciding if an adjective describes it well (e.g., Bennet et al., 2010; Johnson et al., 2002). This group-reference effect is an extension of a similar effect when encoding words in relation to oneself and seems to be of similar magnitude.

Even without explicit encoding strategies, social identity cues that are unrelated to the task can lead to better memory performance. Jeon et al. (2021) established minimal groups and assigned them colors as their group symbols. In the study phase, the colors symbolizing the ingroup or the outgroup were presented together with words but did not have to be encoded and served no apparent function. Those words that were paired with the ingroup cue were more likely to be remembered than words associated with a neutral symbol or the outgroup cue. The authors explained their findings with preferential allocation of attention for ingroup cues (Jeon et al., 2021). Therefore, the mere presence of an ingroup symbol may enhance memory.

Social Identity in the Transformation of Existing Memories

In addition to incidental and effortful ingroup advantages in the encoding of new memories, pre-existing memories can also be influenced and altered in social interactions. As memories enter a stage of lability (Nadel & Hardt, 2011) when they are retrieved, changes can likely occur within a conversation. In the area of forensic psychology, where the memory accuracy of eye witnesses are a focus of concern (Loftus, 1996), discussions among co-witnesses have been identified as a major gateway for inaccuracies (Granhag et al., 2013). These conversations generally lead to increased similarity of witness accounts and reduced general output (Hope & Gabbert, 2019). This influence, termed memory conformity, is higher when the co-witness is a trusted expert (e.g., Williamson et al., 2013) or a friend or romantic partner (Hope et al., 2008). Critically, even if the recall partner is a stranger, the degree of liking predicts the extent of social contagion (Hope et al., 2008).

While memory conformity often uses videos of crime as encoding material, a more controlled approach to mnemonic influences through conversation is taken in social contagion studies. Social contagion in memory refers to the spreading of memory between individuals through a social interaction (Hirst & Echterhoff, 2012) and is usually tested in a design where a participant and a confederate jointly recall a previously studied word list. The confederate will introduce some false items, so that social contagion is conceptualized by the number of false items in the participant's final individual recall (Roediger et al., 2001). Despite this focus on false items, social contagion is not necessarily negative as the general memory output can also increase after cooperation (e.g., Andrews & Rapp, 2014).

Within this paradigm, elevated levels of social contagion in response to trustworthiness, liking and ingroup identity have been documented. The influence of trustworthiness has been shown in studies where participants were less likely to incorporate false items if the recall partner was elderly and thus perceived to be less qualified for a memory task, as long as age was salient or age stereotypes were primed (Davis & Meade, 2013; Meade et al., 2017; Numbers et al., 2019). There was also less social contagion when the partner apparently had consumed alcohol and could thus be trusted less (Thorley & Christiansen, 2018). In contrast, when the partner was perceived as an expert within the field or had better encoding possibilities, more social contagion was the result (Andrews & Rapp, 2014; Koppel et al., 2014). Furthermore, the role of liking has been demonstrated by studies in which participants recalled together with their romantic partner or friend, which led to elevated social contagion in comparison to a recall with a stranger (Peker & Tekcan, 2009). Lastly, social identity has been directly implicated in social contagion. Andrews and Rapp (2014) used a minimal group paradigm and observed that participants were more likely to incorporate false items if the recall partner was part of their ingroup. The authors attributed this effect to higher attributions of trustworthiness and liking due to the ingroup status (Andrews & Rapp, 2014).

Most of the authors in social contagion studies argued that the effects of trustworthiness and liking are exerted through increased or decreased source monitoring. Source monitoring refers to the process of attributing memories to their original context, which includes perceptual and contextual information (Johnson et al., 1993). In this specific case, the crucial contextual attribution is whether a given item was generated by the recall partner or was part of the original study material. It has been argued that participants engage in more active monitoring when they deem the recall partner untrustworthy. Thus, in the final individual retrieval, they are less likely to confuse information from the original material with information provided by the recall partner, resulting in less social contagion (e.g., Andrews & Rapp, 2014).

Interestingly, pre-existing memories are not only altered by addition, as is the case in social contagion, but also by subtraction. Through a specific technique in which some items are selectively retrieved, related items can be actively forgotten (Anderson et al., 1994). This can even be the case when individuals listen to a speaker selectively retrieving items (termed socially shared retrieval-induced forgetting), but only if the speaker belongs to the same ingroup (Barber & Mather, 2012; Coman et al., 2014; Coman & Hirst, 2015; Yamashiro & Hirst, 2019). This is potentially due to a higher motivation to build up a relationship with a fellow ingroup member, which leads to a concurrent retrieval of the items by the listener (e.g., Coman & Hirst, 2015).

This Investigation – Overview

The reviewed literature underlines how the encoding and transformation of memories is influenced by social identity and its correlates trustworthiness and liking. However, no study so far has investigated its influence on memory integration, a gap that this investigation attempted to fill for three reasons. First, with its strong influence on individual memory, social identity could be one of the factors influencing the response to overlapping events, biasing the coding scheme towards memory integration or pattern separation. As these factors are still poorly understood (Brunec et al., 2020), social identity seems too promising a candidate to discount it. Second, cognition and behavior can be influenced not only by a person's individual identity, but also by their social identities. Therefore, whenever information is learned from or in the presence of somebody else, the ease of its integration could be modulated by characteristics of that person, including social identity. Since we are constantly confronted with information from other people, including a tangible other in the paradigm fills a social void in previous investigations and can align theoretical understanding with real-world situations. Third, the literatures focusing on social influences on memory can equally benefit from this investigation, as memory integration is such a fundamental process in cognition. With regards to the many functions that memory integration underlies, including creativity and spatial navigation (Schlichting & Preston, 2015), a potential social influence on this process would dramatically broaden the scope of the phenomenon as a whole.

The current investigation targeted explicit and implicit facets of memory integration and carefully introduced social factors into established paradigms. In Experiment 1, an associative inference task was adapted by introducing a tangible source persona that presented one of the episodes. This allowed to test directly whether social identity biased coding towards memory integration or pattern separation, thus supporting generalization or encoding of specific events. To substantiate this effort and extend the scope of potential influences of social identity in memory integration, Experiment 2 moved from the explicit to the implicit. It was investigated if the value attached to a stimulus can spread to other stimuli through memory integration when social identity symbols are used. This experiment built on prior efforts in this vein which have used monetary rewards and losses as valued stimuli (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019).

While the two studies have different premises, focuses and implications, both tackle the question of social influences in memory integration within designs that require integration across episodes. Furthermore, social identity was targeted by political views in both studies. This choice was motivated by its high importance, and the immediacy of practical implications drawn from the results.

Experiment 1

This experiment aimed to investigate if social identity could explain whether similarity of new information to prior experiences would lead to memory integration or separation. Based on elevated levels of social contagion from ingroup sources, which likens an integration of new information into an existing representation, it was hypothesized that memory integration would be facilitated by an ingroup source. Outgroup information, on the other hand, was expected to be kept more separate from prior knowledge. Due to this expected lack of integration across events, an increased effect of proactive interference was expected in the outgroup condition. To test memory integration, the associative inference paradigm (Preston et al., 2004; Zeithamova & Preston, 2010) was adapted to include a social identity manipulation. Participants studied overlapping associations in separate episodes (AB, BC). Crucially, the BC-associations were presented by a persona that had been established as belonging to either the participant's political ingroup or outgroup, manipulating the factor of source group. To simplify this assignment of group status, this study was conducted in the United States, where the two-party system creates clearer group distinctions than could be expected in a multi-party democracy.

Furthermore, a baseline condition was introduced in which associations consisted of single episodes with only one stimulus pair (XY), not followed by an overlapping episode. The effect of proactive interference was evaluated by comparing memory for these XY-associations, where no interference was possible, with BC-associations in which interference from AB-associations had to be resolved. Furthermore, with this study being the first one to investigate social identity in the associative inference advantage, testing direct memory (XY and BC) also allowed to assess whether the priorly established ingroup memory advantage found its continuation in this new paradigm.

Four memory indicators were collected, including accuracy (i.e., share of correctly chosen target objects in a memory test), reaction times in this choice test and confidence for the choice. Lastly, episodic specificity (i.e., the degree of detail in the memory trace) was measured. Since previous studies have shown that memory integration leads to a loss of episodic specificity (Carpenter & Schacter, 2017; Varga et al., 2019), lower episodic specificity is an indicator of more integrated memory representations.

Apart from testing if a source group effect occurs, as a second objective, this experiment aimed to investigate if the expected inference ingroup advantage and outgroup interference correlated with behavioral measures. Any social identity can differ in importance between individuals and the magnitude of social identity effects in memory has been shown to be related to the importance attached to the salient social identity (Jeon et al., 2021). Therefore, centrality of political views was measured, offering an account of how important this identity was for each participant (Bai, 2020). Congruently, the degree of liking of the persona was expected to correlate with the effect since a relation between liking of a recall partner and mnemonic influence has been shown (Hope et al., 2008).

Methods

Participants

The sample was obtained via the recruitment platform Prolific (www.prolific.co). To increase the clarity of ingroup and outgroup distinctions due to political identity, only US-Americans who self-identified as Republican or Democrat were eligible. Participants were paid in accordance with Prolific guidelines (11.70 GBP). The data was collected in April 2021.

The study was completed by 44 participants, three of which were excluded due to not complying with task instructions and simultaneous chance level memory performance. The resulting sample of $N = 41$ consisted of 24 women, 14 men and three participants indicating a different or no gender. The mean age was 23.73 ($SD = 2.06$). Participants were moderately interested in politics ($M = 4.80$, $SD = 1.21$, this and the following scales ranged from 1-7) and their political views were fairly central to them ($M = 4.55$, $SD = 1.66$). The sample was rather leftist ($M = 2.71$, $SD = 1.50$) and liberal ($M = 2.83$, $SD = 1.55$) with 33 participants identifying as Democrats and eight as Republicans.

Materials

Memory Stimuli. A total of 210 picture triplets (ABC) was constructed, consisting of two common everyday objects (A and C) and a context scene (B). The context scenes had been previously used in Bramão and Johansson (2017) while the objects were retrieved from the Bank of Standardized Stimuli (BOSS), a large inventory of object photographs with ratings of several aspects including familiarity and visual complexity (Brodeur et al., 2010). Pictures of animals, food and weapons were excluded from the pool to minimize differences in memorability. Triplets were constructed randomly but reviewed and objects were replaced if the associations were deemed too obvious. Out of this pool, 120 triplets were randomly chosen for use in the experiment. Each triplet was assigned to one of four lists, which were in turn randomly assigned to one of the four conditions (ingroup, outgroup, ingroup-baseline, outgroup-baseline) for each participant. No significant differences in rated familiarity or visual complexity (Brodeur et al., 2010) existed between lists or position within a triplet (i.e., A vs. C).

Persona Profiles. Two profiles were constructed that introduced the two source personas to participants (see Figure A1 in Appendix A). The general approach of this introduction followed Meade et al. (2017, Study 2) who used extensive profiles of alleged recall partners in which age was the key manipulation, but additional information was used to create a more holistic impression. Two pictures of women with a neutral expression were obtained from the Face Research Lab London Set (DeBruine & Jones, 2017) which had comparable attractiveness ratings and matched in key visual characteristics. Furthermore, written profiles were composed, consisting of six bullet points (age, origin, hobby, party affiliation and eating habits). The key difference was party affiliation with one persona being Democrat and the other being Republican. All the other information was constructed using common stereotypes about each respective partisanship (Shafranek, 2021). The combination of face and profile was counter-balanced across participants.

