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# Dynamic Contact Effects: Individuals' Positive and Negative Contact History Influences Intergroup Contact Effects in a Behavioral Game

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Positive contact between members of different groups reduces prejudice and increases cooperation, findings known as intergroup contact effects. Yet in real-world settings not only positive, but also negative intergroup contact occurs, which have opposing effects. To date little is known about whether and how an individual's valenced history of intergroup contact influences contact effects and how this dynamic change happens during specific instances of intergroup contact. A pilot study examined the psychological impact of a novel paradigm to assess intergroup contact using a behavioral game. We then conducted two studies, which allowed us to observe a sequence of up to 23 in- and outgroup interactions and their behavioral outcomes in a continuous prisoner's dilemma behavioral game ( $N = 116$ , 2,668 interactions;  $N = 89$ , 1,513 interactions). As expected, participants showed a clear ingroup bias in expectations and cooperation. Furthermore, the quality of contact history moderated contact effects. Specifically, intergroup contact following a positive history of intergroup contact had a stronger effect on intergroup expectations than contact following a negative history thereof. Findings are discussed in view of the importance of considering the valenced history of intergroup contact, as well as new research questions on intergroup contact that can be addressed with this novel contact paradigm.


*Keywords:* intergroup contact, intergroup interactions, negative contact


Improving intergroup attitudes and behavior has long been a core objective of social psychology. Ever since its emergence in the 1950s, intergroup contact theory has been among the most

important approaches seeking to improve intergroup relations (e.g., Brown & Hewstone, 2005; Pettigrew et al., 2011). Intergroup contact theory predicts that positive contact between members of different groups improves intergroup attitudes. This hypothesis has received broad empirical support from a range of studies, including a large meta-analysis (e.g., Pettigrew & Tropp, 2006), longitudinal research (e.g., Swart et al., 2011), as well as contact interventions outside the lab (Lemmer & Wagner, 2015).


Only recently, however, has this area of research begun to acknowledge that in real-world intergroup settings contact experiences may not only be positive, but also negative (e.g., Hayward et al., 2017). This recent research has already provided new insights into why it is important to consider negative contact: Several authors find similar or larger effects for negative than for positive intergroup contact (e.g., Barlow et al., 2012), suggesting that negative contact may even “curb contact's ability to reduce prejudice” (Paolini et al., 2010, p.1724). Furthermore, initial evidence suggests that positive and negative contact might not be independent in their influence on intergroup attitudes. In particular, the effects of positive contact on attitudes vary depending on whether an individual has also experienced negative contact (Árnadóttir et al., 2018; Hayward et al., 2017; but see Ten Berge et al., 2017).


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To date, however, only a few studies have examined whether positive and negative contact interact and the evidence thus far is both preliminary and mixed. The sparse evidence available relies mostly on cross-sectional data, with self-reported measures for intergroup contact and attitudes, and using measures that typically ask about longer timespans or even a more general assessment of past contact (Árnadóttir et al., 2018; Hayward et al., 2017). Furthermore, this first evidence does not include any behavioral outcomes, as is true for most contact research (MacInnis & Page-Gould, 2015). Moreover, the focus on overall ratings of valenced intergroup contact over larger timespans cannot address the complexity of intergroup experiences in real life situations (e.g., Dixon et al., 2005; Pettigrew & Hewstone, 2017). Indeed, recent research demonstrates that in real-world settings, positive as well as negative intergroup contact often entails more casual encounters that occur in informal settings, such as in public spaces (e.g., in parks or in public transport) or while shopping or eating out (Schäfer, Kauff, et al., 2021). Individuals in their daily life are therefore likely to experience multiple short or fleeting intergroup encounters, which may be of different valence: one might have a friendly intergroup contact experience while buying a newspaper, and a less friendly one with the bus driver—but then experience another positive intergroup encounter when greeting a waiter in a restaurant, and so on. Intergroup contact, similar to any interpersonal interaction that occurs, is thus sequential and cumulative in nature. This is a vital aspect of intergroup contact that has, to our knowledge, not been sufficiently addressed in prior research. We argue that considering the dynamic interplay of shorter and more fleeting instances of intergroup contact that an individual typically experiences (rather than simply focusing on more subjective self-ratings of cumulative experiences over larger timespans) is of key importance to capturing the complexity of intergroup contact experiences in real-life situations and the impact thereof on intergroup behavior.

Furthermore, some studies have examined whether interactions with an outgroup member influence outgroup attitudes to a stronger extent than interactions with an ingroup member influence outgroup attitudes (e.g., Kotzur et al., 2018; Wilder & Thompson, 1980). Yet to our knowledge there is no research demonstrating that the effect of contact on generalized expectations toward the interaction partner's group is specific to the outgroup, or occurs similarly for the ingroup too.

The present research adopts a novel paradigm that not merely analyzes contact with and perceptions of the outgroup, but also measures ingroup and outgroup behavior during a sequence of up to 23 interactions of varying valence using a behavioral game approach. This work thus makes a novel contribution and improves on previous research in three ways. First, we examine how a valenced (i.e., positive vs. negative) history of intergroup contact influences subsequent effects of intergroup contact, thus acknowledging that contacts do not occur in a vacuum but are embedded in a range of experiences. Second, we consider in a dynamic framework not only a cognitive component of intergroup attitudes, namely expectations about the behavior of changing in- and outgroup partners, but also capture actual behavior, specifically the amount of cooperation in the behavioral game. Third, we compare effects of interactions with partners, who are always new and who belong either to the ingroup or the outgroup, which additionally allows us to test whether any effects detected are specific to *outgroup* contact. Using a behavioral games approach to study intergroup contact effects opens up new frontiers

for intergroup contact research. By moving the focus from broad overall ratings of intergroup contact over larger time spans and its impact on (self-reported) attitudes (typically the primary focus of prior research in this area) to the more specific sequential and cumulative intergroup encounters, our approach allows us to acknowledge and capture the dynamic and interactive aspects of positive and negative intergroup contact and how they impact actual behavior.

## The Contact Hypothesis and Recent Developments

Prejudice and discrimination remain persistent challenges to the development of peaceful societies, and finding ways to overcome these constitute a core focus of policy interventions as many societies worldwide are becoming increasingly diverse (e.g., OECD, 2014). A key means to improving relations, which goes back to early social psychological studies in the 1940s, is through intergroup contact (e.g., Pettigrew et al., 2011). Most prominently formulated by Gordon Allport in his classic book, *The Nature of Prejudice* (Allport, 1954), the so-called “contact hypothesis” suggests that intergroup contact between members of different groups could improve intergroup attitudes, especially if the contact occurred under favorable circumstances, namely equal status, cooperation, authority support, and a common goal. Since then, the contact hypothesis has become one of the most frequently investigated hypotheses in social psychology (Pettigrew et al., 2011), and an impressive body of evidence (see, e.g., a large meta-analysis including 713 independent studies by Pettigrew & Tropp, 2006) has demonstrated that intergroup contact typically reduces prejudice. Moreover, from its very beginnings, intergroup contact theory has sought to answer applied research questions and inspired interventions. Meta-analytic evidence of 123 contact interventions conducted outside the lab (Lemmer & Wagner, 2015) has demonstrated its effectiveness in the real world (but see Paluck et al., 2021; for a reminder that rigorous development and evaluation of interventions remains an important task). Furthermore, research on intergroup contact theory has yielded important insights of moderators (such as category salience, Brown & Hewstone, 2005) and mediators (e.g., empathy, anxiety, and increased knowledge, Pettigrew & Tropp, 2008) of intergroup contact effects.

Notwithstanding this impressive evidence base and expansion of the original hypothesis into by now an elaborate theory, more recent advancements in contact research have highlighted crucial aspects that remain to be addressed (Pettigrew, 2021). First and foremost, recent critiques have cautioned that much of prior intergroup contact research may not have sufficiently captured the complexity of intergroup experiences as they occur in real world situations (e.g., Dixon et al., 2005; Pettigrew & Hewstone, 2017). There has also been a disproportionate focus in intergroup contact research on more general outgroup contact (e.g., more general assessments of contact over longer timespans, typically using self-reports) rather than on intergroup interactions (e.g., actual interactions with outgroup members as they occur in day-to-day settings; see, e.g., MacInnis & Page-Gould, 2015). Additionally, much of prior research has focused predominately on more positive types of contact, thus failing to acknowledge or capture the fact that intergroup contact may also be negative (Pettigrew & Tropp, 2011).

The current article directly addresses these two central critiques: the call to integrate intergroup contact research with research on intergroup interactions (MacInnis & Page-Gould, 2015), and

reminders to consider positively as well as negatively valenced intergroup experiences (e.g., Paolini et al., 2010; Schäfer, Kauff, et al., 2021).

