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Negativity Bias in Intergroup Contact: Meta-Analytical Evidence That Bad Is Stronger Than Good, Especially When People Have the Opportunity and Motivation to Opt Out of Contact

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Seventy years of research on intergroup contact, or face-to-face interactions between members of opposing social groups, demonstrates that positive contact typically reduces prejudice and increases social cohesion. Extant syntheses, however, have not considered the full breadth of contact valence (positive/negative) and have treated self-selection as a threat to validity. This research bridges intergroup contact theory with sequential sampling models of impression formation to assess contact effects across all valences. From the premise that positive versus negative contact instigates differential resampling of outgroup experiences when self-selection is possible, we advance and meta-analytically test new predictions for the moderation of valenced contact effects and negativity bias as a function of people's opportunity and motivation to self-select in and out of contact. Our random-effects synthesis of positive and negative intergroup contact studies (238 independent samples, 936 nested effects; total $N = 152{,}985$) found significant valenced contact effects: Positive contact systematically associates with lower prejudice, and negative contact associates with higher prejudice. Critically, the detrimental effect of negative contact is significantly larger than the benefit of positive contact. This negativity bias is particularly pronounced under conditions in which one can self-select, is motivated to avoid contact, among male-dominated and prejudiced samples, in contact with stigmatized, low status, low socioeconomic status outgroups, along nonconcealable stigma, with nonintimate contact partners in informal settings and in collectivistic societies. Considering individuals' motivation and opportunity to self-select, together with contact valence, therefore offers a more nuanced and integrated platform to design contact-based interventions and policies across varied contact ecologies.

Public Significance Statement
This meta-analysis extends earlier analyses of intergroup contact, or face-to-face interactions between members of opposing groups, showing that the quality of contact and opportunities and motivation to avoid contact matter. It demonstrates that positive contact improves intergroup relations, but negative contact worsens them and is more impactful. Negative contact prevails and is thus riskier in real-world settings, offering opportunity and motivation to avoid contact. This enriched knowledgebase can help maximize the benefits of intergroup contact and minimize its risks through more targeted policy and intervention.

Keywords: negative intergroup contact, prejudice, self-selection, contact opportunity, contact avoidance

Supplemental materials: https://doi.org/10.1037/bul0000439.supp

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All data and research materials, including coding protocol and R codes, are publicly available as Open Science Framework supplemental documents at https://osf.io/38rpj/ (see Appendices index for roadmap to file names). Core materials are available as online Supplemental Materials.

Stefania Paolini played a lead role in conceptualization, data curation, funding acquisition, investigation, methodology, project administration, resources, software, supervision, validation, writing–original draft, and writing–review and editing. Meghann Gibbs played a supporting role in conceptualization, data curation, formal analysis, investigation, methodology, project administration, visualization, and writing–original draft. Brett Sales played a lead role in data curation and validation and a supporting role in formal analysis, visualization, and writing–review and editing. Danielle Anderson played a supporting role in data curation, formal analysis, validation, and visualization. Kylie McIntyre played a supporting role in data curation, methodology, and validation.

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It is well established in social psychology that face-to-face interactions between members of opposing groups or intergroup contact can contribute to social cohesion by reducing intergroup prejudice, increasing mutual trust, and improving behavioral intentions toward the outgroup. These conclusions, however, reflect syntheses of early intergroup contact research (Davies et al., 2011; Lemmer & Wagner, 2015; Pettigrew & Tropp, 2006) devoted to establishing the value of intergroup contact for achieving social integration and intergroup harmony (Graf & Paolini, 2016; Paolini et al., 2021; Pettigrew, 1998), against a historical backdrop of intergroup segregation and skepticism about the power of trespassing group boundaries (Baker, 1934; Forbes, 1997; Ford, 1986; McClendon, 1974) at the time of the U.S. civil rights movement and the South African fight against apartheid (Dixon et al., 2005; Pettigrew, 1998).

Given its original zeitgeist, early intergroup contact research was more concerned about applying and evaluating intergroup contact in structured social interventions and less so about unveiling basic psychological processes associated with intergroup contact (Paolini & McIntyre, 2019). The first 60 years of intergroup contact research thus disproportionately sampled and investigated positive intergroup contact and contact-based intervention programs designed to fast-track benign intergroup relations and intergroup harmony (Dixon et al., 2005). In this early work, indicators of contact quantity and contact quality were treated analytically as interchangeable proxies of the independent variable in line with unvalenced conceptualizations of intergroup contact prevalent at that point in time. Ninety-four percent of the studies included in a landmark meta-analysis of half a century of contact research (Pettigrew & Tropp, 2006) recorded a negative association between contact and prejudice and an overall negative effect of small-to-medium size.

This early research and associated syntheses were therefore necessarily ill-equipped to appraise contact effects across the full breadth of contact valence—positive versus negative intergroup contact—or variations in valenced contact effects. These research trends most likely form an incomplete basis to inform contact-based interventions and policies under varied contact contingencies in naturalistic settings (Paluck et al., 2019; Paolini et al., 2010).

A growing interest in exploring the fundamentals of intergroup contact effects and returning a fuller understanding of its precise potential has recently given impetus to fresh research on positive and negative intergroup contact (Schäfer et al., 2021). The notion of a negativity bias in intergroup contact effects or a negative valence asymmetry in impact was first advanced (Paolini et al., 2010) and prospectively tested (Barlow et al., 2012), extending plentiful evidence that bad is stronger than good in many psychological domains (Baumeister et al., 2001). When a negativity bias in intergroup contact holds, negative intergroup contact leads to larger increases in outgroup prejudice than reductions in prejudice following positive contact. In other words, negative individual-to-group generalizations following negative contact should be larger than positive individual-to-group generalizations following positive contact, and this is because negative contact causes increases in the attendance of intergroup differences during contact (or category salience; Paolini et al., 2010).

Given the literature's early fascination with positive contact and diffuse confidence in the benefits of intergroup contact, the idea of a negativity bias has been contentious, contested, and at minimum untested, as it suggests the possibility that the well-established benefits of positive contact could be rendered insignificant or even reverted when negative intergroup contact occurs and is fully assessed. Despite this resistance, research designs that consider both positive and negative intergroup contact have since become the norm in intergroup contact research. This novel research trend has helped unearth a second kind of asymmetry in intergroup contact: Positive contact with outgroup members is often more prevalent in people's reports of intergroup contact than negative contact (Graf et al., 2014). Evidence for positive asymmetries in prevalence is growing at a fast pace (Schäfer et al., 2021) and is noteworthy. It implies that, at least at the societal level, the greater detrimental impact of relatively rare negative intergroup contact could be progressively eroded and possibly even counteracted by the cumulative impact of a large number of modestly beneficial positive contact experiences. In addition, a meta-analysis of experimental research on stereotype change has corroborated the existence and causal direction of negativity biases in prejudiced judgments of stigmatized outgroups, following valenced indirect experiences with specific outgroup members (Paolini & McIntyre, 2019).

Prospective field tests of negativity biases in direct, face-to-face contact have, however, returned mixed findings (Paolini & McIntyre, 2019; Schäfer et al., 2021; Zingora et al., 2021 for overviews). This unexplained variability in results is problematic for progress in both theory and praxis; it injects uncertainty about whether negativity biases are present in people's dynamic, often repeated, affect-driven contact experiences with outgroup members as they occur in naturalistic settings. To address this impasse in contemporary analyses of valenced contact, the present research will systematically interface intergroup contact theory with relevant advances in impression formation research.

Bridging Contact Theory With Sequential Sampling Models of Impression Formation

> Biases in impression formation are likely to be stronger in situations in which information has to be actively sampled.—(Denrell, 2005, p. 968)

> The stimulus input to real learning processes depends on the individual's selective attention and sampling preferences in a complex world that is replete with multiple information sources.—(Fiedler et al., 2013, p. 222)

Although to date substantially underutilized, contemporary models of sequential experience sampling (Denrell, 2005; Fiedler, 1996, 2000; Kashima et al., 2000; Van Rooy et al., 2003) have high relevance for intergroup contact because in naturalistic settings, individuals have regular opportunities and some freedom to actively sample experiences with outgroup members; they can and often self-select in and out of intergroup contact.

Based on this premise and models, we will put to empirical test an integrated suite of novel predictions of moderation of valenced contact effects. We will contend that valenced contact effects should be moderated by one's opportunity to self-select and by several factors linked to one's motivation to self-select, including prejudiced attitudes, contact intimacy, valenced outgroup stereotypes, and conflict settings. We will test these novel predictions for moderation meta-analytically.

Self-Selection Should Moderate Valenced Contact Effects and Amplify Negativity Biases

According to Denrell's (2005) model, the valence of initial experiences with others should profoundly affect how we approach future interactions with these individuals and the resulting impressions and judgments of these and associated targets. Based on purely hedonic considerations, people should be more likely to continue to interact with these contact partners (or sample more experiences/resample experiences with them) if they had an initial positive experience than if they had an initial negative experience. If they had a negative experience, they are unlikely to go back and sample more experiences (see also Thorndike's, 1898 law of effect).