Political Measures. To assess whether participants chose the persona who shared their political identity, their political views were measured. Apart from one item on interest in politics (Shafranek, 2021), two 7-point bipolar scales from liberal to conservative and left to right were administered to assess political ideology (Jost et al., 2009). Furthermore, participants' party affiliation was measured using a 7-point scale between strong Democrat and strong Republican. To assess the importance of political views, a four-item scale for attitude centrality was used. In its original form, it was designed to measure centrality of ethnicity for Caucasians (Bai, 2020). The items were adapted for the present purpose and exhibited good internal consistency (α = .86).

Furthermore, to assess participants' liking of the personas, the interpersonal liking measure designed to assess evaluations of others (IL-6, α = .97; Veksler & Eden, 2017) was used. One item targetting past interactions was not applicable and thus ommitted. For the resulting five items, participants cast their responses on a bipolar 7-point scale that had the two personas at their end points. These relative liking scores were preferred over absolute ratings for each person to induce stronger liking and magnify the ingroup effect.

Procedure

Due to the ongoing pandemic, the experiment was conducted online. It relied on the survey provider Qualtrics (www.qualtrics.com) and the experimental platform Pavlovia (www.pavlovia.org) based on PsychoPy (Bridges et al., 2020). Participants were instructed several times to minimize all distractions and to complete the experiment in one sitting. The experiment took around 90 minutes to complete.

Questionnaire. The experiment began with a questionnaire on Qualtrics including a consent form, minimal demographic questions, and the aforementioned political measures. Being primed with these political measures (Numbers et al., 2019), participants were introduced to the source personas that served as the ingroup and outgroup manipulation. First, they got acquainted only to the written profiles to reduce the influence of attractiveness perception. Whenever they were ready to continue, they saw the profiles again, but this time with a face accompanying each profile. As attention checks, participants were asked to choose the correct profile for each face and, on the next page, to assign each of the attributes to the correct face. Participants had been warned that they would be quizzed on the information and those who were not able to pass these tests were screened out immediately as recognition of the source personas during the later memory task was questionable.

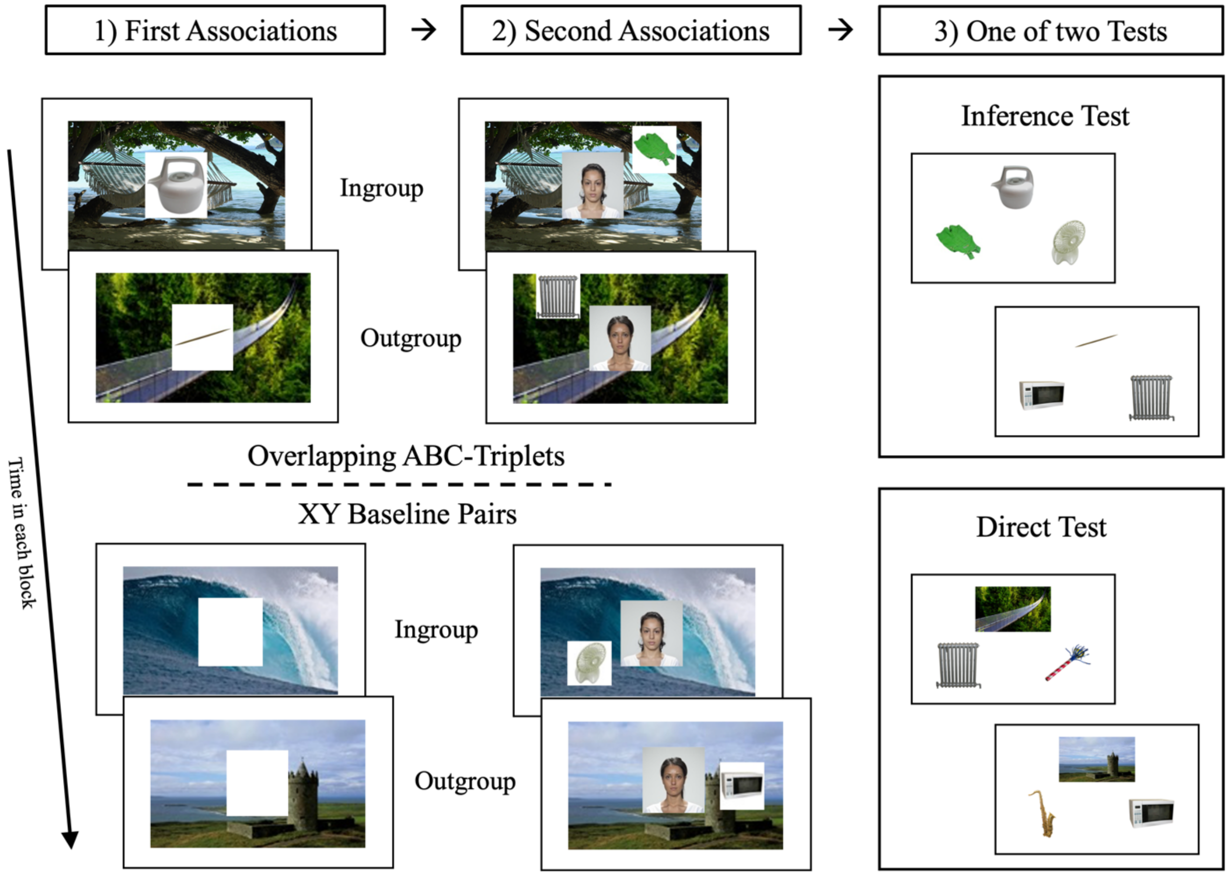
Lastly, liking of the personas was assessed using the IL-6 and a final binary choice in which participants had to decide on one of the personas. This binary choice aimed at amplifying the ingroup perception of the chosen persona by replacing a more differentiated picture with a clear-cut distinction.

After that, participants were redirected to Pavlovia. First, they repeated the binary choice of personas. All choices were consistent between the questionnaire and the experiment. Participants were told that the persona they chose was their team partner during the task and that they would compete against all participants that chose the other persona in order to increase the feeling of intergroup competition.

Memory Task. After a practice phase with extensive instructions, the main experiment was divided into six blocks, each consisting of two encoding phases and one of two memory tests (see Figure 1 for procedure). In every block, 10 ABC triplets and 10 XY pairs were used, each consisting of five ingroup and five outgroup displays. The two encoding phases were introduced to participants as two visits to the same places. They were asked to

Figure 1

Procedure of the Memory Task in Experiment 1



Note. Each block was divided into two association phases and one of two subsequent tests. In the First Associations, 10 objects (A) were associated with a scene (B). Ten baseline trials consisted of a scene (X) with a white square instead of an object. Each display was presented once in random order intermixing the conditions. In the Second Associations, the same scenes (B) were paired with another object (C) and were presented together with a source persona representing either ingroup or outgroup. Baseline objects (Y) were also presented with one of the sources. The objects were placed in a specific location around the source persona. In each block, either the inference test or direct test followed the encoding phases. In the inference test, an object (A) was cued with the target (C) and a distractor that was randomly selected among the baseline objects that had been associated with the same source. In the direct test, studied pairs were tested by cuing a scene (B) with the target (A or C) together with another distractor from the same condition. After that choice, tests for three other memory performance indicators followed (see main text).

imagine themselves in these places to boost memory performance and to memorize everything associated with each place, including the indirect associations across episodes. To ensure attention, participants rated the perceived ease of encoding on a 3-point scale after each display.

In the first association round, all stimuli were presented in random order. Each display consisted of a context scene (B) filling the whole screen and a superimposed object in the centre (A). In the baseline trials, the scene (X) was presented with a superimposed white square. Each scene or object-scene pair was displayed for five seconds. The second association round used the same presentation order so that the same amount of time would elapse between the two overlapping episodes. For two seconds, only the scene (B) and the source persona were displayed to minimize the effects of the face capturing attention and to increase the perception of the persona as the source of the information. Then, the object (C) appeared and stayed on the screen together with the scene and the source for six seconds. Baseline trials looked exactly the same, with an object (Y) and one of the sources superimposed on the scene (X). To be able to measure episodic specificity, each object was randomly displayed in one of 20 positions on a circle around the source and participants were instructed to also memorize the exact position of the object.

After these two encoding rounds, participants were asked to solve math equations for 20 seconds as a distractor task. In half of the six blocks, a direct memory test was administered, while an inference test followed in the other blocks. All participants started with a direct test, followed by an inference test. Participants who failed to reach an above-chance memory performance in the first direct test were screened out for an assumed lack of attention and received partial compensation. After these first two blocks, the test order was counter-balanced across participants to average out potential test order effects.

In the inference test blocks, participants were first cued with an A-object and chose between the target (i.e., the C-object that was displayed with the same scene), and a distractor, which was a randomly selected Y-object from the baseline of the same source group. Thus, ingroup targets were only displayed with baseline objects that had also been presented by the ingroup persona. There was no time limit for this choice. After choosing with a key press, participants indicated confidence on a three-point scale (5 s time limit). Regardless of the choice being right or wrong, the chosen object was then displayed in the centre of the screen and participants were asked to indicate where the object had been placed by clicking on the right location on the circle within six seconds to obtain a measure of episodic specificity. Lastly, there was a source memory test in which participants were asked to indicate which persona presented the object (5 s time limit). The option “I don’t remember” was also available. This test was not used for analysis and merely served to ensure attention for the source personas.

The other blocks were followed by a direct memory test for the direct pairings of objects and scenes by cuing a scene (B or X) and providing the target object as well as a distractor from the same experimental or baseline condition. First, the 10 BC and 10 XY associations were tested in random order, resulting in 20 test trials. The test procedure directly followed that of the inference test with a binary choice, confidence rating, test of episodic specificity, and test for source persona. After that, 10 more trials followed for AB associations which only consisted of the choice and confidence rating, as A-objects were not presented on the circle or by a source persona. Even though AB-performance was not further analyzed, these associations were tested to prevent participants from ignoring them during the encoding phase when they thought a direct test would follow.

After six blocks of the memory task, the IL-6 was repeated to assess changes in relative liking of the ingroup persona. Furthermore, participants were asked to indicate which piece of information from the profiles they based their persona choice on. After that, they were fully debriefed and had the opportunity to report any technical problems or distractions during the experiment.

Ethical Considerations

All materials used in this experiment were of non-emotional nature. No deception was used, and participants could give fully informed consent while being assured that they could terminate the experiment at any time. The recruitment through Prolific relies on anonymous codes so that no data could be matched to an individual person.

Data Analysis

Before data analysis, all trials in which participants took longer than 10 seconds for their initial choice were discarded for an assumed lack of attention (0.59% of all trials). After that, memory accuracy was calculated as the proportion of correct trials for each participant. Since there was little control over what exactly happened if participants answered incorrectly, and because whichever object participants chose was displayed as a cue in the tests for episodic specificity, the analysis of all other indicators was limited on correct trials. For a more robust analysis of the sensitive measure of reaction times, those trials with reaction times outside of the $M \pm 3$ *SD* interval for each participant were discarded (1.88% of all trials in the direct test and 0.98% in the inference test). These trials were not discarded for the analysis of other measures. Episodic specificity was computed for each trial using Euclidian Distance between the midpoint of the initial object position and the point indicated by the participant and could range from 0 (perfect episodic specificity) to .8 (highest deviation).[2]

[2] For programming reasons, participants were theoretically able to click anywhere on the screen. While unrealistic answers were discarded, Euclidian Distance was deemed to be more accurate than degrees as the latter would only make sense if the circle was not variable. However, analyses were also carried out for that value with identical results.