### Intergroup Contact Versus Intergroup Interaction

In *The Nature of Prejudice* (1954), Allport already pointed out that several factors could influence or even spoil the favorable outcome of intergroup contact. Among these factors, which might hinder the beneficial effects of intergroup contact, Allport argued that casual contact might not be sufficient to decrease prejudice. Furthermore, he argued that previous experiences with the respective outgroup could adversely impact the effects of intergroup contact (Allport, 1954, pp. 263–264). Indeed, research has shown that relationally closer forms of intergroup contact, such as intergroup friendships, are a stronger predictor of reduced prejudice, than more casual forms of intergroup contact (Marinucci et al., 2020; Pettigrew & Tropp, 2006). Yet, in everyday life, intergroup contact experiences do often happen in the form of single, concrete interactions: In qualitative reports asking about intergroup contacts, individuals report even very short, fleeting intergroup interactions, such as greeting a neighbor or interactions with store clerks (e.g., Keil & Koschate, 2020; Schäfer, Kauff, et al., 2021). Research on intergroup contact has largely neglected these specific instances of intergroup contact. Moreover, many findings to date are based on comparisons between general assessments of more or less intimate forms of contact (e.g., using self-report measures that ask about frequency of more or less intimate contact with outgroup members). Yet, we argue that specific encounters are particularly important for intergroup contact researchers to consider. Not only do they define common intergroup encounters that individuals are likely to make in their day-to-day lives, but they can also critically impact perceptions and future behavior.

Social psychological research that has considered single interactions has done so mostly in research within the framework of so-called “intergroup interactions”—a field of research in which, rather than considering overall intergroup contact experiences, specific instances of intergroup interactions with strangers are observed, typically in the laboratory (e.g., Blascovich et al., 2001; Shelton et al., 2010; Vorauer & Kumhyr, 2001). This literature has thereby largely focused on the interacting parties’ evaluation of the quality of these interactions. Findings of this line of research demonstrate that even single interactions affect individuals’ perception and evaluations of the respective situations. Yet, most of the research examining intergroup interaction has focused on a comparison of the experience of outgroup compared to ingroup contact, and has scarcely considered effects of several interactions with multiple outgroup members and their effects on intergroup outcomes, such as for example cooperation (MacInnis & Page-Gould, 2015; for an exception see Page-Gould et al., 2008). Still, it is important to keep in mind that the two literatures, on intergroup interactions, on the one hand, and intergroup contact, on the other, are intricately related. Indeed, MacInnis and Page-Gould (2015) state that, “an intergroup interaction is the atomic unit of intergroup contact” (p. 309), which suggests that any findings accrued in the intergroup-interactions literature should also impact intergroup contact more generally. Yet, to date, to our knowledge, little advances has been made in understanding the effects of separate, but repeated intergroup interactions and their dynamic interplay over time, and how they ultimately affect expectations

about, as well as actual, future behavior. It is, in part, our aim in this research to fill this gap.

### Interactions of Positive and Negative Intergroup Contact

Previous discussions of the impact of intergroup contact research have furthermore highlighted that, to date, scarce research has sought to understand when and why intergroup contact does not reach its full potential to improve intergroup relations (Pettigrew & Tropp, 2006; but see, for exceptions, Amir, 1969; Stephan & Stephan, 2000). Indeed, only recently have intergroup contact researchers started to focus more systematically on different types of contact that can maximize the beneficial effects thereof, and to study both positive as well as negative intergroup contact (e.g., Barlow et al., 2012; Paolini et al., 2010). Research on the effects of differently valenced contact has thus far found that although negative intergroup contact is much less frequent than positive contact (e.g., Graf et al., 2014; Schäfer, Kauff, et al., 2021); it sometimes yields effects of stronger magnitude on negative outcomes when compared to the magnitude of beneficial effects of positive contact, thereby risking increased intergroup tensions. Considering negative contact experiences is thus crucial for a comprehensive understanding of intergroup contact in everyday life.

Initial studies including measures of positive and negative contact assumed an additive model of valenced contact effects (e.g., Barlow et al., 2012): These studies discussed whether the adverse effects of negative contact outweighed the beneficial effects of positive contact. Yet, initial research suggests that, instead, positive and negative contact might interact in their effect on outgroup attitudes. The idea of a potential interaction of intergroup contact effects builds on work by Paolini et al. (2014), who examined the effects of a history of positive contact on subsequent valenced contact effects on category salience. In one cross-sectional and three experimental studies they demonstrated that people with a history of positive intergroup contact perceived a lower level of category salience in negative interactions. Following their argument, prior positive contact should result in a weaker effect of subsequent negative contact on prejudice, which is described as “buffering” the effect of negative contact (see Árnadóttir et al., 2018; Fell, 2015). Paolini et al. (2014) did not, however, include any measure of outgroup attitudes in their studies. Four more recent studies included attitudinal outcome measures, but did not explicitly refer to an individual’s history of intergroup contact; they simply examined possible interactions of an individual’s frequency of positive and negative intergroup contact on intergroup relations in greater detail.

In the first of these four studies, involving the most detailed assessment of the nature of valenced contact to date, Hayward et al. (2017) asked White, Black, and Hispanic Americans how often they had experienced 69 specific instances of positive (37 items) and negative (32 items) contact, and how positively or negatively they experienced the respective situation. In their supplementary material they report evidence for so-called *exacerbating* effects (see Árnadóttir et al., 2018; Fell, 2015), namely a stronger effect of negative contact when levels of positive contact are high, for outcome measures of both empathy and avoidance among minority members, and empathy only among majority members. On the other hand, they found evidence of *buffering* effects, namely a



weaker effect of negative contact if levels of positive contact are high, for the outcome measure of anger among majority members, and of outgroup evaluations among minority groups. These mixed results might be caused by the wide range of interactions used as indicators for overall positive and negative contact, for which no measure of statistical reliability is provided (Hayward et al., 2017; supplementary material).

Second, Árnadóttir et al. (2018) provided cross-sectional evidence from a survey among Icelandic majority members. They found significant interactions between positive and negative intergroup contact on outgroup attitudes, trust, and crime estimates. Specifically, negative contact only yielded significant adverse effects on attitudes and trust if the participant had reported low levels of positive intergroup contact. Positive contact thus buffered negative contact effects. From the reverse perspective, positive contact had stronger favorable effects if participants also reported more negative contact experiences. However, the cross-sectional nature of the data in both Hayward et al.'s and Árnadóttir et al.'s (2018) studies makes it impossible to test whether positive contact moderated negative contact effects or negative contact affected positive contact effects.

Third, in a two-wave longitudinal dataset comprising 4,238 pupils in the Netherlands, Ten Berge et al. (2017) tested whether an increase in outgroup best friends would buffer the effects of having outgroup foes, but did not find any interaction between gaining outgroup friends and foes. Having best friends and foes might not, however, tap the full scale of positive and negative experiences, which also led to very low reports of negative experiences in this sample, where, especially at Wave 2 of the dataset, only 5% of the pupils reported having any outgroup foes (hence the variance of negative contact was restricted).

In a fourth study examining interactions of positive and negative intergroup contact using four waves of a large data set from New Zealand, Barlow et al. (2019) found interactions of positive and negative contact in both cross-sectional and longitudinal analyses. Specifically, they found that in 15 out of 16 cross-sectional analyses positive contact yielded a stronger relationship with warmth if participants also reported higher levels of negative contact. Additionally, negative contact was more strongly related with increased anger at low levels of positive contact (again in 15 out of 16 analyses). Yet, when they considered change scores in the longitudinal dataset, they found only two out of 24 analyses to yield a significant interaction. This interaction, however, followed a different direction to those obtained in the cross-sectional analyses: When there was little change in negative contact over time (compared with high change in negative contact), positive contact predicted change in warmth more strongly. These findings demonstrate the importance of disentangling different temporal dimensions when studying interactions of valenced intergroup contact.

Overall, however, this handful of studies on potential interactions of positive and negative contact does not yield unequivocal evidence for whether, and if so how, positive and negative contact interact in their effects on intergroup attitudes. Additionally, these studies focused exclusively on general assessments of intergroup contact over larger time-spans (see MacInnis & Page-Gould, 2015), which, even when utilizing longitudinal data, cannot adequately address the change of attitudes that may occur after discrete instances of intergroup contact that occur on an individual level (Hamaker et al., 2015). This is because attitudes may partially be of a stable, time-invariant nature and thus represent differences between persons;

estimating lagged effects in a traditional cross-lagged panel model will thus not represent the actual relationships of different variables within a person, who, as highlighted above, may experience a variety of more or less casual intergroup contact experiences in their everyday life (e.g., Keil & Koschate, 2020; Schäfer, Kauff, et al., 2021). Understanding such differences requires testing the interaction between more sequential and cumulative instances of positive and negative intergroup interactions, a further gap this paper seeks to address.