Denrell's model advances these predictions from a hedonic standpoint and aligns with a significant body of work showing that individuals naturally seek good experiences and avoid bad ones (Feldman, 2004; Thorndike, 1898). Hence, future interactions with a specific contact partner are more likely if previous interactions suggest that future ones will be rewarding or pleasurable (Altman & Taylor, 1973; Berscheid, 1985; Homans, 1961; Lott & Lott, 1972; Montoya & Horton, 2004; Newcomb, 1953; Sunnafrank, 1986; Thibaut & Kelley, 1959). These patterns have been corroborated in several experimental and field studies. For example, experiments about rewards during interactions show that willingness to continue the interaction is higher if one has experienced rewarding interactions (Chambliss, 1966; D. A. Taylor et al., 1969, p. 333). In a similar vein, attraction ratings of a future interaction partner based on initial information predict one's willingness to continue the interaction (Montoya & Horton, 2004, p. 700; Schwartz, 1966, cited in Byrne, 1971, p. 231).

These same predictions of differential resampling following valenced contact also stand when physical and psychological safety alone are at stake (Baumeister et al., 2001; Murray & Schaller, 2016; Neuberg et al., 2011): Individuals should be more likely to resample experiences with outgroup members if previous interactions have been safe and comfortable rather than unsafe and uncomfortable, because in so doing, safety, integrity of the organism, and survival are more likely to be achieved.

Thinking of these processes from the perspective of judgment accuracy, differential resampling as a function of initial interaction valence implies that false negatives about others' qualities should be more common than false positives because people are likely to stop sampling after negative experiences and thus reduce the opportunities to subsequently correct their impressions with newer/more information (see Denrell, 2005, p. 963). This pattern obviously would offer a particularly fertile ground for the development of stereotypes and the perpetuation of stigmatization, yet it should be modulated by competing motivations, like a desire to develop accurate knowledge structures (Fiedler et al., 2013), a desire to protect them, or bend them at the service of ingroup self-enhancing views (Paolini & McIntyre, 2019).

For the sake of the effects at the core of this work, differential resampling provides the "perfect storm" for the emergence of negativity biases in generalized outgroup judgments or individual-to-group generalizations following intergroup contact. Because positive experiences with outgroup members are likely to lead to resampling, impressions, and judgments of those (initially positive) contact partners, and their group should ultimately reflect a large repertoire of varied experiences and information. In contrast, as negative experiences are likely to lead to limited or no resampling, the impressions and judgments of (initially negative) contact partners and their group should reflect a small repertoire of relatively homogenous (negative) experiences and information. Impressions and judgments following positive experiences with outgroup members should therefore be relatively complex, information-rich, malleable, and moderate in nature. Whereas impressions and judgments following negative experiences should be relatively simple, information-poor, monolithic, and polarized.

It is important to appreciate that this differential resampling explanation for negativity biases does not rely on cognitive or motivational biases internal to the individual. Instead, the mere environmental affordances for opting in and out of experiences with outgroup members based on initial contact valence would suffice for these judgment biases to manifest. Self-selection would inject a bias in the information structure available to the individual (Denrell, 2005, p. 257). The information or experience set available for their impressions and judgments would ultimately depend on the outcome (positive vs. negative) of past interactions with outgroup members and associated opportunities to proactively shape and "curate" upcoming experiences with the outgroup.

This explanation for negativity biases is logically—although not necessarily empirically—independent from explanations based on an attentional advantage of negative information/experiences (i.e., higher diagnosticity; Fiske, 1980; Reeder & Brewer, 1979; Ybarra, 2002; for a review, Skowronski & Carlston, 1989; for a comparison, Fiedler et al., 2013) or self-referential information/experiences (Fazio & Zanna, 1981; Pryor et al., 1977; Zanna et al., 1981; see also Dardenne et al., 2000; Denton, 2018). We now know that under natural, unstructured, and unmonitored conditions, negative intergroup contact is often a rare event compared to positive contact (Graf et al., 2014; Schäfer et al., 2021). Thus, negativity biases in intergroup contact might also present because of the attentional advantage implicated in the rarity of negative contact and/or the self-referential nature of these experiences compounds with the impact of differential resampling. Yet, these attentional mechanisms would lead to negativity biases because negative or self-relevant information acquires higher weight in impressions and judgment due to its rarity, diagnosticity, or encoding richness (Fiedler et al., 2013); the differential resampling mechanism instead would lead to negativity biases even if all experiences (positive and negative; self-referential and non-self-referential) have the same weight (see, e.g., Denrell, 2005, pp. 957–960). We opted to focus on differential resampling over other explanations of negativity biases in this work because of this mechanism's ability to serve as a parsimonious, generative, and integrative framework for the rendering of novel predictions about the moderation of valenced contact effects in naturalistic settings.

Denrell's model predicts that self-selection moderates valenced contact effects and provides a basis for negativity biases in intergroup contact: Negativity biases should be more pronounced and more common in contexts where individuals have opportunity to freely self-select in and out of their contact experiences, compared to contexts where, for example, labor divisions, hierarchies, or roles severely restrict individuals' freedom to self-select future interaction partners (see also Harwood, 2021). Overall, the differential impact of valenced experiences with outgroup members on outgroup judgments should be more pronounced under conditions of ample, rather than restricted, self-selection because, under ample self-selection, contact valence can profoundly shape the size of the information and experiences contributing to impressions and outgroup judgments.

Other Valenced Expectations About Contact Should Also Moderate Valenced Effects

The effects of differential resampling on impressions and outgroup judgments should not be context-invariant. Instead, these effects should be further moderated by a range of factors that are predictive of valenced expectations about the outgroup, the contact partners, and the broad intergroup context. Based on Denrell (2005), this host of valenced expectations should also moderate individuals' proclivity to resample experiences with outgroup members and thus further modulate the size of negativity biases.

Interfacing intergroup contact theory with sequential sampling models is therefore generative also because it helps us deepen and advance our understanding of factors that have already gained significant attention in intergroup psychology, helping us cast a broad net of auxiliary predictions that well-established moderators of prejudice will moderate valenced contact effects and the size of negativity biases in naturalistic settings. We review the main types we put to empirical test in this meta-analysis.

Valenced stereotypes about the outgroup, as derived from the outgroup's perceived valence, status, and socioeconomic status (SES), evoke expectations about intergroup interactions before contact has even occurred (Fiske et al., 1999; Hamilton et al., 1990); they shape impression formation during contact and retrospective appraisals after contact has taken place (Zingora et al., 2021). According to Denrell's model, preexisting outgroup stereotypes should moderate negativity biases under conditions that allow some self-selection: When there is some freedom to opt in/out of intergroup contact, individuals should be particularly unwilling to resample experiences with outgroup members that have been negative if they also hold negative expectations about their group as a whole (i.e., as being stigmatized, low in status, or SES). On the contrary, they might be more willing to resample despite the negative experience if the contact partner belongs to an admired and respected outgroup (i.e., admired, high in status, or SES). The reverse pattern should be present for positive experiences: Individuals should be even more willing to resample after positive experiences if the contact partner belongs to an admired and respected group. They are likely to be less motivated to resample, even if the experience was positive, if the contact partner belongs to a stigmatized group. In other words, based exclusively on differential resampling, under self-selection conditions and once again attention biases aside, negativity biases should be more pronounced in contact with members of stigmatized, low status, low SES groups and should be significantly muted, but perhaps not completely erased, in contact with members of admired, high status, high SES groups, because the valence of the initial contact should still lead to some differential re-sampling.

This reasoning can be easily extended to valenced expectations for the broader intergroup context in which contact occurs and for variations in egalitarian versus prejudiced attitudes of the individual. It is reasonable to expect that individuals will be less willing to resample experiences with outgroup members in conflict-ridden rather than peaceful settings (Meleady & Forder, 2019; Tabory, 1993), and if they are prejudiced rather than tolerant in their intergroup attitudes (Dhont & Van Hiel, 2009; Hodson, 2011), to some degree, irrespective of contact's initial valence. Accordingly, conflict settings and prejudiced attitudes should be associated with exacerbated negative valence asymmetries, whereas peaceful settings and tolerant attitudes should be associated with muted asymmetries.