Higher values on this measure therefore indicated lower episodic specificity. Trials were discarded when clicks were recorded in the center of the circle or far away from the circle (1.01% for each test).

All data was analyzed using mixed ANOVAs in which the source group (ingroup vs. outgroup) and other factors in different analyses were within-subject factors. For continuity, the inference test, which had only one factor, was also analyzed using ANOVAs instead of paired *t*-tests. All data was analyzed using R (R Core Team, 2020) and the "ez" package for mixed ANOVAs (Lawrence, 2016).

Results

Choice of Ingroup Persona

The attention checks after the introduction of personas indicated that participants had good knowledge of which face represented which persona: All participants correctly assigned profile to face and 95% of participants assigned each individual attribute correctly. In the binary persona choice, 90% of participants chose the persona that matched their own partisan affiliation even though only 59% indicated politics as the reason for their choice at the end of the experiment. Instead, 24% indicated the persona's hobby, 15% her eating habits and one person made their decision based on the persona's home state. The relative liking scores were moderate before the experiment (M = 1.82, SD = 0.79) and slightly decreased after the task (M = 1.69, SD = 0.96, both scales from 0 – 3) while being highly correlated (r = .79, p < .001).[3]

General Performance

Accuracy in direct tests was close to ceiling (M = .94, SD = .07) with similar values between A-, C-, and Y-items (A: M = .94, SD = .09; C: M = .96, SD = .09; Y: M = .95, SD = .08). Performance in the inference tests was expectedly lower (M = .79, SD = .15). To ensure attention, participants were also tested on the source persona for each object. Performance in that test was generally high within both direct (M = .89, SD = .11) and inference tests (M = .86, SD = .12). This data was not further analyzed.

Rated Ease of Encoding

Rated ease of encoding was analyzed as a meta-mnemonic measure to investigate if participants subjectively rated displays with the ingroup persona as easier to encode than outgroup stimuli. A significant difference between source groups was found ($F(1,40) = 31.74$, $p < .001$, $\eta^2 = .06$). The displays presented with the ingroup persona (M = 1.93, SD = 0.76)

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[3] Due to technical difficulties, post-experimental liking scores were not recorded for three participants.

were rated as easier to encode than those presented with the outgroup persona (*M* = 2.09, *SD* = 0.75), indicating an ingroup bias on this meta-mnemonic measure.

Direct Test

Data from the direct tests was analyzed to investigate the general effect of proactive interference as well as its potential dependence on social identity. To that end, direct test performance on BC associations, which could have suffered under proactive interference from the AB episode, was compared to XY pairs, which were independent of prior episodes. For each memory indicator, a 2 (association type: BC vs. XY) by 2 (source group: ingroup vs. outgroup) ANOVA was computed in which both factors were within-subject.

Accuracy scores were neither affected by association type ($F(1,40) = 0.98$, $p = .328$) nor was there an interaction between association type and source group ($F(1,40) = 1.07$, $p = .307$). However, source group became marginally significant ($F(1,40) = 3.84$, $p = .057$, $\eta^2 = .01$). This below-significance pattern was driven by slightly higher accuracies in the outgroup (*M* = .95, *SD* = .23) compared to the ingroup condition (*M* = .93, *SD* = .25). Therefore, no proactive interference, but merely a marginal outgroup advantage was observed in accuracy.

Interestingly, this trend of accuracy was inversed in the reaction times. While association type ($F(1,40) = 2.33$, $p = .135$) and the interaction ($F(1,40) = 0.37$, $p = .547$) were not significant, source group reached significance ($F(1,40) = 4.26$, $p = .045$, $\eta^2 = .003$). Post-hoc tests revealed that ingroup reaction times (*M* = 2.04, *SD* = 0.95) were significantly faster than outgroup reaction times (*M* = 2.13, *SD* = 1.02). Similarly, on episodic specificity, source group ($F(1,40) = 7.65$, $p = .009$, $\eta^2 = .01$), but not association type ($F(1,40) = 2.93$, $p = .095$) or the interaction ($F(1,40) = 0.68$, $p = .415$) gained significance. Parallel to reaction times, episodic specificity was higher in the ingroup (*M* = .20, *SD* = .23) than the outgroup (*M* = .23, *SD* = .24; note that higher values indicate higher deviations and thus lower episodic specificity). In contrast to this pattern of ingroup advantages, no factor became significant in the analysis of confidence ratings (association type: $F(1,40) = 0.12$, $p = .729$; source group: $F(1,40) = 0.00$, $p = .976$; interaction: $F(1,40) = 1.08$, $p = .305$).

Two points can be concluded from the analysis of the direct test. First, there was no proactive interference in the task as memory performance between XY and BC pairs was comparable. Second, there was a general memory advantage for the ingroup trials manifested in episodic specificity and reaction times. However, a trend suggested that this pattern is inversed in accuracy.

Inference Tests

To investigate if this direct ingroup advantage persisted in memory integration, the analysis was repeated for the inference test. In this test, participants had to recombine overlapping episodes to choose the correct C-object that was indirectly associated with the cue (A-object).

In the one-factor ANOVAs, neither accuracy ($F(1,40) = 1.03$, $p = .316$) nor confidence ($F(1,40) = 2.55$, $p = .118$) differed significantly between source groups. However, the inspection of the means both in accuracy (outgroup: $M = .81$, $SD = .40$; ingroup: $M = .78$, $SD = .41$) and confidence (outgroup: $M = 2.56$, $SD = 0.69$; ingroup: $M = 2.52$, $SD = 0.70$), albeit below significance, indicated a slight advantage for outgroup trials.

This premonition solidified in the analysis of reaction times. On this indicator, the factor of source group reached significance ($F(1,40) = 5.95$, $p = .019$, $\eta^2 = .02$) with reaction times being faster in outgroup trials ($M = 2.76$, $SD = 1.25$) than in ingroup trials ($M = 2.91$, $SD = 1.39$). Thus, a similar accuracy was reached with faster reaction times in the outgroup condition, indicating superior memory integration. This general trend in favor of the outgroup condition was also observed in episodic specificity (outgroup: $M = .21$, $SD = .22$; ingroup: $M = .22$, $SD = .24$), without being significant ($F(1,40) = 1.54$, $p = .222$).

In conclusion, the analysis of direct and inference tests connotes a potential ingroup/outgroup asymmetry between the tests (see Figure 2). While there is a pronounced ingroup superiority in the direct test, the inference test performance is elevated in the outgroup condition. Interestingly, these two trends correlate with each other ($r = .52$, $p < .001$), implying that the advantages go hand in hand and are not caused by two separate groups in the sample.

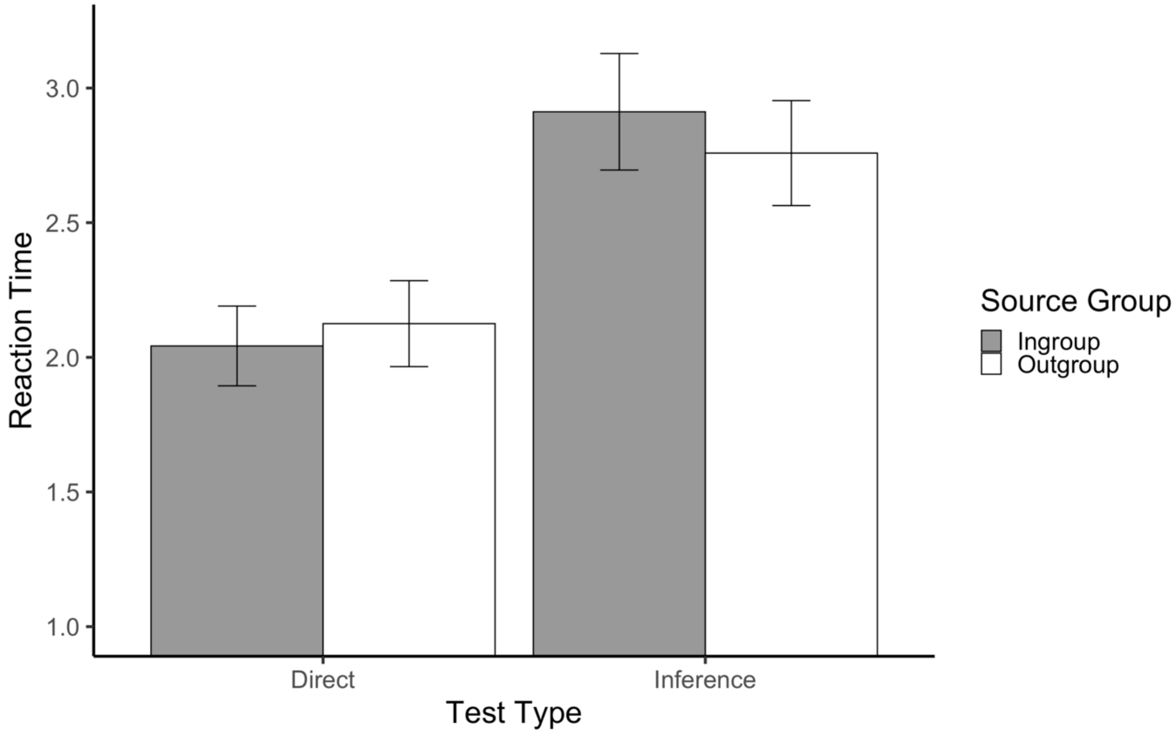
Political Covariates

To understand the expected direct ingroup advantage and the surprising inference outgroup advantage better, correlational analyses with the survey measures were carried out. To this end, bias scores were calculated for each participant. A direct ingroup bias score was calculated using reaction times in the direct test by subtracting the mean in the ingroup condition from the mean in the outgroup condition. This included BC and XY trials since there was no proactive interference. Congruently, an inference outgroup bias was calculated by subtracting reaction times in the outgroup condition from the ingroup condition. Thus, the more positive each value, the stronger the respective bias. The scores were based on reaction times since this was the indicator exhibiting the asymmetry across tests.

Correlational analyses were carried out for centrality of political views, magnitude of partisanship (i.e., deviations from the midpoint of the scale in either direction) as well as pre-experimental and post-experimental liking of the ingroup persona. None of these correlations

Figure 2

Reaction Times across both Tests



Note. In the direct test, reaction times include XY and BC associations, since there was no proactive interference.

gained significance for the direct ingroup or inference outgroup advantage (all $p > .201$).

As an exploratory analysis, the correlations were repeated with that subset of participants who declared to have based their liking decision on the personas' political identities ($n = 22$). Centrality and magnitude of partisanship still failed to reach significance. However, the inference outgroup advantage correlated significantly with pre-experimental ($r = .46$, $p = .027$) and post-experimental ($r = .51$, $p = .013$) liking of the persona. While correlations pointed in the same direction for the direct ingroup advantage, the correlations with liking did not gain significance, but were elevated to a trend after removal of one influential outlier in each correlation (pre-experimental liking: $r = .40$, $p = .065$; post-experimental liking: $r = .38$, $p = .079$).

Exploring Episodic Specificity

The preceding analyses paint an interesting picture. An ingroup advantage in the direct test goes hand in hand with an outgroup advantage on the inference test and the inference outgroup bias depends on post-experimental ingroup-persona liking for those participants basing their decision on politics. This pattern is only evident in reaction times. However, episodic specificity was significantly higher in the ingroup condition on the direct test while no difference was observed in the inference test. Since the prediction was that episodic specificity is reduced after successful memory integration, this could indicate a decrease in episodic specificity in the ingroup condition after successful inference. This pattern could not be detected within the analysis of each test in isolation so that an exploratory analysis across tests was carried out.