## Previous Experiences Shape Subsequent Ones

Providing a potential explanation for why and how positive and negative intergroup contact interact, Paolini et al. (2014) refer to the *perceived fit hypothesis*, which predicts that negative contact will have strongest negative effects when negative experiences fit expectations based on a history of negative contact. Conversely, this could lead to the expectation that a history of positive contact could buffer against the adverse consequences of negative contact (Paolini et al., 2014). This perceived fit hypothesis receives further empirical support from studies on intergroup interactions which demonstrate that an individual's history of positive intergroup contact (i.e., having outgroup friends) leads to a more positive perception of interactions with the outgroup (Blascovich et al., 2001; Page-Gould et al., 2008). As an increase in perceived positivity of contact increases the effects of subsequent positive contact, this in turn should result in a larger effect following positive intergroup contact (Schäfer, Kros, et al., 2021).

In contrast, another established paradigm, *adaptation-level-theory*, suggests that events and entities are judged relative to previous experiences (e.g., Helson, 1964), such that the neutral point of experience should dynamically adapt to previous experience. This adaptation has been demonstrated in a range of fields, for example with regard to visual (e.g., Helson, 1948) and auditory (Bevan et al., 1962) effects, but also with regard to job satisfaction (e.g., Ritter et al., 2016). For the realm of intergroup contact, this adaptation level effect would, for example, result in the strongest adverse effects of negative contact following a history of positive contact, thus an exacerbation effect. Although leading to opposite predictions regarding the direction of the outcome, the perceived fit hypothesis and adaptation-level-theory both suggest that positive and negative contact effects should interact, but they do not differentiate their predictions depending on the valence of a person's previous experiences, thus the overall mechanisms of both theories are not assumed to vary depending on whether the first experience is positive or negative.

Other accounts instead assume that the valence of previous experience matters. Evidence on impression formation shows that change in personality impressions depends on whether prior impressions were positive or negative, with negative first impressions being especially hard to change (e.g., Briscoe et al., 1967; Cusumano & Richey, 1970; Freedman & Steinbruner, 1964; Reeder & Coovert, 1986; Rothbart & Park, 1986; Tausch et al., 2007; Ybarra, 2001). Research on the violation of expectations also suggests a larger change if initial expectations were positive (Burgoon, 1993). In an experiment in which participants were instructed to expect more or less money, those who expected most and received most had the largest positive change in positive moods—even more than those who received more than they had initially expected. In contrast, those who expected most also had the largest decline in positive

mood, if they received less than expected (Austin & Walster, 1974). Extrapolating these findings to the realm of valenced intergroup contact experiences would lead us to expect a larger overall effect of intergroup contact following positive compared with negative intergroup contact experiences.

In sum, generalizing these results to the field of intergroup contact, we expect that positive and negative intergroup contact interact. Yet, evidence of whether a history of negative or positive contact would increase or decrease subsequent contact effects remains unconvincing to date, and different theoretical accounts yield different predictions. To test whether, and examine how, a history of positive or negative contact experiences shapes subsequent contact effects in contexts that entail immediate interactions between individuals, we made use of a behavioral game, a continuous prisoner's dilemma scenario. This allowed us to observe real interactions between individuals representing different groups on repeated occasions, and to analyze the impact of these dynamic changes in the valence of interactions on behavior.

### Using Behavioral Games to Study Intergroup Contact

Behavioral games, used in a range of disciplines including psychology, economics, and sociology, "provide a substantive model of many actual encounters" (Murnighan & Wang, 2016, p. 80) where actual behavior between interacting parties can be directly observed. Behavioral games are usually played with multiple players, whose decisions to cooperate with the other players or to defect are analyzed (Julmi, 2012). For example, they have been used to demonstrate that separating groups even by minimal criteria leads to an ingroup-bias in cooperation and expectations (e.g., Balliet et al., 2014). For the present article, we used a prisoner's dilemma. The *prisoner's dilemma* is one of the most common social dilemmas used as a behavioral game (e.g., Van Lange et al., 2013). In the version of the prisoner's dilemma that we used for the present research (i.e., continuous prisoner's dilemma), two players decide how much they want to cooperate, whereby unilateral defection results in the highest gains for the individual, but cooperation of both players yields the largest shared outcome. Underlying motives for high cooperation in a prisoner's dilemma are trust, fairness, altruism, and social welfare, whereas low cooperation is motivated by fear, greed and competitiveness (Thielmann et al., 2015). Behavioral games, such as the one we employed here, not only provide us with an opportunity to observe multiple, repeated interactions between different members of different groups in a setting that provides high internal validity; they also provide a novel approach to the targeted investigation of negative (compared with positive) contact in a controlled setting. In particular, behavioral games provide the opportunity to observe positive and negative interactions without exposing participants to deception, an ethical concern that may arise when, for example, exposing participants to any bogus negative experience, which may have the undesirable consequence of increasing prejudice and discrimination.

Moreover, behavioral games are established paradigms to demonstrate ingroup bias between a wide range of groups (Balliet et al., 2014). Ingroup bias describes a tendency to favor one's own over other groups or, under more extreme circumstances, even a tendency to derogate outgroups (e.g., Hewstone et al., 2002). Even in so called minimal group paradigms (e.g., Tajfel et al., 1971),

participants show a tendency to prefer their own group over other groups—and these early results suggest that participants even chose the option to maximize the difference between outgroup and ingroup over the option to maximize the outcome for their own ingroup. In a meta-analysis of 137 tests of ingroup bias involving several evaluations of the ingroup and outgroup, Mullen et al. (1992) concluded that findings overall supported an ingroup bias of medium effect size. Similar effects are found when it comes to cooperation between groups in behavioral games: in a meta-analysis of 212 studies, Balliet et al. (2014) found a medium effect size, which indicates that individuals cooperate more with members of their own than with members of other groups. Behavioral games thus constitute a sound way to measure this behavioral tendency in intergroup settings.

Surprisingly, however, to our knowledge behavioral games have rarely been used to test hypotheses derived from intergroup contact theory. In the only study we know of that employed a behavioral game paradigm in the context of intergroup contact research, Dorrough et al. (2015) found that experiences with an outgroup member from the previous round predicted expectations in the next round, even though participants were playing with a new partner each round. They thereby operationalized contact quality, thus the experience of interacting with an outgroup member within each round of the game, as the difference between participants' expectations of what they would receive and what they actually received from their partner.

It is important to keep in mind in such a behavioral game setting that the only information available about the respective partner in each of the rounds is their group membership. Differences in expectations about the future behavior of ingroup and outgroup members will therefore indicate a specific form of more general intergroup bias. Furthermore, behavioral games provide a way to go beyond mere self-reported attitudinal outcomes (i.e., the assessment of expectations) to include measures of actual intergroup behavior (i.e., the respective cooperation with members of one's own and another group): If individuals expect cooperation from their respective partners, on average they will not exploit this situation, but rather are more likely to cooperate. For example, in a meta-analysis of 142 independent studies, Balliet and Van Lange (2013) found a strong relation of  $r = .58$ , CI 95% [.54, .62] between expectations and cooperative behavior. A strong positive relationship between expectations and cooperation is furthermore supported by a meta-analysis of 34 studies by Pletzer et al. (2018). In an intergroup behavioral game setting, as used by Dorrough et al. (2015), we would thus not only expect contact quality to predict expectations toward new members of the outgroup but expectations should also, in turn, predict intergroup cooperation. It is this prediction that we sought to test in our research, but going one step further to also examine the dynamic interplay between positive and negative interactions, and future expectations and behavior.

One important consideration of whether to use a behavioral games paradigm or not pertains to the importance of different aspects of validity (i.e., internal, external, and ecological validity): Building on a classic discussion of the pros and cons concerning different aspects of validity of behavioral games offered by Schlenker and Bonoma (1978), Lodewijkx et al. (2006) stress that behavioral games do not intend to be simulations of real-world situations; to do so they would need to consider as many potential influencing factors from the real-world as possible, which they do not. Therefore, ecological validity

is restricted. Rather, behavioral games are laboratory studies, designed to examine specific research questions, which are derived from theory and that should be tested in controlled settings. Thus, internal validity is typically high. To reach this goal, laboratory studies focus on the most relevant variables for the specific research question to be tested: In our case, a series of valenced interactions between members of different groups. While this design will not be able to fully address the complexity of interactions in real-world settings, it offers the possibility to study the effects of a history of positive and negative intergroup contact on subsequent contact effects in a structured and highly controlled setting. This design thus offers the possibility of maximizing internal validity, similar to other experiments in the field of intergroup relations from which important theoretical insights have been derived (e.g., Fehr & Rockenbach, 2003; Tajfel et al., 1971). The particular behavioral games we used in our study have received much attention in the literature due to (a) their simplicity in terms of structure, (b) the wealth of cognition, emotions, and motives that are activated, (c) their ability to capture core questions about cooperation, and (d) their ability to help understand core societal issues (Van Lange et al., 2014, p. 11).