Drawing from Denrell (2005), considerations of valenced expectations about the contact partner, based on their closeness, intimacy, and familiarity with the individual, should function in a similar manner: Intimacy and familiarity should moderate the differential impact of contact valence on subsequent sampling, impressions, and judgments. There is an expansive and established body of work within the contact literature highlighting the benefits of intimate, close contact with outgroup members for the reduction of intergroup prejudice (Paolini et al., 2004; R. N. Turner & Feddes, 2011; for reviews, Davies et al., 2011; R. N. Turner et al., 2007). This work still has systematically focused on positive contact and mostly neglected broader variations in valenced contact (however, see Fuochi et al., 2020; Graf, Paolini, & Rubin, 2020). Based on sampling models, the intimacy and familiarity of the contact partner should mute the impact of valenced contact because individuals are more likely to resample experiences with intimate or familiar contact partners, to some extent, irrespective of whether the experience was previously positive or negative. The larger resampling would ultimately dilute and mute the impact of both negative and positive experiences on outgroup judgments, as well as attenuate their differential impact (negativity bias). In this meta-analysis, we coded eligible studies along several proxies of intimacy/familiarity that neatly tap either closeness features of intimacy or informality features of intimacy (Fuochi et al., 2020; Graf, Paolini, & Rubin, 2020). We expected ordinal contact valence by intimacy/familiarity interactions reflective of smaller negativity biases under intimate/familiar contact.

The present meta-analysis tested the influence of these additional factors as potential moderators of valenced contact effects and negativity bias in data offering some affordances for self-selection. Whenever possible, we carried out our tests using parallel proxies of these constructs as drawn from distinct procedural aspects of eligible contact studies.

This Meta-Analysis of Valenced Intergroup Contact Effects

This research aims to advance understanding of how and why valenced face-to-face intergroup contact shapes generalized outgroup judgments and broad intergroup relationships in naturalistic settings. To this end, we assessed valenced contact effects and negativity biases across the intergroup contact literature and tested—through a new meta-analysis—moderation by self-selection opportunity and motivation.

Choice of Meta-Analytical Tools for Their General and Unique Qualities

General features of meta-analyses make them attractive research tools. First, meta-analyses are efficient: They take stock of existing data and allow for the rigorous test of theory-driven research questions without requiring new resources to generate new data. They are particularly useful when the literature available for the synthesis is large and expansive as the intergroup contact literature. Second, meta-analyses are effective: By collating together studies from varied settings and populations and using a multiplicity of methods and designs, meta-analyses surpass other research approaches in their inherent ability to control for a multitude of variables or confounds in ways no individual prospective study can ever do. Finally, meta-analyses are powerful: Although they carry some noise in the coding of key variables and in the computation of effect sizes by relying on sometimes poorly reported research, they often counteract that with large samples that are typically not available to any single study.

More importantly, we chose to use meta-analytical tools because of their unique merits, making them superior to primary research for addressing our specific research questions. We reasoned that a meta-analysis of the expansive intergroup contact literature would capture and enjoy large and desirable cross-study variability in self-selection—processes central to all our novel research questions. This variability stems from significant variance in research designs, settings, and procedures used in this area of inquiry to recruit individuals and deploy intergroup contact, which have implications for self-selection.

For example, some contact studies use experimental designs that nullify self-selection through randomization of participants to different contact treatments or interventions. At the same time, a large number of studies use correlational designs and measure natural presentations (or the retrieval) of intergroup contact in everyday settings where self-selection pressures can be significant and variable. In a similar vein, it is desirable that this literature includes a vast range of contact settings: A proportion of these studies investigates contact in institutional and organizational settings where individuals' freedom to opt in and out of contact is significantly restricted. The remainder looks at unstructured, unmonitored, and unsupervised forms of contact away from authorities and normative control. These are places where there should be greater influence of self-selection processes. Studies in this literature also vary in recruitment strategies and methods for contact deployment. The methods used can either limit or allow for actively opt in or opt out of the study and the contact experience.

Altogether, the diversity of procedures and study design within the contact literature captures naturalistic variability in self-selection processes relating to intergroup contact. This diversity and variability increase the incisiveness of our meta-analytic tests of (moderation by) self-selection.

Overview of Research Objectives, Approach, and Predictions

Carrying out meta-analytical tests across the vast intergroup contact literature enabled broad research objectives. We were able to (a) test for contact valence effects and valence asymmetry in data with inbuilt self-selection, thus checking what happens to these effects under conditions that allow individuals to freely self-select in and out of contact. Because of the large variability in research designs, settings, and procedures with implications for self-selection, this literature offers the volume and the scope needed to code studies along these parameters and thus (b) check if self-selection opportunity/affordances and (c) other valenced expectancies about various aspects of the contact experience and broader context, associated with motivation to self-select, moderate valenced contact effects and negativity biases. Finally, exploiting the scope of this novel synthesis, we were able to (d) explore whether additional factors discussed in the contact literature or known to correlate with prejudice influence also valenced contact effects, and negativity bias.

To pursue these research objectives, we searched for studies in the psychological literature that investigated direct, face-to-face intergroup contact and included a measure of its perceived valence so that they could be unequivocally classified along contact valence. To achieve this, we used more restricted eligibility criteria than Pettigrew and Tropp's (2006) original meta-analysis of unvalenced contact (or contact the valence of which is disregarded during analyses). To be included in our meta-analysis, studies needed to pass four critical eligibility criteria: (a) Studies needed to investigate actual face-to-face contact between the participants and members of an outgroup; (b) studies needed to include at least one measure of perceived valenced contact (e.g., a global measure of contact quality, a measure of valenced emotions during contact or valenced appraisals of the outgroup members implicated in contact); (c) studies needed to report statistics for these measures of contact valence that allowed for a neat classification of the (modal) contact experience as either positive, negative, or ambivalent/neutral for the majority of the participants; and (d) studies needed to include at least one generalized judgment of the outgroup as a whole, thus allowing the quantification of individual-to-group generalizations. Overall, this approach enabled us to test the following predictions meta-analytically.

Valenced Contact Effects. We expected straightforward (meta-analytical) valenced contact effects when checking the averaged contact-prejudice effect separately for studies of positive and negative intergroup contact: We expected positive contact studies to be generally associated with reductions in generalized outgroup prejudice (positive generalizations) and negative contact studies to be generally associated with exacerbations in generalized outgroup prejudice (negative generalizations). We had no a priori expectations for the effects of ambivalent/neutral contact and unclassifiable contact valence.

Overall Negativity Bias. Based on sampling models of impression formation and Paolini et al. (2010), we expected an overall negativity bias or negative valence asymmetry across the literature reflecting larger individual-to-group generalizations following negative contact than positive contact. This effect should emerge because self-selection underpinning many of the studies in this literature would provide the grounds to detect the impact of differential resampling of outgroup experiences following valenced contact, with implications for complexity (vs. simplicity) of impressions and extremity (vs. moderation) of outgroup judgments.

Moderation by Opportunity/Affordances for Self-Selection. Drawing from sampling models, we tested whether environmental affordances or opportunity for self-selection moderates valenced contact effects and the negativity bias. We expected negativity biases to be larger under conditions in which self-selection affordances are broader, relative to conditions that restrict or erase self-selection. This is because when self-selection is possible, individuals should differentially re-sample experiences with outgroup members in light of their valence, aiming to increase positive/safe experiences and minimize negative/unsafe experiences.

Moderation by Motivation to Self-Select. Extrapolating from sampling models, we expected valenced contact effects and negativity bias to be moderated by valenced expectancies around contact. Hence, we expected these effects to be more pronounced in studies with prejudiced samples, about contact with negatively valenced outgroups, with nonintimate partners, and in conflict settings, than in studies that capture the other side of these constructs. These moderation patterns should be present because individuals are less likely to resample experiences with the outgroup when they are prejudiced, see the outgroup negatively, contact involves nonintimate individuals, and takes place in a conflict setting. This limited resampling should restrict the evidence base for impressions; primitive impressions should ultimately lead to polarized outgroup judgments.

Moderation by Correlates of Prejudice and Background Factors. We coded for proxies of sample gender, age, education, outgroup and stigma type, geography, and country-level culture. This allowed us to explore the influence of parameters that are recommended for inclusion in all meta-analyses (American Psychological Association, 2010; Appelbaum et al., 2018; see also Cooper, 2010) that have been included in previous synthesis of contact or are well-known correlates of prejudice. These ancillary analyses ascertained whether these variables (a) covary in any meaningful way with our focal design factor (contact valence), (b) moderate the size of valenced individual-to-group generalizations, and (c) valence asymmetries. We had no preexisting expectations for these factors beyond perhaps anticipating that factors traditionally associated with higher prejudice (e.g., male gender, low education, older age) might display larger negative contact effects and negativity bias, hence suggesting that these effects explain their association with prejudice.

With its focus on full variations in the valence of intergroup contact (positive/negative) and the influence of self-selection processes (opportunity/motivation to opt in/out of contact), this meta-analysis offers researchers, practitioners, and policy makers a significantly more nuanced understanding of contact's varied presentations, effects, and mechanisms. This work spans naturalistic settings characterized by significant variations in valence and structuring/monitoring/sanctioning, and thus opportunities and motivation for self-selection. Hence, this synthesis provides a rich knowledge base to maximize the benefits of intergroup contact for broad intergroup dynamics and minimize its risks for detrimental effects across varied settings.