Therefore, a 2x2 ANOVA with the factors of source group and test type was computed. However, source group ($F(1,40) = 0.00$, $p = .956$), test type ($F(1,40) = 2.55$, $p = .118$) and the interaction ($F(1,40) = 2.97$, $p = .093$) failed to reach significance. Thus, while the inspection of means from the inference (outgroup: $M = .21$, $SD = .22$; ingroup: $M = .22$, $SD = .24$) and direct tests (outgroup: $M = 0.22$, $SD = 0.23$; ingroup: $M = 0.20$, $SD = 0.22$) could suggest an interaction and thus a decrease in episodic specificity after integration in the ingroup condition, this observation was merely a trend.

Discussion

In a new adaptation of the associative inference task, the effect of ingroup and outgroup sources on memory integration was tested. An interesting and unexpected pattern emerged in the reaction times where direct memory test performance was boosted by an ingroup persona, while inference test performance was elevated in the outgroup condition. Therefore, there are two main conclusions from this research. First, social identity can in fact play a role in biasing the coding scheme towards memory integration. Second, outgroup information is integrated more strongly than ingroup information.

The direct ingroup advantage is fully consistent with prior research in socially modulated transformation (e.g., Andrews & Rapp, 2014; Coman & Hirst, 2015) and encoding (Jeon et al., 2021; Johnson et al., 2002) of memories. In this experiment, this ingroup advantage was found in reaction times, episodic specificity, and even on the meta-mnemonic measure of perceived ease of encoding, which jointly also suggests that the source group manipulation was successful. The curiosity that accuracy was at the same time marginally enhanced in the outgroup condition, is certainly a caveat for this general picture.

In the social contagion and memory conformity literatures, outgroup advantages have not been documented so far, although pre-warnings about an untrustworthy partner can lead to more social contagion under the specific circumstances of a poor memory quality and dominance of the recall partner (Muller & Hirst, 2010). Nevertheless, in this experiment, all indicators unanimously exhibited an inference outgroup advantage, albeit only gaining significance in reaction times. Therefore, it can be concluded that pre-existing memory representations are more likely to be updated if the new information stems from the outgroup.

While the effect in reaction times was small, this discovery is of significant value for the memory integration literature. In the unsolved puzzle of when similar information becomes integrated or separated (Brunec et al., 2020), social identity could be another factor alongside reinstatement at encoding (Chanales et al., 2019), task demands (Richter et al., 2016), and shared context (Libby et al., 2019) and therefore add predictive value to future models. However, the lack of a significant difference in accuracy commands that conclusions are drawn with caution. Ingroup information was still integrated but, as the response times suggest, to a lesser extent.

Surprisingly, no proactive interference was observed in this experiment and memory performance was not affected by previous overlapping episodes. Memory for BC associations, where the overlapping AB association could have competed for representation, was equal to memory for non-overlapping XY associations. This can most likely be explained by the low difficulty of the task for the participants, which at-ceiling performance for direct associations testifies for. As a result, the role of social identity in proactive interference could not be investigated, limiting the interpretations of this work on different extents of memory integration instead of drawing conclusions for a trade-off between integration and separation.

The overall direct ingroup advantage and inference outgroup advantage were significantly correlated. This suggests that a joint mechanism is responsible for this pattern. As this experiment is the first one to document the present asymmetry, all explanations of the effect must be speculative. Two possible explanations are presented here, basing on differences in reinstatement as well as individuation.

The first explanation targets reinstatement during encoding. It has been shown that the degree of reinstatement of the prior episode during encoding of the new and related information correlates with subsequent memory integration and serves to harmonize both episodes (Chanales et al., 2019; Long & Kuhl, 2019; Zeithamova et al., 2012). Therefore, differences in inference performance between source groups may indicate that more reinstatement took place in the outgroup condition, leading to better memory integration.

Two not mutually exclusive factors could underly this potential lack of reinstatement, which causes a lack of integration. First, it is established that people trust fellow ingroup members more than outgroup individuals (Balliet et al., 2014; Carlin & Love, 2013). Participants could have deemed the outgroup persona unreliable, thus triggering a need to fact-check the information provided by her. The only way to do that in this paradigm where all information is necessarily correct is to retrieve the prior overlapping episode, thus reinstating it and promoting memory integration. Ingroup information, however, was trusted and did not have to be fact-checked, freeing resources for encoding the present information in the best possible way. Therefore, participants were in a retrieval mode in the outgroup condition and an encoding mode in the ingroup condition, leading to more reinstatement and thus integration in the former case.

This explanation converges with the mechanism assumed to be responsible for less social contagion and memory conformity when working with seemingly untrustworthy or outgroup partners. Several studies have argued that participants monitor the source more closely during the conversation and subsequent retrieval if they worked with an outgroup or disliked and distrusted partner and are therefore better at rejecting false items in the final recall (e.g., Andrews & Rapp, 2014; Davis & Meade, 2013; Koppel et al., 2014; Meade et al., 2017; Numbers et al., 2019). As outgroup information could not be checked directly in this design, participants may have implicitly resorted to the previous episode as the only opportunity for fact-checking.

While the lack of trust may be based on a general distrust of any outgroup, it may also be a specific consequence of partisanship. In a polarized and sometimes hostile political climate, the opposing partisanship could have acted as a prewarning not to trust the outgroup. The age of the recall partner has been shown to act in a similar way and, out of a distrust for the mnemonic capacities of elderly people, can reduce social contagion (Davis & Meade, 2013; Meade et al., 2017; Numbers et al., 2019).

Apart from these effects of trustworthiness, a second factor may have contributed to the lack of reinstatement in the ingroup condition. Prior research has tied superior memory for ingroup-related stimuli to elevated attention for them (van Bavel & Cunningham, 2012). Elevated attention has also been argued to be the cause for incidental memory advantages for stimuli that were cued with ingroup symbols (Jeon et al., 2021). It is therefore possible that participants exhibited high attention in those episodes that were presented by the ingroup. This elevated attention was naturally beneficial for the direct test performance that concerned only the ingroup episode at hand. However, it could have come at a cost. The more intense focus on the current episode may have led to less conscious or automatic reinstatement of the previous episode, thus triggering an encoding mode over a retrieval mode. This lack of reinstatement then led to less integrated representations and therefore slower responses in the inference test.

These explanations of trustworthiness and attention are well compatible with each other. However, they remain speculative until additional data has been gathered. Neuroimaging is needed to support the premise of both explanations that less reinstatement occurs in the ingroup than in the outgroup. Eye tracking can furthermore help to understand the deployment of attention during the trials. Lastly, it should be stressed that these explanations are not to be misunderstood as oversimplifications in that there is no reinstatement in the ingroup trials. Since there were no differences in performance between source group conditions, it is likely that reinstatement and integration did occur, but potentially to a lesser extent, leading to the observed pattern in reaction times.

Another explanation that does not embroil differences in reinstatement is conceivable. Instead, it relies on differing degrees of individuation between ingroup and outgroup. Some research shows that if there is intergroup competition, individuals are more likely to organize memory on a group level instead of representing outgroup individuals separately (Brewer et al., 1995). Therefore, ingroup episodes could be encoded in highly individuated separate traces while outgroup episodes were encoded with a smaller degree of resolution. This would then have had the effect of enhanced ability of recombining episodes in the outgroup trials.

However, the opposite case could also be made. One of the pillars of Self-Categorization Theory (Turner et al., 1994) is that upon activation of a social identity, in a shift from "I" to "We", people perceive themselves more deindividualized as part of their ingroup. This could have led to less resolution and resulted in a source persona-based integration with all episodes from the ingroup condition integrated into one big representation. This excess of memory integration could have made it more difficult to retrieve the correct object, especially since the distractor in each test was from the same source persona condition, while more separately stored outgroup traces would have been an advantage.

Which of these two speculative accounts of individuation is better suited to explain the source group asymmetry within memory integration cannot be determined conclusively. However, to investigate the idea of a source persona-based integration, a surprise test could be introduced in which participants need to associate objects presented by the same source persona. Furthermore, since deindividuation of the outgroup depends on competition between groups (Brewer et al., 1995), this factor could be manipulated in the instructions or by using different social groups that stand in differing degrees of competition.

The preceding explanations based on the asymmetry found in reaction times while accuracy and confidence were not responsive to the manipulation. Episodic specificity, however, was higher in the ingroup condition on the direct test, which is consistent with the general ingroup advantage. In the exploratory analysis, however a non-significant decrease in episodic specificity was observed after memory integration, which is consistent with the initial expectations (e.g., Carpenter & Schacter, 2017). While the non-significance of this decrease in the ingroup condition is potentially simply due to missing power, the absence of an effect in the outgroup condition may be explained by the lower episodic specificity already before integration, which precluded a further decrease. Generally, however, this test may have been easier than in earlier studies that used it (Tompary et al., 2020) due to the superimposition of the object on a scene which could make encoding easier than it is with a circle in a void.

A last result was that pre- and post-experimental liking significantly correlated with the inference outgroup advantage and marginally correlated with the direct ingroup advantage for those participants who chose their persona on political grounds. These correlations are in line with previously reported relationships between magnitude of liking with social contagion (Hope et al., 2008) and self-investment in the ingroup with an incidental ingroup memory advantage (Jeon et al., 2021). They express the intuitive idea that effects of social identity should be stronger if the social identity is generally more important and if the source persona is more liked. The correlations were only found in the subset that based their persona choice on politics, after having been primed with it extensively, magnifying the potential impact. For the other participants, the group distinction was based on a hobby or eating habits, both of which are – if not unimportant – probably less emotionally laden and less influential social identities. These correlations are therefore further corroborating the role of social identity in memory integration.

In conclusion, the asymmetry of ingroup and outgroup advantages between the direct and inference test is a new and surprising finding that underlines the value of social identity as a moderator of memory integration. The possible explanations that were explored include a lack of reinstatement in ingroup trials due to elevated attention or higher levels of trustworthiness as well as differences in individuation across groups will have to be tested more specifically by further research.

Limitations

Apart from the already discussed easiness of the task that could be tackled by stimuli that are more difficult to encode like abstract words, shorter encoding times or more items per block, the utilized source group manipulation was the most delicate part of the study. The approach inspired by Meade et al. (2017, Study 2) with holistic profiles that include more information than the target manipulation caused two main problems. First, it is unclear whether participants really perceived the personas as the origin or even source of the information. Out of ethical considerations, participants were not misled with a cover story that explicitly described personas as participants of a prior study who generated the information they presented. Instead, the personas were merely included in the encoding story, while also instructing participants to imagine actually meeting the persona in each scene. Thus, the manipulation blurred the lines between a real source social identity effect that this study was after, and a mere incidental encoding advantage in the presence of the ingroup (Jeon et al., 2021).

Second, the more complete profiles which included more information than the key manipulation of political identity obscured the demarcation between liking and social identity. The choice of personas by each participant may have depended on the pictures or any information from the profile. Even though 90% of participants chose the persona that matched their partisanship, less than two thirds declared that they made their choice based on politics. However, this lack of demarcation may not be problematic, given that most prior studies have explained the ingroup advantages by means of trustworthiness, liking or relational motives (e.g., Andrews & Rapp, 2014). Nevertheless, future studies should use cleaner manipulations that isolate one of the factors to identify the boundary conditions of the present effect.