## The Current Research

In our research, we focus on small and rather casual intergroup interactions, with a focus on the dynamic interplay between positive and negative experiences. In real life settings such positive and negative experiences will be influenced by many additional factors. For a thorough laboratory test of these dynamics, it is thus important to isolate the respective variables of interest in a structured design. A behavioral game provides an opportunity to actually observe several instances of valenced in- and outgroup contact and their effects on expectations concerning ingroup and outgroup members' behavior, as well as actual cooperative behavior toward them. This is a possibility that is unprecedented in intergroup contact research, especially in research that has focused more on general assessments of past contact and is thus not able to capture such temporally nuanced fluctuations in expectation and behavior.

For the present studies participants played a continuous prisoner's dilemma with respective interaction partners who were either members of their own group (ingroup) or a different group (outgroup; i.e., Study 1: younger vs. older students; Study 2: Dutch vs. international students). Participants thereby stated, for each round, how much they expected from the respective (anonymous) ingroup or outgroup member, and expressed cooperation by sending more or less money; we thus measured both expectations and actual behavior, respectively.

We specifically predict that:

Participants expect to receive more from and cooperate more with a person from their own than from the other group, an ingroup bias in expectations and cooperation (Hypothesis 1).

The individual's history of valenced intergroup contact affects subsequent effects of contact valence on expectations with the outgroup (Hypothesis 2). We furthermore explore the nature of this interaction, to compare the effects of valenced contact following a history of positive and negative contact.

In addition, we assume that expectations, in turn, predict the amount of cooperation within the respective round (Hypothesis 3).

## Pilot Study

We conducted a pilot study to confirm Dorrough et al. (2015) assumption that the difference score between expectations and actual received amount could indeed be used as a reliable indicator for contact quality. Specifically, we examined whether this difference score between received amount and expectation correlated with perceived contact quality.

## Method

### Participants

Participants were recruited in a face-to-face seminar, which was held at a distance learning university in Germany. Students at this university in general are older than typical university students (80% are currently employed and study part-time; Roth & Mazziotta, 2015), which allowed us to study intergroup behavior between age groups. At this distance learning university, students only attend two face-to-face seminars during the whole period of their undergraduate studies, thus most of the students scarcely know each other. For the pilot study, 40 participants (680 data points, age  $M = 33.97$ ;  $SD = 11.74$ ; female: 31 or 77.5%; male: nine or 22.5%) played 17 rounds of the behavioral game and were grouped into older ( $M = 43.90$ ,  $SD = 7.42$ ) and younger ( $M = 24.05$ ,  $SD = 4.50$ ) students.

### Materials and Procedure

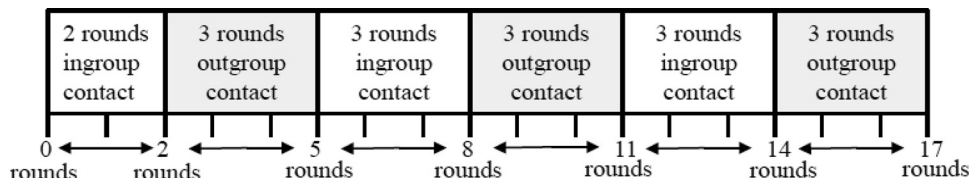
**Procedure.** We ran two different sessions of the behavioral games, each with new participants, during which 10 individuals from two different age groups played with their respective interaction partners (20 participants per session completed the behavioral game at the same time). Participants for the two experimental sessions were recruited from a university seminar, where participants were asked to line up according to their age. The group was subsequently split at the median age of each seminar's participants, and the first half of participants were then guided to the laboratory, whereby participants were randomly assigned to whether they would participate in the first or second trial. Once seated at their assigned computer, participants followed the procedure and rules for the behavioral game (which was programmed in oTree; Chen et al., 2016).

**Behavioral Game.** The pretest used the same behavioral games setting as Study 1 and 2. The game was played over 17 rounds. During these 17 rounds the participants first played two rounds with someone from the ingroup, followed by blocks of three rounds of outgroup contact, followed immediately by three rounds of ingroup interactions. Figure 1 illustrates the sequence of rounds.

This procedure was chosen to keep group membership salient, as previous research suggests that it is especially the change between ingroup and outgroup interactions that leads to an ingroup bias (Dorrough et al., 2015). At the beginning of each round, participants were randomly paired with a new player, and informed whether they were playing with a member of their in- or outgroup, and how many rounds remained until the end of the game. Additionally, if they were matched with an outgroup member on a given round, the background of the screen turned from white to blue. It is important to note that, during the game, pairing was entirely anonymous; thus, while participants knew whether they were playing with someone from their own or the other group,



**Figure 1**  
Sequence of Rounds Analyzed From the Behavioral Game



*Note.* Participants were randomly assigned a new fellow player in each round, who was drawn from either the ingroup or the outgroup, in line with the respective round number. For example, in Rounds 1 or 2, participants were randomly assigned to play with someone from their own group, while in Rounds 3, 4 and 5, the fellow player was drawn from the respective outgroup.

they did not know with whom specifically they were playing, which controls for potential retaliation effects.

Every player received an endowment of 10 units of the game's currency. One unit equaled 2 Euro-Cents. Next, participants stated how much they expected to receive from the other player. Subsequently, participants could transfer any integer between 0 and 10 from their own to their current partner's account. The amount they did not send remained in their own account and the sent amount was doubled and transferred to the partner's account. Finally, the interaction partners learned simultaneously about what their partner had sent them, and thus, how much was booked on their account at the end of the respective round.

If both players sent all 10 of their currency points, both ended up with 20 points in their account. In this way, cooperation by both parties led to a collective gain of twice the original amount. Yet, if one player decided to send nothing, and the other sent everything, the free-riding partner would end up with a maximum individual outcome of 30 points, while the sending partner received nothing. After their last round participants answered some questions assessing outgroup attitudes before they entered their bank account details to receive the money they had earned during the game.

### Measures

The behavioral game allowed us to observe actual computer-mediated inter- and intragroup contact, expectations about behavior, and actual behavior in a highly standardized setting. We assessed the expected amount at the beginning of each round, by asking participants to answer a one-item measure explicitly asking them to state how much of the game's currency (scaled from 0 to 10 units of the game's currency) they expected to receive from their fellow player in the upcoming round ( $M = 7.12$ ,  $SD = 3.06$ ). We assessed cooperation by means of the actual amount (ranging from 0 to 10) each player sent during each round ( $M = 7.01$ ,  $SD = 3.64$ ). Following the procedure proposed by Dorrough et al. (2015), the difference score indicator of intergroup contact quality was computed by subtracting the received amount from the expected amount ( $M = -0.11$ ,  $SD = 4.16$ ). In the pretest, participants were also asked to state after each round how they had perceived the contact quality of the respective round on a scale from 1 (*negative*) to 7 (*positive*;  $M = 5.41$ ,  $SD = 2.07$ ).

### Results and Discussion

We used Mplus 8 (Muthén & Muthén, 1998–2017) to estimate two-level hierarchical models to reflect that the 17 interactions are

nested within the 40 individuals. For this pretest we were only interested in whether, as proposed by Dorrough et al. (2015), the difference score indicator of contact quality (received minus expected amount), was correlated with the explicit measure of intergroup contact quality. We therefore analyzed the data of both groups, and all interactions. In line with our assumptions, the difference score indicator of contact quality was highly correlated with the rating-scale measure of perceived contact quality on a within person level ( $b = 4.86$ ,  $SE = 0.54$ ,  $p < .001$ ,  $\beta = .66$ ; Cohen, 1988), but was not correlated between individuals ( $b = 0.16$ ,  $SE = 0.23$ ,  $p = .487$ ,  $\beta = .13$ ). This finding supports the claim of Dorrough et al. (2015) that the difference score of received and expected amount provides a valid indicator of the perceived intergroup contact quality of intergroup encounters in an experimental game setting. These results assured us that the experimental design offers a valid procedure to examine the dynamic interplay of repeated intergroup (and intra-group) interactions in a controlled way.

### Study 1

Study 1 built on these first results of the pilot study, and was implemented in a setting with students of different age groups (i.e., younger and older students) providing the ingroup-outgroup distinction. We tested our three hypotheses: (a) participants expect to receive more from and cooperate more with the ingroup than the outgroup, reflecting an ingroup bias in expectations and cooperation (Hypothesis 1); (b) the individual's history of valenced intergroup contact affects subsequent effects of contact valence on expectations with the outgroup (Hypothesis 2); (c) expectations will predict the amount of cooperation within the respective round (Hypothesis 3).