Method

Studies Contributing to the Synthesis and Search Strategies

One hundred ninety-one valenced contact studies (238 independent samples/tests; 936 nested tests; $N = 152{,}985$ participants) were included in our meta-analyses as eligible for stringent tests of valenced contact and negativity bias. They were extracted from 156 discrete data sources, including 135 research articles published between 1958 and 2020 (Kelly et al., 1958 vs. Bagci et al., 2020; Graf, Paolini, & Rubin, 2020), 10 PhD and master's dissertations, five articles that were either unpublished, under review, or in preparation, and six unpublished data sets. We provide an overview of study characteristics at the beginning of the Results section.

Three strategies were used to search for prospective studies to undergo eligibility screening. First, we examined Pettigrew and Tropp's (2006) meta-analysis reference list as our pool of potentially eligible studies for research published between 1940 and December 2000. With its 713 independent samples from 526 studies, Pettigrew and Tropp's meta-analysis is regarded as a comprehensive synthesis of the intergroup contact research in the 20th century (Paolini et al., 2021). As the present meta-analysis delves into valenced contact, our eligibility criteria (see below) are more conservative than those used by Pettigrew and Tropp. Thus, we can be reasonably confident that no eligible study for our synthesis was missed for the 1940–2000 period, using Pettigrew and Tropp's studies as our pool of potential studies for that period. Second, fresh systematic searches of the APA PsycInfo database were undertaken to identify potentially eligible studies post-2000; these searches encompassed the period between January 2001 and March 2020. The APA PsycInfo database incorporates research from 2,281 journals with a focus on behavioral and social sciences (American Psychological Association, 2020). Searches were undertaken using a large set of search terms and their linguistic variants (e.g., contact, interaction, experience), linked with inclusive logical operands, to scope for research reporting on valenced direct contact and generalized outgroup judgments in the abstract. The complete list of search terms and algorithm is available in Open Science Framework (OSF) supplemental documents (Appendix A; all appendices can be found at https://osf.io/38rpj/). These searches collectively identified 535 studies published between 2001 and 2020; of these, 185 were deemed eligible and included in our meta-analysis. Studies that failed to meet our eligibility criteria or were not available in English were excluded. Third, we undertook a search for unpublished research. We asked 24 professional societies and research networks to circulate our call for unpublished research that met our specific criteria (the complete list appears in OSF Appendix B, https://osf.io/38rpj/). Additionally, we made direct contact with the authors of eligible studies published since 2010 to solicit any unpublished studies in their possession; the timeframe for this search was set to ensure the researcher was still active and contactable. Via this approach, we collated an additional 40 independent effects from 25 eligible studies that were unpublished at that point in time to contribute to our synthesis. Our search for unpublished data was concluded in July 2021.

Eligibility Screening

To determine whether the located studies had the potential to qualify as eligible for inclusion, we examined each abstract to ensure that the article was empirical in nature rather than a review or an opinion piece. Studies were excluded if they were not written in English and required payment for access (e.g., preroutine digitalization microfilms) by well-resourced libraries of three large, highly research-focused, and internationally facing institutions.

All potentially relevant studies needed to meet four inclusion criteria to contribute to the present synthesis, following the process described in the flowchart in online Supplemental Figure S1. First, studies needed to report on direct or face-to-face contact between members of distinct social groups. Studies that reported exclusively indirect contact (e.g., media, imagined, vicarious, or extended contact) were excluded. The eligibility criteria did not stipulate any restrictions regarding setting, social groups, or participants. Second, studies needed to include at least one measure of perceived valence of the contact experience to enable a classification of the effect sizes along the contact valence factor as either "positive," "negative," "ambivalent/neutral," or "unclassifiable." Studies that reported only unvalenced contact frequency/quantity (e.g., Islam & Hewstone, 1993) were excluded. Eligible measures could tap valenced contact or emotions (positive/negative); they could be either evaluations of one or more outgroup members involved in the contact experience, of experiences with these individuals or an indicator of overall valence of the contact experience(s) or contact program. The measure of contact valence needed to be reported in sufficient detail to enable classification of the measure as "unipolar," "bipolar," or "mixed." Eligible unipolar measures used a scale that measures contact negativity or positivity in the shape of a valenced emotion (e.g., anxiety or happiness; Kauff et al., 2017), overall contact valence (e.g., likeable or dislikeable overall contact; Hillman & Stricker, 1996), overall negative or positive contact quantity (Green & Stoneman, 1989), one or more of Allport's conditions for optimal (or positive) contact (Surace & Seeman, 1967), or the number of intergroup foes or friends (Bullock, 1978). Critically, to class as unipolar, these indicators' scales had to capture the absence/presence of contact positivity or negativity (e.g., *very little/none/not at all* vs. *a lot/all/very much*). Eligible bipolar measures spanned between contact positivity and contact negativity; responses are provided on a scale where one extreme represents positive contact and the opposite extreme represents negative contact (e.g., good–bad intervention; Chou & Mak, 1998). Mixed measures would contain a combination of unipolar and bipolar scales, thus providing a murkier indication of valenced contact.

All eligible effects (independent and nested) were classified along contact valence as either "positive," "negative," "ambivalent/neutral," or "unclassifiable" based on eligible measures of contact valence with 89.17% interrater agreement. Unipolar scales provide a direct and unambiguous classification of contact based on whether the measure assessed positivity or negativity, irrespective of the specific rating provided. This is because, for example, a measure of frequency of positive contact (Bagci et al., 2020) would indicate positive contact irrespective of how large the frequency is rated to be. Therefore, this class of indicators does not require additional descriptive statistics to establish the modal valence of the contact experience for most participants. Additional descriptive statistics were used to classify effects along contact valence in the case of bipolar and mixed valence measures. Contact was coded as ambivalent/neutral based on bipolar and mixed scales when central tendency information (i.e., mean, mode, or median) for contact valence was rated as being less than one standard deviation from either side of the scale midpoint; contact was classed as positive or negative if the central tendency information fell outside one *SD* around the midpoint of the scale. The valence of contact was categorized as unclassifiable when bipolar or mixed scales were used, and insufficient central tendency and dispersion statistics were reported to enable unequivocal study classification along contact valence.

Third, as we were interested in the impact of contact on broad intergroup relations or generalized outgroup judgments rather than more circumscribed impressions of the contact partners (cf. some of the data in Pettigrew & Tropp, 2006), studies needed to include a measure of outgroup impressions to be eligible for inclusion. Eligible outgroup judgments asked participants to rate either the outgroup as a whole, a typical or hypothetical outgroup member, or an outgroup member that was uninvolved in the target contact experience. Eligible outgroup judgments could assess attitudes, prejudiced beliefs, emotions, behavioral intentions, or general orientations around diversity or a combination of these. Finally, to be eligible, statistics for the relationship between contact valence and outgroup judgment had to be reported. A combination of specific statistical indicators allowed for the computation of effect sizes. These included zero-order correlations; unstandardized *b* coefficients with standard error or standardized β coefficients; mean with standard deviation; means with *t* test; independent *t* test; *p* value of *t* test; *F* test ($k = 2$ or $k > 2$). Statistics for these relationships were extracted with 80.43% interrater agreement.

Each study was screened for eligibility by this article's second, third, and fourth author or two psychology research students; difficult cases were discussed, and decisions were resolved with the help of the first author. The full list of eligible studies included in this meta-analysis is provided in the Reference list (see entries with asterisks).

Coding of Studies and Reliability Checks

A structured coding protocol (online Supplemental Material S2) was developed by the first, second, and last author and two senior colleagues for the purpose of this meta-analysis to ensure reliable coding of data relevant to our research questions; this included operational definitions for all the constructs of interest, their levels, and clear instructions for coding. The protocol was based on similar instruments employed in published meta-analyses in related areas (McIntyre et al., 2016; Paolini & McIntyre, 2019; Pettigrew & Tropp, 2006). It was developed in accordance with the iterative methods described by Krippendorff and Bock (2009) and piloted 2 times on a subset of studies for inclusion in early stages of this investigation. The final version of the coding protocol extracted data relevant to 73 variables for each eligible sample. A set of 11 articles and 14 studies, published between 1971 and 2018, and using varied methodologies and seven coders, trained to the coding protocol in small groups but coding independently, contributed to establishing interrater reliability. Interrater reliability between coders was consistently good (agreement ranging between 80% and 100%); interrater coefficients are shown in Table 1. Having established satisfactory interrater reliability, each eligible study was independently coded by one of the five researchers engaged in the screening process or one of six research assistants of varied age and ethnic backgrounds trained to code the studies. The coders were trained by the first and last author, following the principles outlined by Lipsey and Wilson (2001). This process started with training in the use and understanding of the coding protocol. Next, each coder coded a small set of studies, and the results were compared and discussed to resolve any inconsistency. This process was repeated multiple times until satisfactory convergence in coding interpretation was reached to ensure clarity and consistency of decision and coding.