A last limitation pertains to the online setting of the study. While participants were instructed several times to minimize all distractions, their behavior during the experiment is unknown. Not only are general attention and distractions beyond control, but so is technical equipment with differing screen sizes and qualities. Furthermore, while laboratory experiments with student samples are often conducted during the day, many participants may have chosen to complete the current experiment in the evening, which may have increased the level of fatigue in a time of studying and working from home. While none of these factors would systematically bias the data, they introduce noise, which a laboratory setting would minimize.

Future Research

This investigation aimed at contributing to the debate of factors that facilitate pattern separation and memory integration. While an influence of social identity on integration was found, the lack of proactive interference in the design precluded conclusions for pattern separation. A more demanding memory task with sizeable levels of proactive interference would allow this. Furthermore, it is vital to understand the neural processes of the social identity influence, which calls for the use of neuroimaging. This would also allow to assess if there was less reinstatement in the ingroup condition. Neuroimaging could also address the question of how social identity modulates memory integration. One region of special interest could be the medial prefrontal cortex, which is strongly involved in memory integration

(Schlichting & Preston, 2015), and has also been implicated in ingroup favoritism (Volz et al., 2009). Lastly, eye tracking could elucidate the deployment of attention during the encoding phases and uncover potential differences between ingroup and outgroup displays.

The blurring of source perception and incidental effects are of stronger interest for the social contagion literature and could be continued to be tackled in purely behavioral designs. To increase the perception of the personas as the source, auditory stimuli from two distinct voices as well as a stronger cover story within the ethical limits of deception could prove useful. As a complementary approach, a decrease in source perception could help to test the basis of the present effect. This could be achieved by replacing the personas with social identity symbols such as party logos. Similarly, future efforts could focus on a demarcation of social identity from liking and trustworthiness by using sparse profiles in which only the target identity of each persona is revealed, or by replicating the study to isolate liking by using pictures of participants' friends or romantic partners instead of the ingroup persona and a stranger in lieu of the outgroup persona.

A striking detail about the effect is that the material was not political or controversial but consisted of neutral objects. This raises the question whether the effect would be stronger if the material was related to the social identity or if, on the contrary, message characteristics play a bigger role than the source persona. A design by Varga and Bauer (2017) may prove useful to that end, in which the overlapping episodes consisted of two facts that could be integrated to a self-derived fact. Those facts can be easily adapted for political messages that could be either uncontentious or polarizing to partisans from opposing sides, allowing to investigate if message content moderates the asymmetric effect.

Experiment 2

In Experiment 1, it was established that social identity of a source modulates memory integration in an explicit paradigm. This finding provided the leeway to extend this investigation to an implicit sphere by adapting an established design (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019) to investigate if value can spread between items within an integrated memory representation when value is provided by social identity symbols. This mechanism could underly a decision bias in which preferences are observed for items that have never directly been associated with value but were associated with other stimuli that have been directly linked to value. Thus, the role of social identity was investigated in one of the more intriguing direct behavioral consequences of memory integration (Schlichting & Preston, 2015).

The crucial alteration in this experiment compared to prior studies was that value was induced to stimuli not through monetary rewards (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012) and losses (Yngve, 2019) but through social identity symbols, namely party logos. Specifically, the logos of each participant's favorite party served as the ingroup symbol in lieu of a monetary reward, while the logo of the least favorite party was used as the outgroup symbol, replacing monetary loss. These symbols (C) were consistently paired with direct stimuli (B) that had been associated with indirect stimuli before (A).

The main outcome measure were pairwise decisions between two stimuli from two different conditions that formed three comparisons (ingroup vs. baseline; baseline vs. outgroup; ingroup vs. outgroup). Each of these comparisons were formed for direct stimuli which were themselves paired with the social identity symbols, where the choices informed a value of direct learning. Crucially, to measure a decision bias based on the spread of value through memory integration, indirect stimuli that were not directly presented with value were also pitched against each other for decision.

Apart from the positive results of Experiment 1 that suggested the effectiveness of social identity in memory integration, two more motivations guided this venture. First, we are constantly confronted with social identity symbols like coat of arms, national flags, symbols of civil society movements, or party logos. Therefore, by replacing monetary value with social identity symbols, real-world environments are mirrored more closely, hence dramatically broadening the scope of the phenomenon.

Second, as discussed earlier, previous studies have not found a decision bias on a group level whilst being confronted with strong inter-individual variance (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019). One potential reason which has not been explored yet is that employing monetary rewards and losses as a conditioning stimulus may be the very reason for this variance. Depending on perceived importance of money (e.g., being stingy or generous) as well as one's own financial situation, gaining a dollar may have dramatically different meanings and evoke reactions between strong joy and a mere twitch of the eyebrow. Using social identity symbols allowed to measure the perceived importance of the social identity as a whole (measured as centrality in this paradigm) and the value attached to the utilized social identity symbols. These two dimensions were expected to explain some of the inter-individual variance and thus give indications for the necessary conditions of a decision bias.

Methods

Participants

Participants were recruited through convenience sampling, aided by several posts on social media. No compensation was offered, but participants were able to enter a raffle for eight gift cards of 99 SEK each. Swedish and German citizens or long-term residents were eligible for participation as a separate version for each country was provided. They were also required to understand English since both versions of the experiment were in English. The study ran in April 2021.

After exclusion of one participant who experienced technical difficulties in the decision phase, $N = 49$ participants completed the experiment of which 38 took the German and 11 took the Swedish version. The mean age was 27.06 ($SD = 3.86$) and 30 participants were female, 18 were male, and one participant did not indicate their gender.

The sample was generally interested in politics ($M = 4.90$, $SD = 1.45$) and predominantly leftist ($M = 2.57$, $SD = 0.82$) and liberal ($M = 2.33$, $SD = 0.85$, all on scales from 1-7). This political orientation also manifested in their choices for most and least liked party with all but two participants choosing green or left-wing parties as their preferred one and also all but two participants choosing the respective right-wing party as their outgroup party (see Table B1 in Appendix B).

Materials

To construct the triplets for the characteristic ABC-structure of memory integration, the experiment used scenes as indirect stimuli (A), fractals as direct stimuli (B) and party logos as social identity symbols (C). Sixty scenes from Bramão and Johansson (2017), and the 30 fractals that had been used by Yngve (2019) were obtained for this purpose. The party logos of all German and Swedish parties represented in parliament were downloaded from their websites. All pictures were under a Creative Commons License. The construction of triplets was individual for each participant and depended on their ratings before the encoding phase of the experiment. Furthermore, the same political measures as in Experiment 1 were used in the questionnaire (Centrality [$\alpha = .91$], interest in politics, political ideology).

Procedure

Due to the ongoing pandemic, the experiment was conducted online with the same set-up as Experiment 1 using Qualtrics for the questionnaire and Pavlovia for the memory task. Participants were asked to minimize all distractions. The experiment took around one hour.

Questionnaire and Political Measures. Participants received a link to an online questionnaire. After a consent form and few demographic questions, they were shown the logos of all parties represented in the Swedish or German national parliament at the time of data collection. Participants were asked to indicate for each logo if they were familiar with it and, on the next page, to write down the name of the party it belongs to. Both questions served as a quality check and no participants were excluded on their grounds. Participants also indicated liking of each party on sliding scales from 0 to 100 and chose both their most liked and least liked party to determine the ingroup and outgroup symbols. The value difference was calculated by subtracting the rating of ingroup party from the rating of the outgroup party. Next, participants completed the measures of interest in politics, political ideology, and centrality of political attitudes. Upon termination of this questionnaire, they were redirected to the memory task on Pavlovia.

Stimulus Ratings and Triplet Construction. After the redirection, participants repeated their choices of most and least favorite parties. To determine the final set of experimental stimuli (A and B), participants were asked to rate all 60 scenes and 30 fractals on a scale from 1 – 5 with increasing pleasantness. Stimuli rated neutrally or close to neutral were used for the subsequent phases to limit the influence of strong pre-existing opinions that could interfere with the manipulation.

The scenes (A; indirect stimuli) and fractals (B; direct stimuli) were randomly combined to form pairs that were presented in the association phase. Twelve of these 18 pairs were also randomly assigned to one of three conditions through association with the ingroup symbol, outgroup symbol, or a white square acting as the baseline. The remaining six pairs served as dummies that were only displayed in the association phase.

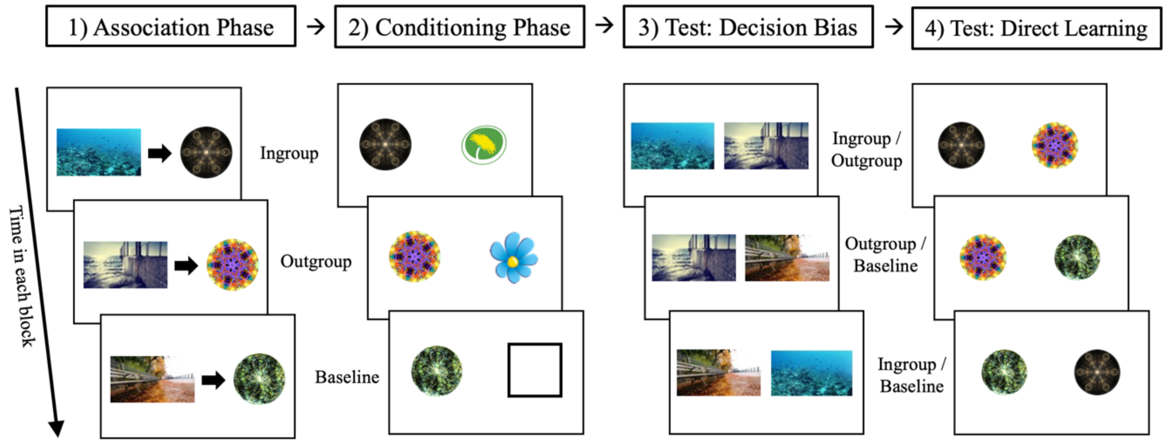
Association Phase. In the association phase, participants implicitly learned associations between the indirect and direct stimuli (see Figure 3). It was not revealed to them that stimuli were paired consistently. After a fixation cross (1 s), each pair was presented sequentially starting with the scene and followed by the fractal (each presented for 1.75 s). Each pair was followed by 500 ms of a blank screen. All experimental stimuli had a white frame while all dummies had a green frame around them. To ensure attention, participants were asked to respond to the frame color by alternate button presses and were also instructed to respond to the fractals with a button press.

The presentation order was randomized within 12 blocks. Each block contained all experimental couples as well as three of the dummy pairs. Thus, each target pair was presented 12 times and each dummy pair was presented six times in total. Participants could take a self-paced break after every other block.

Conditioning Phase. In this phase, the same direct stimuli were coupled with the three symbols. Unlike the association phase, participants were told about the consistent pairings and were explicitly instructed to learn them. In each trial, after a fixation cross for one second, the fractal was presented first on the left side of the screen for 1.75 s before the

Figure 3

Procedure of Experiment 2



Note. The figure schematically depicts the procedure of the association and test phases. The choice of party logos in this figure is based on the most typical ingroup and outgroup choices in the sample. In the test phases, no stimulus was actually presented in more than one comparison, the repetitions are for simplicity. See main text for explanation of the procedure.

symbol was added on the right side of the screen for another 1.75 s. This semi-sequential presentation was used to increase the perceived coupling of both stimuli while making sure that participants did not only pay attention to the symbol to which they had to respond with alternate button presses. Each association was presented 18 times in pseudo-random order and participants could take a self-paced break after every third block.