### Method

#### Participants

Participants were recruited in three face-to-face seminars at the same university described in the pilot study. The final sample comprised 89 participants, 68 (76.4%) female and 21 (23.6%) male, which yields a total of 1,513 data points,<sup>1</sup> and age ranged from 18

<sup>1</sup>First simulation studies for dynamic structural equation modeling (DSEM; Asparouhov et al., 2018), and comparable models estimated with Bayesian methods, suggest that inferences, even from small samples ( $N = 20$ ), with few time points ( $T = 10$ ), can be used to make inferences. An increase in participants in particular thereby decreases the bias (e.g., Jongerling et al., 2015; Schultzberg & Muthén, 2018).



to 74, with a mean age of 32.38 ( $SD = 11.50$ ). These 89 participants were then grouped, for the purpose of the behavioral game, into 44 “older students” ( $M_{\text{age}} = 41.20$ ,  $SD = 10.08$ ) and 45 “younger students” ( $M_{\text{age}} = 23.76$ ,  $SD = 3.33$ ).

### Materials and Procedure

We ran five different sessions of the behavioral games, each with new participants, during which 10 to 12 individuals from two different age groups played with their respective interaction partners (i.e., 20 to 24 participants per session completed the behavioral game at the same time). Participants for these sessions were recruited in three seminars, where we followed the same procedure as described in the pretest. At their assigned computer, participants were first asked to reaffirm their group membership. They then stated their gender, and completed ratings of their identification with their respective ingroup, as well as in- and outgroup attitudes. Following these questions, participants read through a thorough explanation of the game’s rules before starting the behavioral game. This study received approval from the university’s ethics committee and was fully preregistered (<https://osf.io/4quhe>) and all materials, including data and program are available online ([https://osf.io/u4gfy/?view\\_only=6171d7504e9b477399e9013289c6394f](https://osf.io/u4gfy/?view_only=6171d7504e9b477399e9013289c6394f)).

**Behavioral Game.** For Study 1 we used a continuous prisoner’s dilemma with a complete stranger procedure (Ghidoni et al., 2018), whereby participants are not only randomly assigned to a partner at the beginning of each round, but also know that they will never play against the same person twice. This is important, as we are interested in expectations and cooperation toward the group rather than a given individual, and thus wished to avoid retaliation effects, which might occur if a person expects to playing with the same person twice. The game was played over up to 23 rounds. A total of 107 students participated in the assessment. However, due to the complete stranger matching procedure, in only 17 rounds did all of the included 89 participants play the same sequence of ingroup and outgroup interactions (based on two categories of “older” and “younger” students). Because the analysis required parallel data-structures, we only included these first 17 rounds in our analyses. As illustrated in the Pilot Study (see Figure 1), participants first played two rounds with someone from the ingroup, followed by blocks of three rounds of outgroup contact, followed immediately by three rounds of ingroup interactions, which were played subsequently, to keep group membership salient. Study 1 used the same behavioral game as described in the pilot study but did not include a measure of perceived contact quality after each round.

### Measures

The behavioral game allowed us to observe actual computer-mediated inter- and intragroup contact, expectations about behavior, and actual behavior in a highly standardized setting. We assessed the expected amount at the beginning of each round, by asking participants to answer a one-item measure explicitly asking them to state how much of the game’s currency (scaled from 0 to 10 units of the game’s currency) they expected to receive from their fellow player in the upcoming round ( $M = 4.61$ ,  $SD = 2.33$ ). We assessed cooperation by means of the actual amount (ranging from 0 to 10) each player sent during each round ( $M = 4.77$ ,  $SD = 3.23$ ). We calculated contact quality and history of contact for all

interactions, and for ingroup and outgroup interactions separately. The contact quality of the previous round was computed by subtracting the expected amount in the previous round from the amount the participant had actually received in the respective round. This measure thus ranged from  $-10$  to  $10$ , where negative scores indicate negative contact quality and positive scores indicate positive contact quality (for overall quality,  $M = 0.14$ ,  $SD = 3.89$ ; for ingroup contact quality,  $M = 0.16$ ,  $SD = 3.84$ ; for outgroup contact quality,  $M = -0.04$ ,  $SD = 3.94$ ). Because we wanted to test the effects of the history of intergroup contact on subsequent effects of contact, the history of intergroup contact was calculated as the mean of all contact quality ratings up to the second to last interaction. For example, in the third round, only the contact quality of the first round was included as a measure of contact history; while for the 17th round, the mean score included all contact experiences from the first to the 15th round, so that this variable could moderate the effect of the last contact on expectations in the subsequent round. Thereby, we accounted for all of an individual’s contact experiences so far (for overall history,  $M = 0.20$ ,  $SD = 1.94$ ; for ingroup contact history,  $M = 0.33$ ,  $SD = 2.20$ ; for outgroup contact history,  $M = -0.00$ ,  $SD = 2.40$ ).

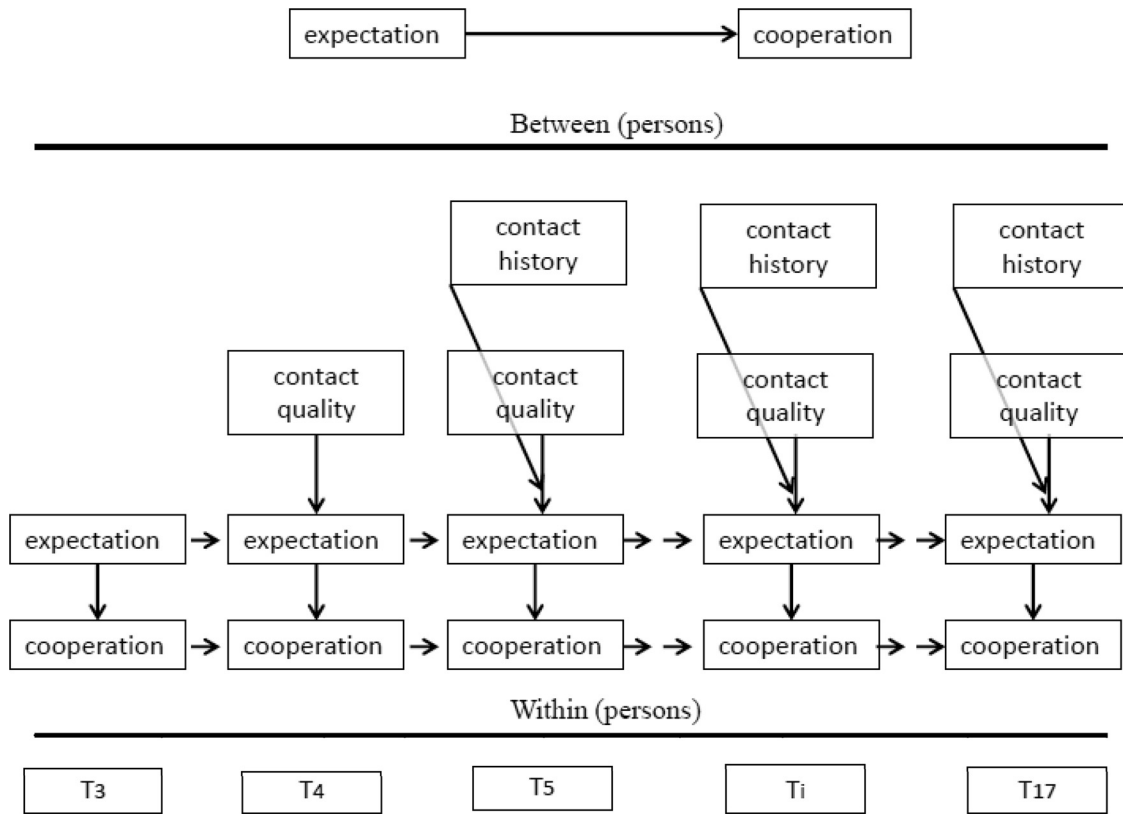
### Results and Discussion

We used Mplus 8 (Muthén & Muthén, 1998-2017) to estimate models from the *dynamic structural equation modeling* (DSEM) framework (Asparouhov et al., 2018), which not only differentiates between- and within-person parameters but additionally accommodates for within-person autoregressive effects. For a conceptual diagram of the full model, see Figure 2. DSEMs are estimated with Bayesian methods, the Markov chain Monte Carlo (MCMC) procedure and the potential scale reduction (PSR) criterion. For the purpose of this article we used uninformative priors. The data was clustered over 89 participants, and each of the 17 time points (nine for all analyses regarding outgroup interactions only) was regressed on the previous time point for our time-dependent variables, namely expected amount and actual amount of cooperation. If cooperation was included, expected amount was estimated to predict the cooperation on the between level. Expected amount and cooperation were modeled with lagged effects and cooperation was regressed on the expected amount in the same round. Quality of the last outgroup interaction as well as the history of intergroup contact were entered as predictors of expected amount and cooperation on the within level.