Extracted Variables

For the purpose of our main analyses, the eligible studies were coded using our coding manual (online Supplemental Material S2) along three classes of variables: (a) focal variables, (b) key moderators, and (c) ancillary moderators. The codes (raw and calculated) for all variables extracted and recoded for analyses can be found in OSF Appendix F2 (https://osf.io/38rpj/).

The focal variables were contact valence (independent variable or *IV*) and outgroup prejudice (dependent variable or *DV*). These two variables were relevant to calculate effect sizes to test for an overall (unvalenced) contact effect à la Pettigrew and Tropp, valenced contact effects, an overall negativity bias in impact, and moderation tests: More information in the Eligibility Screening section above and the Computation of Effect Sizes and the Statistical Approach sections below). Details about indicators for key and ancillary

Table 1
Indicators of All Moderators Included in the Main Analyses

Indicator	Operationalization	Variable at data extraction	Recoded for analysis[a]	Agree[b]
1. Self-selection indicators				
Able to opt out of data collection	Whether participants were able to refuse to participate in the study data collection based on the structured and formalized nature of the procedure and setting.	[v21][c] 1 = could opt out or cannot be established (181, 76%); 2 = could opt out but difficult to do so or could not opt out (57, 24%)[e]	[v21a][d] 0 = not able to opt out/no self-selection [2]; 1 = able to opt out/self-selection [1].	100%
Use of concealment	Whether concealment and what degree of concealment was used limiting participants' awareness of the intergroup and/or interactive nature of the study and thus reducing their ability to actively refuse opting in.	[v9] 1 = study presented as intergroup and including interaction (3, 1%); 2 = study presented as intergroup (3, 1%); 3 = study presented as including interaction (0, 0%); 4 = participants not told about intergroup/interaction or cannot be established (5, 2%); 5 = naturalistic not experimental study (218, 92%); −99 = no reporting of information provided to participant (9, 4%).	[v9aR] 0 = concealment/no self-selection [3, 4, −99]; 1 = no concealment/self-selection [1, 2, 5].	97.50
Study design	Specifies the design used by the study with implications for individuals' ability to actively opt in or opt out of the contact experience.	[v17] 1 = cross-sectional correlational (204, 86%); 2 = longitudinal (16, 7%); 3 = experimental/pre–post one group (0, 0%); 4 = experimental/treatment control (1, 0.04%); 5 = experimental/pre–post treatment control (5, 2%); 6 = experimental: nonrandomized treatment/control posttreatment only (2 groups)(2, 0.08%); 7 = quasi-experiment/nonrandomized control treatment (10, 4%).	[v17aR] 0 = experimental, controlled/no self-selection [2, 3, 4, 5, 6, 7]; 1 = correlational/self-selection [1].	92.50
Having to actively opt in to contact	Whether the participants needed to actively opt in contact/intervention to take part based on the nature of the recruitment process and structured nature of the setting.	[v18] 1 = needed to actively opt in (e.g., respond to a flyer; 108, 45%); 2 = was automatically/default opted in (e.g., classroom with ethnic teacher; 130, 55%).	[v18aR] 0 = *defaulted in/no self-selection* [2]; 1 = *actively opted in/self-selection* [1].	92.50
Having to actively opt in to data collection	Whether participants needed to actively opt in to data collection to provide data.	[v20] 1 = needed to actively opt in (e.g., respond to a flyer; 120, 50%); 2 = automatically/default opted in (e.g., classroom survey administration; 118, 50%).	[v20aR] 0 = *default opted in/no self-selection* [2]; 1 = *needed to actively opt in/self-selection* [1].	100
Participant incentive	Whether participants received any incentive and type with implications for willingness to opt in.	[v7] 1 = course credit (26, 11%); 2 = money (22, 9%); 3 = raffle entry (12, 5%); 4 = organizational incentive (1, 04%); 5 = no incentive given (27, 11%); −99 = cannot establish (148, 62%).	[v7aR] 0 = *no incentive/no self-selection* [3, 5, −99]; 1 = *incentive/self-selection* [1, 2, 4].	100
Sampling procedure	Type of sampling procedure used with implications for ability to actively opting in or opting out.	[v10] 1 = structured sampling method, includes random, stratified, and so forth (69, 29%); 2 = convenience sampling or unspecified (169, 71%).	[v10aR] 0 = *structured/no self-selection* [1]; 1 = *convenience/self-selection* [2].	100
Able to opt out of contact	Whether the individuals could opt out of contact based on informality, intimacy, unstructured nature of the contact experience.	[v19] 1 = could opt out (e.g., casual contact with a stranger; 188, 79%); 2 = could opt out but difficult (e.g., contact with relative; 23, 10%); 3 = could not opt out (27, 11%).	[v19aR] 0 = *could not opt out/no self-selection* [2, 3]; 1 = *could opt out/self-selection* [1].	97.50
2. Valenced expectations indicators				
Sample prejudice	Intergroup attitudes of the sample based on scores (or expected scores by authors/coder) on relevant individual difference measures (e.g., prejudice, intolerant ideologies, social dominance orientation, right wing authoritarianism, high religiosity) or roles associated with right-wing or liberal orientations (e.g., policeman, human right lawyer).	[v8] 1 = prejudiced sample (78, 33%); 2 = unprejudiced/egalitarian sample (80, 34%); 3 = prejudiced dimension as moderator (e.g., a sample of policemen vs. no policemen; 19, 8%); 4 = liberal dimension as a moderator (e.g., a sample of both republican and democratic voters; 0, 0%); −99 = nonclassifiable sample/dimension not considered (61,26).	[v8a] 0 = *prejudiced* [1, 3 (prejudiced), 4 (nonliberal)]; 1 = *neutral* [−99]; 2 = *nonprejudiced* [2, 3 (nonprejudiced), 4 (liberal)].	100

(*table continues*)

Table 1 (*continued*)

Indicator	Operationalization	Variable at data extraction	Recoded for analysis[a]	Agree[b]
Outgroup valence	Valence of the outgroup within the wider social context as indicated by the authors or inferred by the coder.	[v33] 1 = positive (e.g., a group that is admired in the social context and/or is attributed positive characteristics; 18, 8%); 2 = negative (e.g., a group that is stigmatized in the social context and/or is attributed negative characteristics; 160, 67%); 3 = mixed/neutral/cannot be established (e.g., when positive and negative groups are both present in the study and are aggregated together in the reporting; 60, 25).	[33a] 0 = *positive* [1]; 1 = *neutral* [3] 2 = *negative* [2].	97.56
Outgroup status	Status of the outgroup, relative to that of the participants/ingroup, in the broader context as indicated by the authors or inferred by the coder.	[v36] 1 = the outgroup status is lower than that of the participants/ingroup (158, 66%); 2 = the outgroup status is higher than that of the participant/ingroup (30, 13%); 3 = outgroup has same status as participants/ingroup (36, 15%); 4 = majority and minority and respective stats kept separate (7, 3%); 5 = minority and majority and stats collapsed (7, 3%).	[v36c] 0 = *minority/low status* [1, 4 (minority)]; 1 = *same/similar status* [3, 5]; 2 = *majority/high status* [2, 4 (majority)].	97.56
Outgroup SES	Identifies the socioeconomic status and/or access to resources/power of the outgroup relative to that of the participants/ingroup based on information on occupation, income, or employment status. Check authors' framing or best infer.	[v37] 1 = lower: the outgroup SES status is lower than the participants/ingroup (e.g., participants are from general population and target is mental health patients; 91, 38%); 2 = higher: the outgroup SES is higher than the participants/ingroup (e.g., participants are from general population and outgroup is doctors; 19, 8%); 3 = same or cannot establish (e.g., participants are psychology students and outgroup is business students; 128, 54%).	[37c] 0 = *lower SES* [1]; 1 = *same/similar SES* [3]; 2 = *higher SES* [2].	92.68
Conflict setting	Levels of conflict between the groups involved in the study in the context of broader society.	[v24] 1 = conflict between groups (e.g., Bosnia during the war)(47, 20%); 2 = postconflict society (e.g., contemporary Northern Ireland, Cyprus; 48, 20%); 3 = peaceful setting (143, 50%).	[v24a] 0 = *conflict* [1]; 1 = *postconflict* [2]; 2 = *peaceful* [3].	95
3. Intimacy indicators				
Number of contact partners in contact	Number of contact partners in the contact experience with implications on the intimacy of the experience.	[v38] 1 = one contact partner (8, 3%); 2 = multiple contact partners or unspecified (31, 13%); 3 = group as a whole (199, 84%).	[v38a] 0 = *nonintimate/large number* [3]; 1 = *intimate/small number* [1, 2].	100
Number of contact partners in measure	Number of contact partners in contact valence measure with implications on the intimacy of the experience surveyed.	[v39] 1 = one contact partner (6, 2.5%); 2 = multiple contact partners (6, 25%); 3 = group as a whole (226, 95%).	[v39a] 0 = *nonintimate/large number* [3]; 1 = *intimate/small number* [1, 2].	95.00
Intimacy of contact partner in measure	Intimacy between the self and the contact partner as assessed in the contact valence measure.	[v40] 1 = close relationship (e.g., friend, family member; 37, 16%); 2 = acquaintance (e.g., neighbor, work colleague; 16, 7%); 3 = unfamiliar individuals (12, 5%); 4 = mix or not specified (173, 73%).	[v40a] 0 = *nonintimate* [3, 4]; 1 = *intimate* [1, 2].	100
Contact length	Length of the contact experience with implications for closeness between the self and the contact partner.	[v22] 1 = less than 1 day (16, 7%); 2 = between 2 and 7 days (4, 1%); 3 = between 1 week and 1 month (2, 1%); 4 = 2–11 months (11, 5%); 5 = 1 or more years (1, 0.1%); 6 = naturalistic with intimate/familiar other (50, 21%); 7 = naturalistic with generic other or mixed intimacy (152, 64%); −99 = unable to establish (2, 1%).	[v22c] 0 = *nonintimate* [1, 2, 7, −99]; 1 = *intimate* [3, 4, 5, 6].	86.96