Decision Phase. In this phase, participants made binary decisions between two indirect stimuli that had been encountered in the association phase and were associated with a social identity symbol through the direct stimulus. The three conditions formed three comparisons (ingroup vs. baseline; baseline vs. outgroup; ingroup vs. outgroup). Each scene was only used for one type of comparison, leaving four different configurations of stimuli for each comparison. Each decision was repeated four times with counter-balanced stimulus positions, totaling 16 trials per comparison. Participants had five seconds to make their choice. They were told that based on their prior behavior during the experiment, one picture always matched their political profile more closely and that they should try to choose that option. The same procedure was repeated for the direct stimuli in order to measure the success of the conditioning phase.

Repeated Picture Ratings. Lastly, the 12 direct and indirect stimuli used in the experiment were presented for another judgement mirroring the initial liking ratings. This data was used to assess changes in stimulus liking in response to symbol condition. The experiment ended with a debrief and the possibility to enter an e-mail address to participate in the raffle.

Ethical Considerations

Participants were briefed in the consent form that the experiment was of cognitive nature and measured reaction times. The fact that memory integration was the concept of interest could not be revealed to preserve the implicit nature of the association phase. However, participants were fully debriefed about this at the end of the experiment. The stimulus material was fully non-emotional, and participants were told that they could end the experiment at any point. The online setting and full anonymity with minimal demographic information, which could also be skipped, made it impossible to match data to any particular person. Furthermore, e-mail addresses were collected and stored independently.

Data Analysis

Each of the three comparisons (ingroup vs. baseline, baseline vs. outgroup, ingroup vs. outgroup) consisted of 16 trials which were coded with "1" if the choice was in the predicted direction, so the ingroup option in the ingroup vs. baseline and ingroup vs. outgroup comparisons, and the baseline option in the comparison between baseline and outgroup. If the participant made the opposite choice or failed to respond within five seconds, the trial was coded with "0". The mean of those decisions was computed for each comparison and participant. These means informed the decision bias for choices between indirect stimuli and a direct learning score for choices between direct stimuli. All data was analyzed in R (R Core Team, 2020) using the "ez" package for mixed ANOVAs (Lawrence, 2016).

Results

Participants' performance in the cover tasks during the encoding phases indicate that attention was given. During the association phase, there was a total of 95.23% correct button presses and during the conditioning phase, there were 95.80% correct responses. Furthermore, due to the preferential selection of neutrally rated stimuli for the experiment, no differences between the conditions were found in scenes (p = .335) or fractals (p = .594) prior to the experiment.

Decision Behavior

To investigate if participants were more likely to choose those stimuli that were directly or indirectly associated with the ingroup symbol and avoid those related to the outgroup symbol, participants' decisions were analyzed within a 2 (decision type: decision bias, direct learning) by 3 (comparison: ingroup vs. baseline, baseline vs. outgroup, ingroup vs. outgroup) ANOVA. This ANOVA revealed a main effect of decision type (F(1, 48) =

25.96, $p < .001$) but neither a main effect of comparison ($F(2, 96) = 1.46$, $p = .238$) nor an interaction ($F(2, 96) = 0.43$, $p = .649$). Post-hoc tests indicated that the effect of decision type was driven by more predicted choices in direct learning ($M = .72$, $SD = .30$) than within the decision bias ($M = .51$, $SD = .31$).

To assess if the decision bias and direct learning in each comparison were different from chance level, six follow-up one-sample *t*-tests against chance level ($\mu = 0.5$) were carried out with Bonferroni-corrections. As can be seen in Figure 4, the decision bias did not differ from chance level in any comparison (all $p = 1$). Direct learning, on the other hand was observed in the ingroup vs. baseline ($M = .70$, $SD = .31$, $t(48) = 4.41$, $p < .001$), baseline vs. outgroup ($M = .69$, $SD = .31$, $t(48) = 4.32$, $p < .001$), and ingroup vs. outgroup ($M = .77$, $SD = .28$, $t(48) = 6.86$, $p < .001$) comparisons in the expected directions.

In conclusion, the results suggest that direct learning has taken place since participants made predicted choices above chance level for direct stimuli. This value, however, did not spread to overlapping episodes as no decision bias was found on a group level. Nevertheless, as Figure 4 demonstrates, the data is marked by strong inter-individual variance and some individuals do indeed show a tendency towards a decision bias. These impressions were supported in the analysis of stimulus ratings where significant changes only occurred in direct learning but not as a decision bias (see Text B1 and Figure B1 in Appendix B for analysis).

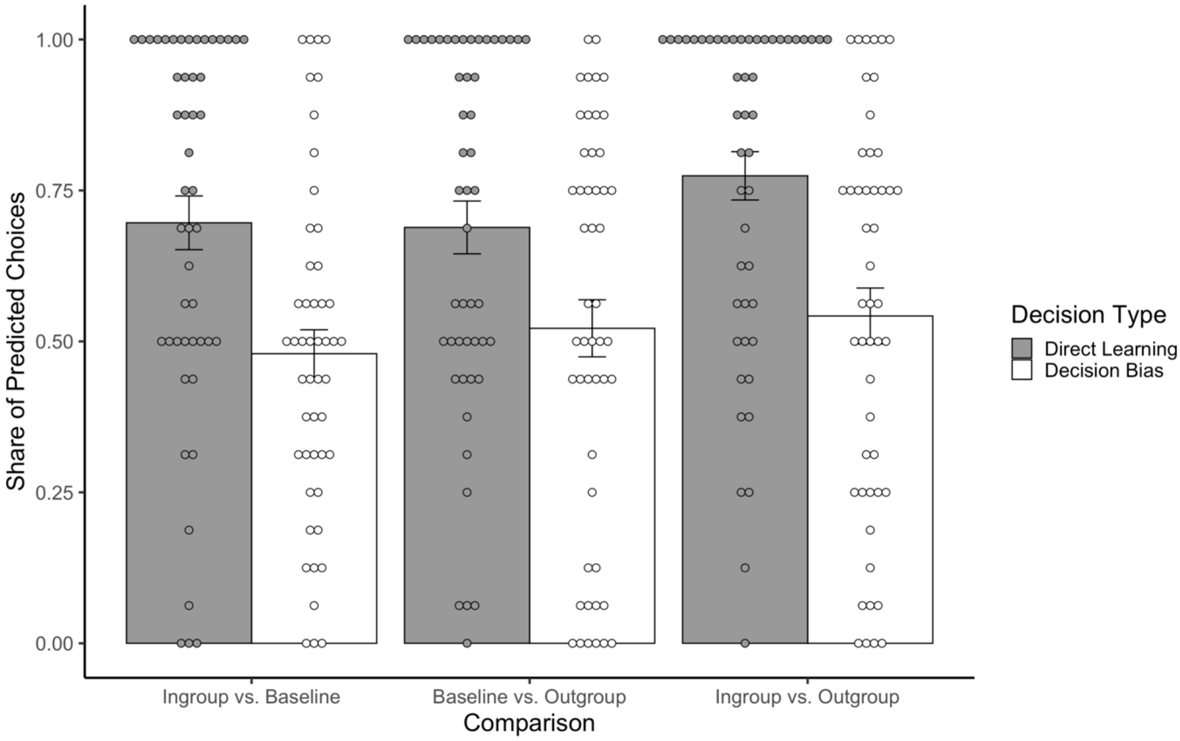
Political Covariates

The second objective of this experiment was finding behavioral correlates of the decision bias, especially focusing on the centrality of political views and the value difference. The latter was obtained by subtracting the rating of the outgroup party from the rating of the ingroup party to capture the relative desirability of both the ingroup and outgroup option. Value attached to the ingroup party was relatively high but more variable ($M = 81.10$, $SD = 9.80$) than value attached to the outgroup party which was uniformly at floor levels ($M = 1.76$, $SD = 4.49$, both on a scale from 0-100). Thus, the value difference ($M = 79.35$, $SD = 11.30$) was dominated by the rating of the ingroup party.

To assess if the magnitude of the decision bias correlated with centrality or the value difference, correlational analyses were carried out. None of these correlations gained significance (all $p > .290$). However, to account for potential interactions between centrality and value difference, both were entered simultaneously into linear regressions for each decision bias. To counter high levels of multicollinearity, the variables were scaled. In the ingroup vs. outgroup comparison, no factor gained significance. For the ingroup vs. baseline comparison, however, a marginally significant interaction was found which gained

Figure 4

Direct Learning and Decision Bias by Comparison on a Group and Subject Level



significance after removal of one outlier (β = .31, *p* = .040). Centrality (*p* = .142) and value difference (*p* = .776) did not gain significance in their own right.

When regressing the baseline vs. outgroup bias on centrality and value difference, the interaction between both factors gained significance (β = -.29, *p* = .047) while centrality gained marginal significance (β = .24, *p* = .096). The interaction was negative due to the negative slope of value difference (β = -.04, *p* = .798). This pattern surprisingly suggests that when both centrality and value difference increase, there is a lower tendency to choose the predicted baseline option. An increase in centrality alone, however, tends to promote the opposite effect.

Explicit Knowledge

At the end of the experiment, participants' explicit knowledge of AB associations was probed by cueing a direct stimulus and asking participants to choose the correct indirect stimulus among two alternatives. As expected with regards to the implicitness of the association phase, there was no overall group effect in explicit knowledge above chance level (*M* = .48, *SD* = .18; *t*(48) = -0.61, *p* = .545).

Discussion

The purpose of this experiment was to investigate whether a value spread through memory integration could occur to a similar extent when using social cues instead of monetary rewards. The results of both decision scores and stimulus rating changes (see Appendix B) are fully consistent with prior research (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019). While there was no significant decision bias in any of the three comparisons, direct learning did occur in each of them.

The effects in direct learning are interesting in their own right and suggest that preferences can arise solely based on social identity symbols without employing monetary rewards and losses. However, the value that the direct stimuli obtained through the pairing with the symbols did not spread to the indirect stimuli. This absence of a value spread through memory integration on a group level is consistent with prior research employing the same implicit paradigm (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012; Yngve, 2019). However, strong inter-individual differences in prior work and the present experiment (see Figure 4) show that some individuals seem to exhibit a value spread.

While consistent with the three aforementioned studies which lent their design for this experiment, the current results are opposed to those from Carpenter and Schacter (2018a). These authors did find a significant decision bias in a design with some pivotal changes within an explicit design. Most crucially, before the decision phase, the authors placed a memory test phase that required participants to flexibly recombine episodes to make inferences. Only after successful inference was a value transfer observed, leading the authors to conclude that it is not integrative encoding facilitating reward learning but flexible retrieval underlying value transfer in this paradigm (Carpenter & Schacter, 2018a). The current experiment seems to support their argument that while both processes may be involved in the decision bias, flexible retrieval seems to offer higher chances for success.

Apart from the timing of memory integration, another possible explanation for the lack of a decision bias lies in the order of encoding phases. In the current paradigm, the stimulus pairs from the association phase are under two-fold mnemonic strain which adds to their merely implicit learning. First, they need to be retained longer than the material from the subsequent conditioning phase, which is also explicitly encoded. Second, as conditioning phase pairs overlap with association phase pairs, retroactive interference may be resolved with pattern separation instead of memory integration, precluding the value spread within integrated representations and preventing a decision bias. Thus, the fundamental question of this investigation concerning the factors promoting memory integration and separation can play a role in the results.