#### Ingroup Bias

We first tested for an ingroup bias and therefore included data from all 17 rounds of the prisoner’s dilemma. We therefore regressed both expected and sent amounts on a dummy variable on the within level, coding whether the participant was interacting with an outgroup (0) or an ingroup (1) member in the respective round. We found an ingroup bias for the amount expected,  $b = 0.23$ ,  $SD = 0.11$ ,  $p < .001$ , 95% CI [.01, .43], as well as for the amount sent,  $b = 0.41$ ,  $SD = 0.12$ ,  $p < .001$ , 95% CI [.18, .64]. This means that on average participants sent 0.4 units more of the experimental currency to ingroup as compared with outgroup members. These results confirm Hypothesis 1, because participants showed a clear ingroup bias with regard to expecting and sending more to their own than to the other group.

**Figure 2**  
Conceptual Diagram for the Full Model



*Note.* This figure depicts the conceptual diagram for the full model on the between and within levels, including only outgroup interactions. The third time point (T3) was the first outgroup interaction. The same pattern is repeated over all time points (Ti) that assessed outgroup interactions, up to the 17th round.

**Outgroup Contact**

In line with Hypothesis 2, we found that a history of valenced intergroup contact effects moderated the effect of intergroup contact on expectations in the respective subsequent round,  $b = 0.02$ ,  $SD = 0.01$ ,  $p = .008$ , 95% CI [.003, .03]. This effect suggests that there was a significant difference in the effects of the quality of contact in the previous round on expectations, depending on individuals' contact history. We subsequently examined the simple slopes for this interaction. As demonstrated in Figure 3, a negative history of contact experiences ( $-1 SD$ , thick black line) resulted in a weaker effect of contact on expectations,  $b = 0.17$ ,  $SD = 0.03$ ,  $p < .001$ , 95% CI [.12, .23], than a history of positive contact experiences ( $+1 SD$ , dashed line),  $b = 0.26$ ,  $SD = 0.03$ ,  $p < .001$ , 95% CI [.20, .32]. In line with Hypothesis 3, expectations in turn affected the behavioral outcome, cooperation, depending on the history of contact,  $b = 0.17$ ,  $SD = 0.01$ ,  $p = .009$ , 95% CI [.002, .03].

**Ingroup Effects**

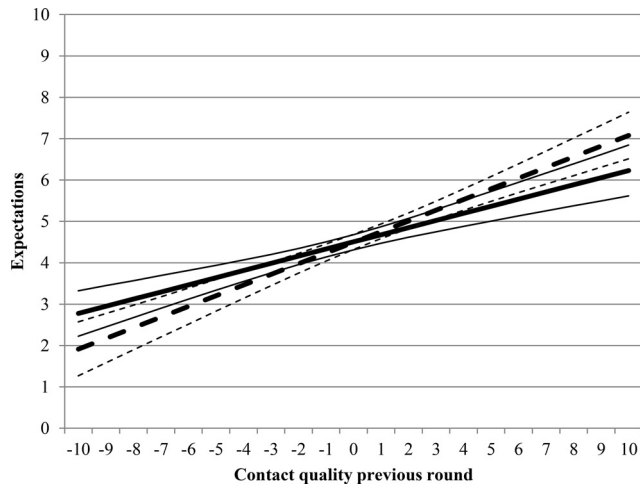
To demonstrate that the history of intergroup contact is specifically relevant in interactions with outgroup members (MacInnis & Page-Gould, 2015) we additionally estimated the same model for

interactions with the ingroup only, to test for contact effects and the interaction of history of contact. We did not find an interaction of history of ingroup contact and previous contact for interactions with ingroup members,  $b = 0.01$ ,  $SD = 0.01$ ,  $p = .227$ , 95% CI [-.01, .02]; the magnitude of the estimated coefficient was roughly halved compared to outgroup contact. Yet, we followed the recommendation by Cumming (2009) to compare the overlap of CI-Intervals, and results suggests that the difference between the effect of the ingroup and outgroup interaction effects was not significant.

Study 1 shows, in line with previous research (e.g., Balliet et al., 2014), that participants tend to expect more from and cooperate more with members of their own group than the outgroup (Hypothesis 1). It furthermore provides first evidence that a history of intergroup contact influences subsequent contact effects on both expectations and cooperation (Hypothesis 2). More specifically, a history of negative contact decreased the effect of subsequent interactions compared to a history of positive contact. In this first study we responded to the call to integrate a relative comparison of ingroup relative to outgroup interactions into intergroup contact research (MacInnis & Page-Gould, 2015) and demonstrated that effects of a history of intergroup contact on subsequent contact effects are specific to outgroup interactions, and do not occur for

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**Figure 3**  
*Expectations Depending on Contact Quality of Previous Round for a Positive and Negative Contact History*



*Note.* This figure shows individuals' expectations dependent on the contact quality of the previous round for a positive (+1 *SD*) history of intergroup contact (thick dashed line) and a negative (−1 *SD*) history of contact (thick black line) for Study 1. Figure includes upper and lower credible intervals (respective thin lines). Note that the interaction on the within level is plotted with the grand mean of expectations (4.50).

ingroup interactions. It should, however, be noted that in this study the interaction term for ingroup rounds was not significantly different from the interaction term for outgroup rounds. In addition, Study 1 supported Hypothesis 3, that an increase in expectations in turn increases cooperation. Overall, these results confirm a successful implementation of the chosen behavioral game paradigm to test questions arising from the established field of intergroup contact research. In the context of relations between younger and older students, levels of expectations and cooperation were moderately high (with mean scores reaching 4.5 out of 10 possible points; see Figure 3). Thus, even though we found a clear ingroup bias between the two groups, there was evidence of cooperation and positive expectations between the two groups, which might be due to the specific intergroup context considered here and the possibility that there may be few preexisting tensions between younger and older students.

## Study 2

To test the robustness and generalizability of the results of Study 1, we ran another study involving a different context of intergroup relations (involving Dutch and international students at a Dutch university). In Study 2 we again tested our three hypotheses by examining dynamic contact effects during repeated outgroup and ingroup interactions. First, we predicted that participants would expect to receive more from, and cooperate more with, the ingroup than the outgroup, an ingroup bias in expectations and cooperation (Hypothesis 1). Second, we predicted that an individual's history of valenced intergroup contact would impact subsequent effects of contact valence on expectations with the outgroup (Hypothesis 2). Building on the results from Study 1, we specified Hypothesis 2 accordingly, to predict that intergroup contact would have a weaker effect following a history of negative compared to positive intergroup contact. Third,

and finally, we assumed that expectations would predict the amount of cooperation within the respective round (Hypothesis 3, <https://osf.io/2zhdg>).

## Method

### Participants

Participants were recruited via several university mailing lists. We organized six sessions of the behavioral game, during which eight to 12 individuals from the group of Dutch or international students played with each other (thus 16 to 24 participants per session). Overall, 116 students participated (56 Dutch, 56 international students), of which 84 identified as female and 32 as male. This procedure yielded 2,668 data points.

### Materials and Procedure

Upon arrival for participation in the study, the two groups (i.e., Dutch and international students) were led into the same computer room separately in which the sessions were run. Once seated at their assigned computer, participants were first asked, offline, to read through the basic rules of the game and to generate a code to guarantee anonymous handling of their data. Afterward they were asked to start the computer assessment, following the procedure described in Study 1. This study received approval from a university's ethics committee and was fully preregistered (<https://osf.io/2zhdg>) and all materials, including data and program are available online ([https://osf.io/geacr/?view\\_only=17f3ef0413e643a9bf7bdb30cc7a7b1f](https://osf.io/geacr/?view_only=17f3ef0413e643a9bf7bdb30cc7a7b1f)).

**Behavioral Game.** For Study 2, we decided to change from a continuous prisoner's dilemma with perfect stranger matching to imperfect stranger matching, where participants are randomly assigned to their new partner, but may eventually end up with the same partner. The imperfect stranger matching design used in Study 2 had the advantage that it allowed us to include all available information, which is a benefit in comparison to Study 1, where we had to exclude several participants (because a complete stranger procedure would lead to different data structures for some participants, if the number of participants vary between trials). It is important to note that due to the imperfect stranger matching there was therefore a chance that individuals could end up playing with the same partner again. Still, as noted above, it was important for our research question to yield results comparable to perfect stranger settings, as we were interested in expectations of and cooperation toward the group and thus had to avoid retaliation effects, which might occur if a person expects to play the same person twice. Avoiding retaliation effects is ensured, by a low rematching probability. Previous evidence demonstrates that the results of imperfect stranger matching at a rematching probability of .2 or lower equal those of perfect stranger settings (see Ghidoni et al., 2018). As participants played in groups of eight to 12 participants, the rematching probability between two rounds ranged from .14 to .09, and from .13 to .08, when playing outgroup rounds. The game was played over 23 rounds, following the same structure as displayed in Figure 1, to keep group membership salient (Dorrrough et al., 2015). In all other respects Study 2 followed the same procedure as described for the pretest above, except for the small change that, due to university regulations concerning remuneration of research participants at the university at which

Study 2 was conducted, one unit of the game's currency equaled 4 instead of 2 Euro-Cents.