(*table continues*)

Table 1 (*continued*)

Indicator	Operationalization	Variable at data extraction	Recoded for analysis[a]	Agree[b]
Contact location	Formality and structured nature of the location where contact took place.	[v25] 1 = lab (6, 2.5%); 2 = recreational (0, 0%); 3 = work/organizational; 4 = educational (20, 8%); 5 = residential (0, 0%); 6 = tourism (1%); 7 = private/household (1, 0.04%); 8 = naturalistic variation, mixed or cannot be established (207, 87%).	[v25x] 0 = *formal/nonintimate* [1, 3, 4]; 1 = *informal/intimate* [2, 5, 6, 7, 8].	100
Location of data collection	Formality and structured nature of the location where data collection was carried out.	[v26] 1 = lab (41, 17%); 2 = classroom (42, 18%); 3 = other educational setting (9, 4%); 4 = work/organizational (5, 2%); 5 = public place (4, 2%); 6 = private (6, 22%); 8 = mixed, online, cannot be established (115, 48).	[v26x] 0 = *formal/nonintimate* [1, 2, 3, 4]; 1 = *informal/intimate* [5, 6, 7, 8].	81.75
4. MARS and ancillary indicators				
Gender	Percentage of female participants who took part in study.	[v4] % of females in the sample (continuous)	[v4agender] 0 = *male-dominated (50% and below);* 1 = *female-dominated (above 50%).*	85.00
Age	Age category the central tendency for age falls in across the whole sample or across all groups in the study.	[v2] 1 = children (up to 12 years; 9.4%); 2 = adolescents (13–19 years; 29, 12%); 3 = college/university students (81, 34%); 4 = young adults (20–30; 24, 10%); 5 = adults (30–60 years; 90, 38%); 6 = elderly (60+ years; 2, 1%).	[v2aage] 0 = *younger* [1, 2, 3, 4]; 1 = *older* [5, 6].	90.00
Education	Highest level of education undertaken or completed by the majority of the participants.	[v6] 1 = primary education (14, 6%); 2 = secondary/high school (58, 24%); 3 = undergraduate (95, 40%); 4 = postgraduate (1, 0.04%); −99 = unclassifiable/information not available (66, 28%).	[v6deduc] 0 = *primary* [1, −99]; 1 = *secondary* [2]; 2 = *university* [3, 4].	80.00
Outgroup type	Records the target outgroup along which the prejudice is assessed.	[v50] target outgroup involved (open-ended text).	[v50b] 0 = *ethnicity/nationality/religion*; 1 = *gender/sexuality*; 2 = (old) *age*; 3 = *other* (e.g., student majors, nurses, obese people, children with disability, HIV-infected).	95.00
Stigma type	Records whether the intergroup distinction is visible/nonconcealable or invisible/concealable.	[v50] target outgroup involved (open-ended text).	[v50c] 0 = *visible/nonconcealable* [ethnicity, nationality, gender, age]; 1 = invisible/concealable [sexuality, mental health, religion, HIV virus, students major, nurses]	100
Geographical area	Country where the contact study was conducted classified according to geographical area.	[v23] opened end response recording country of data collection.	[v23b] 0 = *Europe;* 1 = *North America* [Canada, United States]; 2 = *other [countries in Asia/Oceania and Africa]*	97.50
Country-level culture	Country where the contact study was conducted classified according to culture based on (Načinović Braje et al., 2019; see also Hofstede, 1980).	[v23] opened end response recording country of data collection.	[v23a] 0 = *individualistic country*; 1 = *collectivist country.*	100
Publication year	Year of publication.	[vyear2b] publication year (continuous).	[vyear2c] 0 = *old* (equal to or before median year of 2015); 1 = *recent* (after median year of 2015).	100

Note. Italicized labels identify moderators' levels used for analyses. SES = socioeconomic status; MARS = meta-analysis reporting standards.
[a] This field reports how each variable was recoded for analysis. Values outside brackets indicate values used for analyses; values inside brackets indicate values of the raw variable as on previous column that contributed to each recoded value. [b] This field reports interraters' reliability expressed in terms of percentage of agreement. [c] Indicates variable name as for the coding manual as in the online appendices. [d] Indicates variable number as for datasheet during analyses. [e] This field reports descriptives for raw variables as number of observations (k) and percentages of the 238 independent/purest samples in main analyses.

moderators are organized in tabulated form in Table 1, including indicators' operational definitions and levels for the raw indicators and for the recoded indicators used for analyses, descriptives, and interrater agreement. Indicators that had insufficient spread to allow for moderation tests in a design inclusive of contact valence were excluded from analyses and reporting. Because the main constructs had several parallel indicators, we grouped together the results for these indicators in tables and the main text.

The key moderators are indicators relevant to test for moderation by (a) opportunity/affordances for self-selection and (b) motivation for self-selection—as associated with valenced expectations about intergroup contact and broad intergroup dynamics. We had eight indicators for self-selection. These included procedural, design, and setting parameters with implications for individuals' affordances/opportunity to self-select in and out of intergroup contact, including having to actively opt in if wishing (having to actively opt in to contact, having to actively opt in to data collection), being able to opt out if not wishing to take part (able to opt out of data collection, use of concealment, able to opt out of contact, participant incentive), or a combination of the two (study design, sampling procedure). When appropriate and practically viable for coding, we kept track of the phase of the study (contact/intervention vs. data collection) self-selection referred to (e.g., able to opt out of contact vs. able to opt out of data collection; full details appear in Table 1's Panel 1). We had five indicators tapping valenced expectations about various aspects of the contact experience and broader dynamics with implications for individuals' motivation or willingness to opt in and out of intergroup contact. They captured qualities of the participants (sample prejudice), the outgroup (outgroup valence, outgroup status, and outgroup SES), and the broader intergroup setting (conflict setting); see full details in Table 1's Panel 2. Six study parameters had clear implications for the key moderator intimacy and had a sufficient spread of observations for inclusion in our moderation analyses. Four indices mapped onto closeness features of intimacy; they included the number of contact partners in the experience, the number of contact partners in the contact measure, intimacy of the contact partner, and contact length. Two of the intimacy indices mapped onto informality features of intimacy; they included contact location and data collection location; full details appear in Table 1's Panel 3.

Last, in the class of ancillary moderators, we included theoretically relevant variables investigated in past moderation tests of unvalenced contact effects (Pettigrew & Tropp, 2006), established correlates of outgroup prejudice, and variables required for best practice in meta-analyses (American Psychological Association, 2010; Appelbaum et al., 2018). These included sample characteristics (gender, age, education, geography), bases for stigmatization (outgroup type, stigma type), cultural differences (country-level culture), and year of publication (publication year); see Table 1's Panel 4. OSF Appendix F2 (https://osf.io/38rpj/) reports all eligible studies included in this meta-analysis together with relevant codes (raw and recoded) on all variables extracted for this synthesis.