While prior studies have established neural substrates in those participants who did show a decision bias (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012), this experiment aimed to find political covariates, specifically focusing on centrality (the importance of the social identity) and value difference (a measure of relative strength of the ingroup and outgroup symbols). The resulting picture is mixed but does offer some preliminary suggestions for the importance of each factor and their interaction. Centrality and value difference alone did not correlate with any of the decision biases. However, their interaction did become significant predicting the ingroup vs. baseline and baseline vs. outgroup biases. When predicting the latter bias, centrality also gained marginal significance.

This pattern can be considered as preliminary, albeit weak, evidence for possible separate but interacting effects of centrality and value difference. Theoretically, both concepts are distinct. Centrality is the overall and stable importance of the social identity in question (Bai, 2020), which was politics in this experiment. Value difference describes the relative pleasantness or positivity of the symbols utilized in the conditioning phase and could be manipulated by using symbols with more or less value attached to them. While both dimensions should lead to stronger reactions in the conditioning phase, they are independent from each other. For instance, an individual may consider his own political values as unimportant for his overall identity but still have rigid opinions about different parties. Likewise, a strongly politically interested person whose views are extremely important to her may rate several parties as relatively similar to each other, either because of a lack of differentiation within the mid-part of the political spectrum or out of a disdain (or uniform support) for all political parties represented in parliament.

These considerations would warrant an independent examination of both factors and their interaction. Increasing each factor should lead to a stronger reaction, so a higher decision bias. This model bears similarity with the Circumplex Model of Affect (Russell, 1980), in which emotions are arranged on two orthogonal dimensions, namely pleasantness and arousal. Centrality of the social identity determines the potential arousal towards the symbols while pleasantness is manifested in the liking of each utilized symbol, representing ingroups or outgroups.

The interaction of both dimensions led to a higher decision bias towards ingroup stimuli over baseline exemplars, which is fully consistent with the explanation. Unexpectedly, a simultaneous increase of both dimensions also led to a lower decision bias in the baseline vs. outgroup comparison. This is especially surprising since centrality alone gained marginal significance with a positive slope, indicating that those whose political views are more important to them tended to be more likely to choose the baseline over the outgroup option.

This negative interaction may well be due to a chance-based artefact that resulted in a small and insignificant but negative slope for decision bias alone. However, four additional theoretical considerations warrant general caution with the outgroup condition of this experiment.

First, due to the consistently low ratings of the outgroup party, the value difference was dominated by the ingroup party rating, which makes the interpretation of its effect in the baseline vs. outgroup comparison difficult. This approach was still preferred over considering ingroup and outgroup party ratings separately as the trends based on the latter group would have been determined by a few outliers who cast ratings higher than zero or one. Furthermore, it may be argued that separate scores make little sense in a design where both conditions are directly compared to each other and may be implicitly influential even in trials that they are not involved in.

Second, like and dislike of political parties are complex concepts. The literature distinguishes between ideological polarization based on rational deliberations about stances and policies, and affective polarization, so affective partisan dislike or even contempt (Iyengar et al., 2012). Whether the low ratings of the outgroup party were due to affective or ideological aversion cannot be determined with the instruments used in this experiment. However, it is possible that affective polarization alone leads to a higher decision bias while ideological polarization does not have the same effect.

Third, it has been argued that affective polarization depends not only on magnitude of dislike but also the size of the disliked party. The bigger the party, the bigger its threatening potential and the higher the affective polarization (Wagner, 2021). In Sweden and Germany, the respective right-wing party, which was chosen as the outgroup party by almost all participants, took less than 13% of the votes in the latest general elections, with recent opinion polling being on similar levels (Kantar, 2021; ZDF-Politbarometer, 2021). Thus, there seems to be no realistic chance for either party to form or participate in a government after the next elections. This fact could have limited affective polarization and thus the value-inducing effect of the outgroup symbol.

Fourth, these complications converge with a possible separation of ingroup favoritism and outgroup derogation. While this work has considered an intergroup bias as a one-dimensional concept, the argument can be made that intergroup bias can be driven by ingroup favoritism, outgroup derogation or both (Brewer, 1999; Hamley et al., 2020). In politics, specifically, the extreme left and right exhibit elevated levels of outgroup derogation, which is lower in the middle of the political spectrum (van Prooijen et al., 2015). With the sample rather being mid-leftist and considering that ingroup favoritism is generally more prevalent than outgroup derogation, it is possible that the effect of the outgroup symbol was lower than that of the ingroup symbol.

Thus, conclusions about the outgroup comparisons should be drawn carefully. It should be stressed that the interaction between value difference and centrality did gain significance in the ingroup vs. baseline comparison that did not involve the potentially problematic outgroup condition. This fact supports the usefulness of the proposed model, which could be tested in future research.

In conclusion, the present experiment provides first evidence for the potential of a value spread due to memory integration using social identity cues to a similar extent as in prior studies. Furthermore, first careful conclusions may be drawn about the role of centrality of the targeted social identity and the affective strengths of the symbols playing a role in the magnitude of the decision bias.

Limitations

There are three more general limitations to this experiment. First, the deliberations about political identity in a field of complex forces of liking, affective polarization, ideological evaluations, and lifetimes of experience with political parties and its supporters underline that political views are a peculiar social identity. All conclusions in this experiment therefore remain politics-specific until repeated for other social identities or minimal groups. Furthermore, as the sample was almost exclusively leftist, all conclusions are limited to that side of the political spectrum.

Second, the ongoing pandemic precluded the collection of neuroimaging data that could have provided additional evidence for similarities and differences between using social identity cues and monetary rewards. This also means that it cannot be said with certainty if the value spread observed in some participants relied on the same process that was implicated by prior studies (Kurth-Nelson et al., 2015; Wimmer & Shohamy, 2012). It is unclear if the indirect stimulus was reinstated during the episode including the direct stimulus and the symbol, even though this is the most logical conclusion to draw.

Third, this experiment is the first one in the field that implemented the paradigm in an online and at-home setting. From the perspective of the participants, this experiment is both straining, as the encoding phases are long, with many repetitions of the same material, as well as highly mundane with simplistic tasks. Even though engagement during the encoding phases was high, as the button press accuracy implied, participants' behavior during the experiment is unknown and a lack of distractions, high cognitive effort, and even understanding of the instructions cannot be guaranteed.

Future Research

Led by the results and discussion of this experiment and grounded on prior attempts to capture a value spread through memory integration, three ventures may be recommended for future investigations. As no study so far has been able to find an above-chance-level decision bias in the design by Wimmer and Shohamy (2012), these ventures serve the two main objectives to determine why there has been no decision bias and under which circumstances it may arise as well as to understand the strong inter-individual variance that has marked the data in all investigations. All three ventures would also benefit from neuroimaging, which may help to elucidate the exact processes at play.

First, new studies may investigate the proposed model with the two orthogonal dimensions of centrality and value. Value can be systematically manipulated using monetary incentives by varying the sums in the reward and loss conditions to analyze if the decision bias changes in response. While it would be possible to employ the political framework for this purpose by manipulating which ranks in each individual's party hierarchy are chosen as symbols, the lack of a decision bias using the strongest possible difference is discouraging.

Centrality of pre-existing social identities, on the other hand, cannot be split into manipulations that participants are randomly assigned to but can only be measured. To improve this measurement and if time pressure is less prevalent than in a self-administered at-home study, more complex instruments could be employed that consist of several facets (e.g., Leach et al., 2008), some of which could selectively correlate with a decision bias.

The problems with independently targeting centrality and value suggest the second horizon, which is a closer look at the social identity of choice. While political views are a generally important domain, party affiliation in multi-party democracies is a complex concept that may not work in simple dichotomies with individual patterns of concepts like outgroup derogation and ingroup favoritism (van Prooijen et al., 2015), affective polarization (Wagner, 2021) as well as highly idiosyncratic experiences with each party and its supporters. Two-party systems like the United States where the outgroup party could always gain power could allow clearer cuts between ingroup and outgroup and hence lay the foundation for high levels of affective polarization. Alternatively, samples from opposition groups in countries like Russia or Belarus could be pursued for the purpose of clear group distinctions.

Alternatively, other social identities that may be less complex in carefully chosen samples and circumstances such as university affiliation, religious membership, professional sports clubs, or in some cases even nationality could be explored. However, the best way forward may be a minimal group paradigm (Bourhis, 2020; Tajfel et al., 1971). In these studies, participants are assigned to made-up groups that have no real-world relevance. By introducing different group symbols varying in alleged value to that minimal group and by manipulating the importance of the group to each participant, both dimensions can be targeted in isolation. In fact, minimal groups have been found to lead to elevated social contagion as well as to an incidental memory advantage, stressing their usefulness in memory research (Andrews & Rapp, 2014; Jeon et al., 2021).

As a third venture, several methodological alterations may be worth exploring. Since the conditioning phase has always employed an explicit encoding instruction, it should be investigated if the effects are sustained if participants are not aware that the pairings between direct stimuli and symbols are consistent. In a complementary manner, both phases could have an explicit instruction while not asking participants to integrate across episodes. Thus, the implicitness of memory integration would be preserved while providing better grounds for a decision bias. Furthermore, to explore a potential role of retroactive interference from the BC-episode on the AB-pairings, a pre-cueing paradigm may be applied in which reward phase and association phase are switched. This could increase salience and vividness of the first episode which may then be retained more easily for the subsequent association phase. The number of exposures may also be varied as each pairing is presented very often, which may have little in common with the real world where preferences may be formed based on a single exposure. Furthermore, one may speculate whether the high number of exposures only serves to corrode the attention of even highly motivated participants and induce a dislike for absolutely everything they see on the screen.

These methodological considerations have high relevance for the conclusions that can be drawn. The current design can only provide implications about a retroactive value spread in which prior experiences are colored with the value from a new, overlapping episode. However, a proactive value spread is equally conceivable in which new episodes are encoded in the light of prior, value-laden episodes that are reinstated during the new experience. A similar thinking is valid for the number of exposures. Even though we are constantly exposed to social identity symbols like party logos, a consistent pairing for many times within a small temporal interval may not often be the case. Smaller numbers of exposure may thus be a more realistic setting.

A last methodological variation is the timing. All prior studies have taken place within one session in which the decision phase immediately followed the encoding phases. Since spaced learning is more effective than massed learning (e.g., Feng et al., 2019), spreading out the encoding phases into several sessions over several days could have the potential to evoke a decision bias and mirror real-world circumstances more closely. The online set-up in this experiment is especially suited for this end. Furthermore, introducing a retention interval of several hours or days between the encoding phases and the decisions will be necessary to investigate the durability of the effect once a consistent decision bias has been found.

General Discussion

Across two experiments, spanning the explicit basis and an implicit application of memory integration, the potential effects of social identity factors on memory integration have been investigated and demonstrated for the first time. Experiment 1 found that while direct memory benefits from an ingroup source, the speed of inferences benefits from an outgroup source. In Experiment 2, the possibility of a value spread, based solely on social identity symbols, has been indicated. Furthermore, behavioral correlates were found in both studies that tie the effects more closely to liking of the ingroup persona (Experiment 1) and the importance of the social identity in focus (Experiment 2).

The findings are of chief interest for the memory integration literature. Since the factors leading to memory integration or separation when facing overlap in episodes are still relatively unknown, identifying social identity as a potential modulating factor is of significant value. Furthermore, for the subfield of value spread due to memory integration, this investigation found the first behavioral correlate of the decision bias, thus laying the grounds to create circumstances that allow a value spread.