### Measures

As in Study 1, the expected amount was assessed at the beginning of each round and scaled from 0 to 10 of the game's currency,  $M = 2.77$ ,  $SD = 2.87$ . Cooperation refers to the amount each player sent during each round, and also ranged from 0 to 10,  $M = 2.76$ ,  $SD = 3.20$ . Following the procedures outlined for Study 1, we computed contact quality (for overall quality,  $M = -0.02$ ,  $SD = 3.64$ ; for ingroup contact quality,  $M = 0.02$ ,  $SD = 3.73$ ; for outgroup contact quality,  $M = -0.11$ ,  $SD = 3.76$ ) and the history of intergroup contact (for overall history,  $M = -0.09$ ,  $SD = 1.99$ ; for ingroup contact history,  $M = -0.02$ ,  $SD = 2.54$ ; for outgroup contact history,  $M = -0.15$ ,  $SD = 2.35$ ).

### Results and Discussion

Study 2 was estimated with the same model used for Study 1 (see Figure 2). In this study data was clustered over 116 participants, with 23 time points each (12 for all analyses regarding outgroup interactions only). We again used Mplus 8 (Muthén & Muthén, 1998-2017) to estimate models from the DSEM framework (Asparouhov et al., 2018).

#### Ingroup Bias in Expectations and Sent Amount

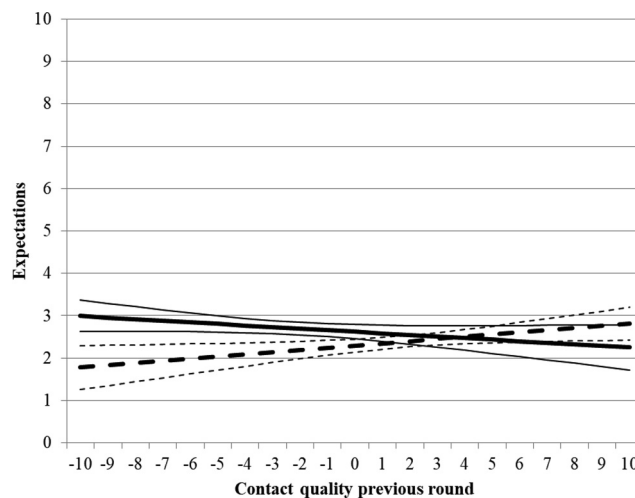
To estimate ingroup bias and test Hypothesis 1, we included data from all 23 rounds of the prisoner's dilemma. On the within level, we regressed the respective variables on themselves, as well as a dummy variable coding whether the participant was interacting with an outgroup (0) or an ingroup (1) member in the respective round. We found an ingroup bias for the amount expected,  $b = 0.51$ ,  $SD = 0.09$ ,  $p < .001$ , 95% CI [.34, .68], as well as for the amount sent,  $b = 0.54$ ,  $SD = 0.09$ ,  $p < .001$ , 95% CI [.37, .72]. This means that, on average, participants both expected from, and sent to, .5 units more of the experimental currency in the case of ingroup as compared with outgroup members. These results confirm Hypothesis 1, because participants show a clear ingroup bias with regard to expecting and sending more to their own than to the other group.

#### Outgroup Contact

For the test of whether the contact effect is moderated by the individual's history of intergroup contact (Hypothesis 2), we only included data of rounds played with an outgroup member. Confirming Hypothesis 2, we found that a history of valenced intergroup contact effects moderated the relationship between contact quality in the previous round and expectations in the subsequent round,  $b = 0.02$ ,  $SD = 0.01$ ,  $p < .001$ , 95% CI [.01, .03]. With every unit increase in more positive contact (above expectation) in earlier rounds, the absolute magnitude of positive and negative effects of quality of contact in the previous round increased by .02 units. We subsequently examined the simple slopes for this interaction.

As demonstrated in Figure 4, a negative history of contact experiences ( $-1 SD$ , thick black line) resulted in a weaker effect of contact on expectations,  $b = -0.04$ ,  $SD = 0.02$ ,  $p = .043$ , 95% CI  $[-.08, .01]$ , than a positive history ( $+1 SD$ , dashed line) of contact

**Figure 4**  
*Expectations Depending on Contact Quality of Previous Round for a Positive and Negative Contact History*



*Note.* This figure demonstrates the expectations depending on the contact quality of the previous round for a positive ( $+1 SD$ ) history of intergroup contact (thick dashed line) and a negative ( $-1 SD$ ) history of contact (thick black line) for Study 2. Figure includes upper and lower credible intervals (respective thin lines). Note that the interaction on the within level is plotted with the grand mean of expectations (2.45) and that the effect for a negative history of intergroup contact does not reach significance.

experiences,  $b = 0.05$ ,  $SD = 0.02$ ,  $p = .009$ , 95% CI [.01, .10]. Furthermore, participants who expected more from their outgroup partner sent more to that partner,  $b = 0.60$ ,  $SD = 0.02$ ,  $p < .001$ , 95% CI [.56, .65], which is in line with Hypothesis 3.

#### Ingroup Effects

To demonstrate that the history of intergroup contact is particularly relevant for interactions with outgroup as opposed to ingroup members (MacInnis & Page-Gould, 2015), we additionally estimated the same model pertaining to interactions with the ingroup. In this case, we did not find a significant interaction of history of ingroup contact and previous contact,  $b = -0.01$ ,  $SD = 0.01$ ,  $p = .188$ , 95% CI  $[-.02, .01]$ ; thus, the magnitude of the estimated coefficient for ingroup interactions was roughly halved compared to outgroup contact. Following the recommendations by Cumming (2009) to compare the overlap of confidence intervals suggests that this difference was significant.

Study 2 provides further support for our three hypotheses. In line with Hypothesis 1, participants showed a clear ingroup bias both with regard to expecting and to sending more to their own than to the other group. Furthermore, our results support the assumption that a history of valenced intergroup contact experiences moderates the effects of contact quality on subsequent expectations (Hypothesis 2). Thus, the same interaction pattern observed in Study 1 emerged. Moreover, as an extension to Study 1 we had explicitly preregistered the direction of this interaction: As predicted, intergroup contact following a history of negative contact had weaker effects on expectations than contact following a history of positive contact. Again, the same interaction did not yield



significant results when probing for it in the realm of ingroup interactions; moreover, for Study 2 this difference between ingroup and outgroup effects was significant. In addition, Study 2 demonstrated that an increase in expectations also translated into an increase in cooperation with the outgroup, which is in line with Hypothesis 3. In this sample of Dutch and international students, expectations and cooperation yielded overall means well below the midpoint of the scale (see Figure 4), which supports our assumption that this setting would be characterized by more intergroup tension to begin with than that of Study 1.

### General Discussion

Building on evidence accrued from 4,181 interactions collected across two studies involving different intergroup contexts, the current article demonstrates that a history of valenced intergroup contact influences subsequent contact effects on both expectations and cooperative behavior with the outgroup. More specifically, we found in both studies that a history of negative intergroup contact reduced the effect of subsequent positive intergroup contact on expectations toward the outgroup, while a history of positive intergroup contact increased the effects of contact. Specifically, positive intergroup contact following a history of positive contact increased expectations and cooperation with the outgroup more than positive contact following a history of negative contact. On the other hand, negative intergroup contact after a history of negative contact did not reduce expectations and cooperation as much as negative contact following a history of positive contact.

Our findings extend the emerging research on positive and negative intergroup contact in three key ways, and thereby also address current critiques of intergroup contact research. Specifically, our findings help shed light on the complexity of intergroup contact experiences as they are experienced in real-world settings in a way that could not be tapped by more conventional ways of measuring intergroup contact. First, using a behavioral game in the context of intergroup contact research allowed us to observe a sequence of actual behaviors with different ingroup or outgroup members, thereby providing a first step in addressing calls both to address the temporal dimension of contact (MacInnis & Page-Gould, 2015) and to bring behavioral measures back into “the science of behavior” (Baumeister et al., 2007, p. 396), at least by observing individuals’ interactions. Second, this procedure allowed us to model the change of expectations and cooperation within individuals over distinct instances of intergroup contact. This is in contrast to most studies on intergroup contact, which predominately use more general measures of contact, typically involving overall assessments of contact that consider larger time-spans (and often in self-report format). Conversely, while most studies on short-term intergroup interactions focus on aspects such as nonverbal behavior but neglect intergroup measures, our research specifically considered such intergroup measures concerning both expectations of and cooperation with both ingroup and outgroup members (MacInnis & Page-Gould, 2015). Third, including not only outgroup, but also ingroup contact allowed us to demonstrate that these findings appear indeed be specific to repeated outgroup interactions. We now discuss the main issues arising from this research, acknowledge some limitations, and identify areas for future research.