Computation of Effect Sizes

Our meta-analyses were performed with Comprehensive Meta-Analysis (CMA, Version 3.0; Borenstein et al., 2011) and with R Version 4.1.2 (R Core Team, 2021) of the package metafor Version 3.8-1 (Viechtbauer, 2010). Most data of interest were expressed in primary sources as correlation coefficients (*r*s), describing the relationship between valenced contact (positive vs. negative; independent variable) and outgroup evaluations (positive vs. negative; dependent variable). For longitudinal studies, (cross-sectional) zero-order correlations were extracted from one randomly selected data wave to maximize consistency with the remainder of the data. In the few cases when zero-order correlations were not available, Lipsey and Wilson's (2001) calculator was utilized to convert available statistics into *r*s. In a few instances of studies reporting multiple regression analyses or path analyses and omitting zero-order correlations, partial standardized β and unstandardized *b* values were extracted as proxies of Pearson's *r*s. The effect sizes used in the analyses and in the reporting were expressed as Fisher's *z* and denoted as *z*(*r*)'s in text, tables, and figures. To increase direct comparability with Pettigrew and Tropp's (2006) meta-analysis, overall effects and averaged effects for moderation by contact valence were also expressed (in text) in Pearson's *r*s.[1] The effect sizes were coded in the direction used in Pettigrew and Tropp (2006): Positive values indicated that contact was associated with more prejudice (a negative/worsened impression); negative values indicated that contact was associated with less prejudice (a more positive/improved impression). In addition to the reporting of Fisher's *z*, *Z*-values were also reported for inferential tests (denoted as *Z*s).

For our primary analyses, one independent effect size was extracted from each eligible sample, as required by traditional meta-analysis methods (Borenstein et al., 2009). To ensure that our meta-analytic findings drew from the best data available, carrying minimal noise with regards to testing for contact valence effects, the "purest" effect size was identified and extracted from within the set of all available (nested) effect sizes for samples with multiple eligible valenced contact-prejudice relationships. These decisions were made using a theory-driven ranking tool, which prioritized neater contact valence indicators (e.g., unipolar) over murkier ones (e.g., mixed) and outgroup judgments that provided direct evaluations of the outgroup (e.g., warmth), away from cognitive elaborations. This ranking tool can be found in online Supplemental Material S3. A composite purity ranking score was calculated for each eligible effect size based on the purity of the contact valence and outgroup judgment measures; the purest effect size was chosen for inclusion. When the options were equivalent on the purity ranking score, any effect size was selected at random.[2] Secondary analyses used a multilevel meta-analytical approach; these used all

[1] It is useful to keep in mind that, while Fisher's *z* has values larger than ±1.00 as Pearson's correlations approach +1.00 and −1.00, for smaller values between −.50 and +.50, the two statistics' values are very similar (Borenstein et al., 2009). Thus, most of the (small and moderate) effect sizes reported in this article as *z*(*r*) values can be readily interpreted as equivalent to *r*s.

[2] When multiple contact-prejudice pairings of similar purity were available and at least one negative contact pairing was available, an effect size for negative contact was (randomly) selected to ensure sufficient representation of negative contact studies in our final sample for a literature recognized for a positivity bias (Graf & Paolini, 2016; Pettigrew & Tropp, 2006). On 50 occasions (out of 238 independent samples; 21%), a negative contact pairing was selected over other valences of equal parity with this method. For our ancillary analyses with nested effects of varied purity, all eligible contact-prejudice pairings were included in the analyses. This second data set returned very similar results to the independent/purest effects data set, hence demonstrating that this approach to ensure sufficient representation of negative contact in our primary analyses did not unduly pollute our meta-analytical tests of valence asymmetry.

(nested) eligible effect sizes of varied purity that could be extracted from all eligible samples. A total of 238 independent/purest and 936 nested/varied purity effect sizes entered our primary/secondary analyses.

Statistical Approach

In all analyses, a random-effects analysis approach was used. In our primary analyses, using exclusively the independent/purest effect sizes, weighted mean effect sizes were calculated for each sample, which allow for samples with larger sample sizes to have a greater weight within the analysis; data were modeled using CMA Version 3 (Borenstein et al., 2011). Tests of heterogeneity of effects used the Method of Moments approach, also known as the DerSimonian–Laird (DL) estimator. In our secondary analyses, carried out as robustness tests with all nested/varied purity effect sizes, a multilevel random-effects model was fit using Restricted Maximum Likelihood estimation to address nonindependence of effects within three hierarchical levels (reports, studies nested within reports, and samples nested within studies). The multilevel analysis was carried out using R 4.1.2 (R Core Team, 2021) with the package metafor Version 3.8-1 (Viechtbauer, 2010).

We used a mixed-effects metaregression analysis for tests of moderation; the primary (father) model had a single moderator, contact valence (positive vs. negative). Two variable moderator models, including contact valence, were run to examine the effect of key and ancillary moderators using both main effect only and full factorial models with the purpose of determining if the additional variables moderated the basic valenced contact effect. The additional variables included self-selection indicators, valenced expectation indicators, intimacy indicators, and ancillary indicators. For models inclusive of moderator variables with three or more levels, the Q statistic was used to assess significance. Simple effects for our two factor models were obtained from single-factor models of one variable at the time for subsets of the data where the other factor was kept constant at a chosen level. Overall, these tests afforded stringent tests of moderation of valenced contact effects and valence asymmetries.

Transparency and Openness

This review project's hypotheses, methods, and analyses were not preregistered; they received approval from the first author's ethics board (H-2016-0381). We adhered to the meta-analysis reporting standards' guidelines for meta-analytic reporting (Appelbaum et al., 2018). All meta-analytic data (raw and recoded to enter this synthesis' analyses), research materials, and coding scheme are available as OSF Supplemental Documents at https://osf.io/38rpj/; core materials are available as online Supplemental Materials. Data were modeled using CMA (Version 3; Borenstein et al., 2011), R Version 4.1.2 (R Core Team, 2021), with the package metafor Version 3.8-1 (Viechtbauer, 2010). CMA uses an interactive platform that does not save codes; all R codes are included in OSF Appendices J1 and J2 (https://osf.io/38rpj/).

Results and Discussion

Study Characteristics

Several background variables for sample, study, and contact characteristics were extracted and are summarized in Table 2. With regards to research design, as anticipated, most eligible samples were drawn from cross-sectional correlational designs, with the remaining (one seventh) using experimental or longitudinal designs. This variability in research design and good representation of correlational designs confirm that these meta-analysis data are significantly infused by healthy variations in self-selection. Regarding sample age and education, the data were mostly from samples of young adults and adults, with smaller contingents of children, adolescents, and older adult samples. A large proportion of participants had or were undertaking an undergraduate degree or attended/had completed secondary school; other levels of education were less represented (primary and postgraduate). The meta-analysis data were relatively gender-balanced.

Table 2
Descriptive Statistics Along Sample and Study/Contact Background Variables Contributing to Ancillary Moderation Analyses

Design parameter/level	*k*	%
Research design		
Cross-sectional	201	85.17
Experimental	19	8.050
Longitudinal	16	6.78
Sample age		
Children	9	3.81
Adolescents	29	12.29
Young adults	103	43.64
Adults	90	38.14
Older adults	3	1.27
Sample education		
Primary	14	5.96
Secondary	58	24.58
Undergraduate	96	40.68
Postgraduate	2	0.85
Sample gender		
Female-dominated	144	61
Male-dominated	92	39
Contact location		
Varied naturalistic settings	205	86.86
School/educational setting	20	8.47
Research laboratory	6	2.54
Work/organizational	3	1.27
Recreational/tourism	1	0.42
Residential	1	0.42
Geographical location		
Europe	140	59.32
North America	76	32.20
Asia/Oceania	16	6.78
Africa	2	1.69
Conflict setting		
Peaceful society	140	59.32
Postconflict society	51	21.61
Conflict society	45	19.07
Intergroup setting		
Ethnicity/nationality	160	67.80
Religion	25	10.59
Gender/sexuality	20	8.47
Age	15	6.36
Other social categories	14	5.35

Design parameter/level	*M*	*SD*	Min	Max
Publication year	2008.9	14.14	1958	2021

Note. Descriptives are calculated against a total k of 238 independent samples/tests and 152,985 participants. Min = minimum; Max = maximum.

Turning to contact characteristics, most contact interactions investigated took place across a variety of naturalistic contact settings, followed by schools and educational settings (other locations were less investigated: research laboratories, work/organizational, recreational/tourism, and residential). The studies reported data from 36 countries and four continents; these overrepresented Europe and North America over all other locations (Asia/Oceania and Africa). The majority of the samples came from peaceful societies; conflict and postconflict societies were similarly but more rarely represented. The studies varied in the intergroup setting of interest. As expected, the largest proportion of studies focused on contact across ethnicity and national divides, followed by religion, gender and sexuality, age, and other intergroup categories.

We checked and reported below covariations between these background variables/ancillary moderators and our focal predictor (contact valence) to ascertain predictors' independence prior to moderation analysis.