Furthermore, the findings have implications for collective memory. It has been argued that transformations facilitated by ingroup status underly the formation of collective memories in communities (Hirst & Echterhoff, 2012). Through conversations that lead to selective remembering and forgetting, shared representations of the past arise. These representations are not static, but are constantly re-negotiated and often build the very basis of a community's identity (de Saint-Laurent, 2018). The present results have divergent implications for this account, and both enrich and weaken it.

The results from Experiment 2 certainly extend this account of collective memory by suggesting a mechanism of how it can serve the underlying self-elevating purpose of social identity. By positively or negatively coloring neutral prior experiences simply due to an indirect relation to the ingroup, a group's narrative inevitably becomes positive in hindsight. These positively colored representations could then serve the purpose of self-esteem derived from a group membership.

Experiment 1, on the other hand, suggests a caveat for the present view of collective memory as a reconstructive process facilitated by social identity. While a direct ingroup advantage has been found in this experiment, this advantage may come at the cost of reduced mnemonic flexibility of the shared representations. This simplification in the shared accounts possibly underlies their usefulness for group cohesion and group-derived self-esteem (Tajfel & Turner, 1979), but may lead to a reluctance to accommodate new information that may potentially weaken a group's collective narrative. Deeper partisan trenches would be the consequence.

However, focusing on potential consequences for politics and political discourses, the fact that outgroup information is integrated better could also have positive indications. Outgroup information is not necessarily rejected or forgotten but does become integrated in pre-existing knowledge structures. Thus, instead of having to fight against blind adherence to information from ingroup echo-chambers, politicians of opposing parties or leaders of civil society still have a chance for their messages to be heard, incorporated, and remembered. Contrary to an impression of a polarizing society, a basic process of memory seems to counter one-sided partisan memory. Naturally, this mechanism does not discriminate between the intentions of the outgroup and would also allow messages from extremists, autocrats, and anti-democratic forces to access and update knowledge structures. Replacing the neutral information from this investigation with political messages will allow investigating the effects of attitude-congruency of the message and is therefore an aim for future research with high societal relevance.

Once again, the consequences of Experiment 2 for political life go into a different direction. The fact that value spreads could potentially be triggered by social identity symbols implies that preferences can develop without any awareness or deliberation, merely due to indirect association. This magnifies the sphere of influence of a mere exposure effect according to which attitudes towards an object become more positive the more often it is encountered (Zajonc, 1968). A value spread based on social identity symbols extends this effect to indirectly related objects, so that not only the evaluation of a party would get more positive by seeing its logo constantly on posters and merchandising products, but so would the politicians or policies associated with that party. The omnipresence of political advertisement during election campaigns suggests that the basis for value spreads is laid almost constantly.

Regarding the novelty of the present approaches, paradigms, and findings, the discussed implications and other conclusions drawn in this work are merely cautious first steps into a new horizon. While much work is still to be done, this investigation has underlined the usefulness of a union between hitherto disparate literatures and thus the "socialization" of memory integration.

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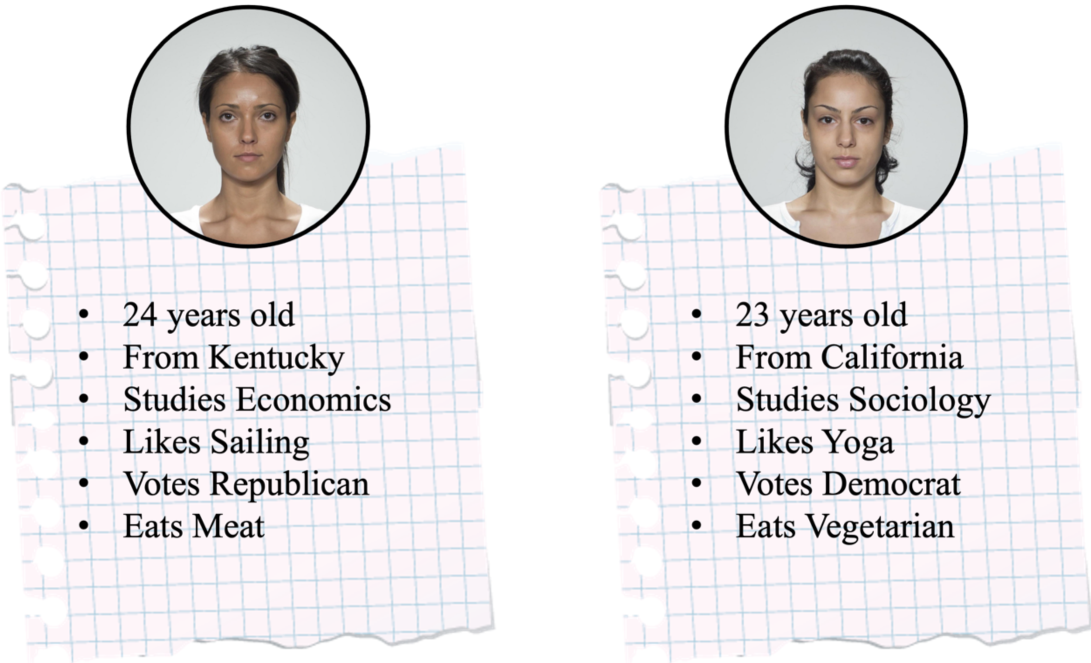
Appendix A

Supporting Information for Experiment 1

Figure A1

Persona Profiles that were Presented to Participants



Note. The matching of face and profile was counter-balanced across participants. Thus, only one of the two possibilities is depicted here.

Appendix B

Supporting Information for Experiment 2

Table B1

Ingroup and Outgroup Parties Chosen by the Participants

Party	German Sample	Swedish Sample	Total
Ingroup Party			
Green	26 (Die Grünen)	4 (Miljöpartiet)	30
Social Democratic	8 (SPD)	2 (Socialdemokraterna)	10
Left	3 (Die Linke)	4 (Vänsterpartiet)	7
Conservative	1 (CDU)	1 (Centerpartiet)	2
Outgroup Party			
Right-wing	38 (AfD)	9 (Sverigedemokraterna)	47
Other		1 (Centerpartiet)	2
		1 (Moderaterna)	

Text B1

Analysis of Changes in Rating

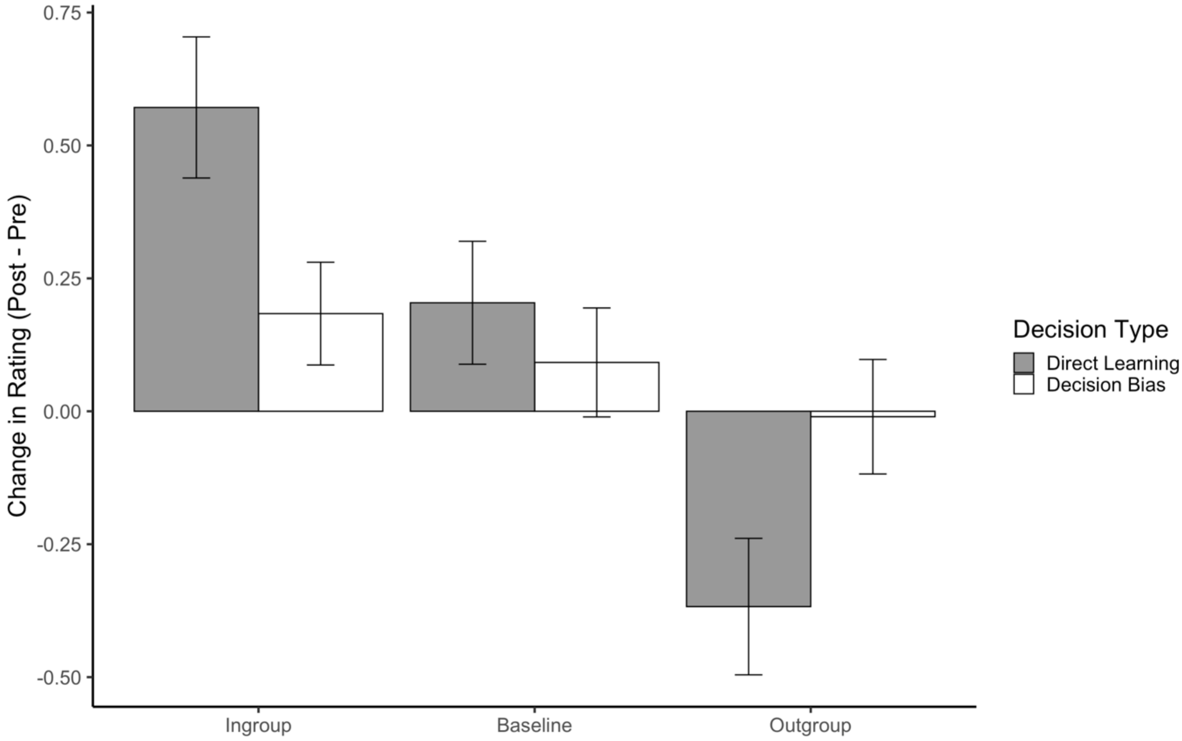
At the beginning and end of the experiment, participants rated all utilized direct and indirect stimuli. The changes in these ratings can be used as an additional indicator for the change of value. Therefore, to assess if stimuli ratings changed as a function of condition, a 3-way ANOVA was computed with the factors of associated symbol condition (ingroup vs. baseline vs. outgroup), decision type (decision bias vs. direct learning) and time (pre- vs. post-experimental). This ANOVA yielded main effects for all three factors, so for decision type ($F(1, 48) = 32.40$, $p < .001$), condition ($F(2, 96) = 11.24$, $p < .001$) and time ($F(1, 48) = 6.65$, $p = .013$). Furthermore, the interactions between condition and type ($F(2, 96) = 8.94$, $p < .001$) and condition and time ($F(2, 96) = 15.97$, $p < .001$) as well as the three-way interaction between condition, decision type, and time ($F(2, 96) = 6.78$, $p = .002$) gained significance (see Figure B1).

Post-hoc tests revealed that stimuli were generally rated better after the experiment ($M = 3.05$, $SD = 1.15$) than before ($M = 2.94$, $SD = 0.76$). The inspection of the interaction of condition and time suggested that this was based on the strong increase in ratings of stimuli in the ingroup condition (pre-experimental: $M = 2.94$, $SD = 0.76$; post-experimental: $M = 3.32$, $SD = 1.12$) while those in the outgroup condition only showed marginally significant decreases (pre-experimental: $M = 2.93$, $SD = 0.76$; post-experimental: $M = 2.74$, $SD = 1.18$). As the exploration of the three-way interaction revealed and as Figure B1 supports, these changes in the ingroup condition were driven by increases due to direct learning (Pre: $M = 2.83$, $SD = 0.93$; Post: $M = 3.40$, $SD = 1.24$), while no significant changes occurred as a decision bias.

In conclusion, the analysis of changes in rating supports the findings in decision behavior. While direct learning is clearly taking place, a decision bias cannot be demonstrated on a group level.

Figure B1

Changes in Stimulus Rating by Decision Type and Condition



Note. This figure depicts change scores obtained by subtracting the post-experimental rating from the pre-experimental rating for each stimulus. Positive values therefore indicate an increase in rating.

Acknowledgements

This work would have been a lot harder and a lot less enjoyable without the people who accompanied me through the oscillations between desperation and enthusiasm. Thanks is due to everyone from the Lund Memory Lab for discussing my ideas and providing a much-needed structure during a digital semester. Special thanks to my supervisor Inês Bramão for her availability and guidance as well as constant support and encouragement. I would also like to thank Linn, Anne, Lena, and Anderson for their comments on the manuscript and, perhaps more importantly, for helping me to put it aside for a while.