### Contact History Matters for Intergroup Contact Effects

Our finding that a history of intergroup contact moderates contact effects is in line with our assumptions and previous research (e.g., Árnadóttir et al., 2018; Paolini et al., 2014). With regard to the nature of this interaction (i.e., a stronger effect of intergroup contact following a history of positive, compared with negative, contact) between members of ingroup and outgroup, our results cannot be explained by theoretical accounts that do not differentiate between the valence of previous experiences, such as adaptation-level-theory (Helson, 1948; Ritter et al., 2016) or perceived-fit hypotheses (Paolini et al., 2014). Instead, our results suggest that the valence of previous experiences matters: The stronger effect of negative intergroup contact following a history of positive contact is in line with results from research on impression formation (e.g., Briscoe et al., 1967) and expectation violations (e.g., Austin & Walster, 1974).

Thereby, our results on the exacerbation of negative effects are in line with some previous research in the realm of intergroup contact (e.g., Hayward et al., 2017), but contradict previous accounts reporting buffering effects (e.g., Árnadóttir et al., 2018). This contradiction may, however, be more apparent than real. As we noted above, previous studies typically assessed intergroup contact with measures gauging overall contact over larger time spans, whereas the present study looked at distinct instances of valenced intergroup contact in a sequential manner. Moreover, these previous studies were limited in their number of waves of available data, which constrained appropriate ways to analyze the data, and thus might not have appropriately differentiated between changes within and between persons (Hamaker et al., 2015). By testing repeated interactions over a series of up to 23 interactions our research was able to separate within and between person effects.

### Using Behavioral Games to Study Intergroup Contact

Both studies not only replicated an ingroup bias in behavioral games (e.g., Balliet et al., 2014), but went beyond this by demonstrating that the core assumption of intergroup contact theory, namely the increase in cooperation after positive contact (e.g., Pettigrew & Tropp, 2006), holds when tested in the realm of a behavioral game paradigm. Additionally, we demonstrated this effect on actual measures of cooperative behavior, and not just mere positive outgroup perceptions. These findings confirm that behavioral games provide a novel but particularly fruitful paradigm for studying intergroup contact, one that allows researchers to include and account for the dynamic and reciprocal nature of contact that is typically missing in intergroup contact research. Furthermore, we only obtained interaction effects of the valenced history of contact with subsequent contact effects for contact with the outgroup, but not the ingroup. Failing to find that the history of contact moderates contact effects in the case of ingroup interactions is, to our knowledge, a new insight—but one that is of vital importance to understanding intergroup contact effects. Indeed, previous critiques have pointed out that intergroup contact research has far too often relied on a comparison of having no or few intergroup contacts to having many intergroup contacts, instead of additionally addressing the comparison of outgroup to ingroup contacts (MacInnis & Page-Gould, 2015). Yet, we must note that although both our studies failed to find a significant interaction effect of a history of contact with subsequent contact

effects for ingroup contact, in Study 1 the effect of the nonsignificant interaction in the case of ingroup contact did not significantly differ from the significant interaction effect found for outgroup contact. Nonetheless, it still seems plausible from a theoretical perspective to find a weaker influence of previous experience on subsequent experience in the realm of ingroup compared with outgroup interactions: Outgroups are generally perceived to be more homogeneous (e.g., Judd & Park, 1988) and outgroup members are perceived to be more similar to each other than ingroup members (e.g., Crump et al., 2010), which might lead to higher expectations of consistent behavior toward the out-, but not the ingroup, thereby facilitating the generalization of experiences to other members of the respective group. Additionally, expectations of and attitudes toward ingroups might be held with greater certainty (e.g., Christ et al., 2010), due to more frequent and direct experiences with the ingroup; this might explain why ingroup expectations are less likely to be affected by only a select few experiences. Our findings in this realm provide initial insights, which need to be backed up by future theorizing and empirical evidence.

Our novel approach to analyzing observed intergroup interactions in a behavioral game further allowed us to address the concern that social psychology has largely neglected actual behavior (e.g., Baumeister et al., 2007; although we are admittedly observing behavior in an artificial setting). It also provided a highly standardized measure of contact quality and provided a robust test of our assumptions.

### Limitations and Future Directions

Notwithstanding this novel contribution to studying the dynamic and sequential nature of valenced intergroup contact, we acknowledge some limitations of this research. First, while we argue that this paradigm provides a useful model to examine our research questions derived from intergroup contact theory, the highly standardized procedure reduces its ecological validity to some extent (Lodewijk et al., 2006). For example, more complex, real-world intergroup interactions might be evaluated positively and negatively at the same time (Cacioppo et al., 1997), which we were unable to capture here. Using more complex simulations, such as in virtual reality environments, might help to increase the ecological validity without losing too much experimental control.

Second, our research focused on intergroup settings that arguably entail low levels of intergroup conflict (i.e., younger vs. older age groups, Dutch vs. international students). While cooperation between these groups nonetheless remains an important part of modern societies, research involving more conflictual intergroup settings might yield different results. Having said this, levels of cooperation were already rather low between international and Dutch students in Study 2, reflecting that our behavioral game occurred in an intergroup context in which intergroup relations were not benign. Future research, as well as cautiously addressing more conflict-laden settings, could also use behavioral games other than the prisoner's dilemma, which could also address other underlying motivations (e.g., Thielmann et al., 2015). Furthermore, the group of international students, as groups based on students of different ages, are rather heterogeneous groups within themselves; future research might want to address this by either systematically varying the homogeneity of the respective groups, or choosing more homogenous groups. Nonetheless, researchers interested in extending this research to more

extreme or conflictual settings of intergroup relations should carefully consider ethical concerns before allowing members of groups to experience positive and especially negative behaviors in a setting in which the researcher is merely observing actual behavior.

Third, our repeated instances of contact happened over a relatively short time span; future research could search for ways to complement such short-term interactions with observations of interactions over larger timespans, as intergroup contact effects might change over larger time-intervals, such as days or months. To further increase comparability of our results to large scale studies of intergroup contact over larger time-spans, it would also be interesting for future research to consider including a more traditional prejudice measure alongside the behavioral bias employed within the dynamic paradigm employed here, and to more closely examine the role of perceived contact quality as a potential mediator of contact effects. However, we wish to point out that explicitly asking about the perceived quality for each interaction, as well as trying to measure intergroup attitudes using a more traditional self-report measure after each round, would likely not only affect the duration and experience of the game, but, more critically, might affect the behavioral outcome and overall pattern of results in undesirable ways, for example by sensitizing participants to the hypotheses being. We thus recommend that researchers interested in complementing our design with more traditional prejudice measures in future work carefully consider which measures to use, and at which point during the game to place them.

Overall, our research opens up new avenues to examine theoretical extensions of our findings. We were prevented from using dynamic structural equation models with random rather than fixed effects (Asparouhov et al., 2018), as that requires much higher numbers of participants (Schultzberg & Muthén, 2018), which is something future research may seek to do. Using random effects models would allow researchers to include differences in these contact effects between participants and, for example, to consider gender or other person-level variables, which might help to close the gap with previous studies on intergroup contact using overall measures of contact, often using cross-sectional data. Additionally, further studies should test whether perceived similarity between outgroup but not ingroup members explains why we only find a moderation of a history of contact for outgroup, but not for ingroup contact. In this vein, systematically increasing the probability that individuals might interact with another player more than once during the game would be an interesting extension to further research. Furthermore, experimental games could easily be used to additionally manipulate the status or power of the groups (i.e., majority vs. minority, advantaged vs. disadvantaged), which might also affect critically impact intergroup contact effects (Tropp & Pettigrew, 2005).

### Conclusion

Taken together, our research provides an important extension of current research and theorizing on intergroup contact and intergroup interactions. It points to the importance of not only considering overall ratings of contact experiences over larger time spans, but also studying the dynamics of specific sequential and cumulative intergroup interactions over time. Positive and negative contact typically occur in complex combinations and individual experiences thereof influence the effectiveness of subsequent intergroup contact

experiences. Using behavioral games for intergroup contact research thus provides a wide range of opportunities to study dynamic aspects of intergroup experiences, and offers promising avenues for future work in this field. Some future research questions which might be addressed with this paradigm include possible effects of ingroup behavior on expectations toward the outgroup and vice-versa (Locksley et al., 1980), the importance of first encounters with outgroup members, and the question of how many valenced intergroup contact experiences it might take to cancel out the experience of contact of the opposite valence.

To conclude, even though intergroup contact theory remains an important framework for addressing ongoing challenges in modern and increasingly diverse societies, our results add to the valid concern that negative contact may curb the desired effects of positive contact (e.g., Barlow et al., 2012; Paolini et al., 2010). Our research has provided a novel paradigm to explore pertinent research questions on valenced contact. These findings also highlight the importance of recognizing that positive as well as negative contact can be influenced by the full history of valenced contact, which, for the individual, can change over time, and which we should be modeling in our research.

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