Overview of Main Analyses

This fresh attempt at synthesizing early and contemporary research on intergroup contact, focusing on valenced face-to-face contact or direct contact that can be precisely characterized as falling somewhere along the positive-negative valence spectrum, will ascertain the extent to which contact valence acts as a boundary condition to the beneficial effects of intergroup contact and moderates the size of the contact-prejudice link in ways consistent with a negativity bias. In addition, we will check whether self-selection opportunity and motivation, as well as established correlates of prejudice and background factors, further modulate these patterns. To this end, results reporting are organized into five sections:

> First, we replicate the analyses in Pettigrew and Tropp's (2006) meta-analysis with our data, determining the average effect of intergroup contact on outgroup judgments across eligible studies for this meta-analysis *disregarding contact valence,* to examine what we label an overall *un*valenced contact effect. Second, we meta-analytically test whether contact valence moderates this unvalenced generalized effect. Third, we check for the existence of an overall negativity bias or negative valence asymmetry in contact effects by comparing the magnitude of individual-to-group generalizations for negative versus positive contact. We carry out all these tests twice, once with all independent/purest effect sizes (k = 238) and once with all nested/varied purity effect sizes (k = 936). Fourth, we assess moderation by self-selection affordances and motivation (in terms of valenced expectations, and intimacy) to test hypotheses derived from sequential models of impression formation. Fifth, we explore moderation by other theoretically relevant variables in the contact literature and ancillary moderators.

Overall Unvalenced Contact Effect

Our analyses started with assessing the overall effect of intergroup contact on outgroup judgments. For this, the contact-prejudice effects were coded and treated in the same way as in Pettigrew and Tropp's (2006) meta-analysis: Positively signed effects indicated more outgroup prejudice with increased contact (a detrimental effect); negatively signed effects indicated less outgroup prejudice with increased contact (a beneficial effect). However, by looking at both positive and negative intergroup contact, this research departs significantly from Pettigrew and Tropp and earlier syntheses, which, by virtue of their time in history, oversampled prejudice reduction studies and failed to capitalize on more recent studies of negative contact (Paolini et al., 2021). Because of our critically distinct pool of studies (reflective of a sharper focus on valence and pure evaluative contact-prejudice relations and the significantly greater variance in contact valence), we expected our overall (unvalenced) contact effect testing the link between contact and prejudice the way Pettigrew and Tropp did to be markedly different from theirs and to reflect a more balanced aggregation of prejudice reduction effects of positive contact and prejudice exacerbation effects of negative contact, and thus lead to more muted conclusions about the general benefits of intergroup contact.

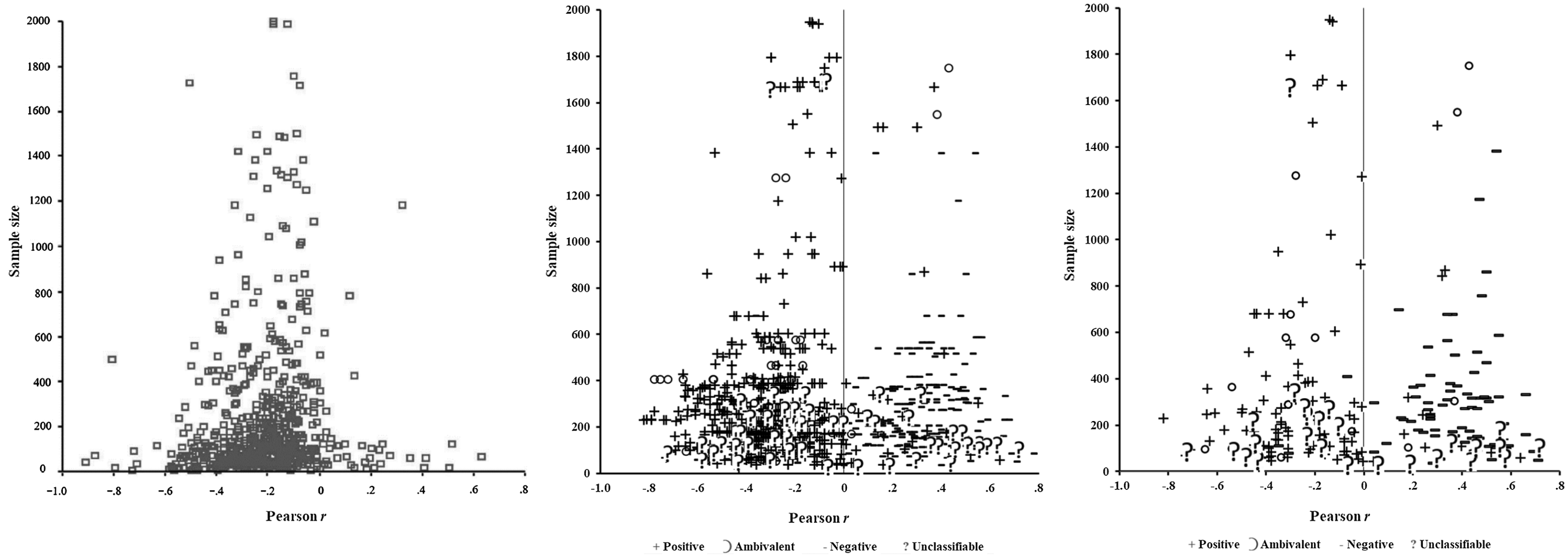
The approach we took enables a direct comparison between the results of Pettigrew and Tropp's meta-analysis and those of the present synthesis. Figure 1 compares the distributions of effect sizes in Pettigrew and Tropp's meta-analysis (left panel) with those from our synthesis of all nested/varied purity effects (middle panel) and of all independent/purest effects (right panel). The funnel plots for this synthesis identify whether our effect sizes stemmed from positive, ambivalent, negative contact, or contact unclassifiable along contact valence (see +, −, o, and ? notation); this information was not available in Pettigrew and Tropp.

Inspection of Distributions

The three distributions are noticeably different in density, shape, and averaged effect size. Pettigrew and Tropp (2006) reported an overall effect of $r = -.21$ in their meta-analysis of contact of unspecified valence that is much larger than our overall unvalenced effect for independent/purest effects (right panel) and still larger than our overall effect for nested/varied purity effects (right panel; cf. vertical lines for respective *M*s). We interpret these important differences as reflecting key differences in (a) inclusion criteria, (b) purity of operationalizations, and (c) research trends. Pettigrew and Tropp's meta-analysis drew from a larger number of studies because it used a less restrictive set of inclusion criteria for both the independent variable (i.e., unvalenced contact) and the dependent variable (both contact-specific and generalized prejudice) and aggregated across pure and murkier evaluations of the contact-prejudice relation. The present synthesis includes data from more recent investigations, post-Pettigrew and Tropp's cutoff date (December 2000), which are richer in negative intergroup contact effects. As a result, especially our distribution of independent/purest effect sizes (Figure 1's right panel), which aimed at balancing the number of positive and negative contact effects toward stringent and balanced valence asymmetry tests, includes more positively signed effects and a more uniform spread across positively and negatively signed effects, indicative of the larger number of investigations on the detrimental impact of negative intergroup contact in the more recent literature (negative contact, 30%, k = 71; positive contact, 49%, k = 117). Our distribution of nested effects, which, like Pettigrew and Tropp's, does not discriminate between high and low quality operationalizations and aggregates across all effects is also more skewed, like Pettigrew and Tropp's original, to overrepresent positive contact (negative contact, 28%, k = 263; positive contact: 60%, k = 564) and, as a result, returns an overall unvalenced contact effect that is closer in size and direction to Pettigrew and Tropp's original mean effect. We formalized these observations with the meta-analysis tests in the following sections.

Figure 1

Funnel Plots of Contact-Prejudice Effects in Pettigrew and Tropp (2006) and This Research's Data Sets



Note. Funnel plots of contact-prejudice effects as a function of sample size, as per Pettigrew and Tropp's original plot, where positively signed effects indicate more outgroup prejudice with increased contact (a detrimental effect); negatively signed effects indicate less outgroup prejudice with increased contact (a beneficial effect). The plot on the left panel displays the distribution from Pettigrew and Tropp's (2006) meta-analysis of unvalenced contact effects; the plot in the middle panel displays the distribution of nested/purity varied effect sizes from secondary analyses of the present meta-analysis of valenced contact effects, and the plot on the right panel displays the distribution of independent/purest effect sizes from the primary analyses of this meta-analysis. The plots for this meta-analysis also indicate how each effect size was classified along contact valence. Effect sizes classified as positive contact are identified by "+" signs; effect sizes classified as ambivalent/neutral contact are identified by "o" signs; studies classified as negative contact are identified by "–" signs; and effect sizes classified as unclassifiable along contact valence are identified by "?" signs. Vertical lines superimposed on each plot identify overall (averaged) effect size. Effects from samples with an N greater than 2,000 participants are omitted to ease visual comparison. The studies relevant to this meta-analysis that were omitted from display are marked with an asterisk in the meta-analysis reference list in OSF Appendix D (https://osf.io/38rpj/). The plots for this meta-analysis in the middle and right-hand panels are also available plotted as a function of *SE*s (rather than Pettigrew and Tropp's original sample size here) in the OSF Supplemental Documents at https://osf.io/38rpj/. *SE* = standard error; OSF = Open Science Framework.

Independent/Purest Effects

A random-effects metaregression conducted with all 238 independent/purest effects found a nonsignificant, close-to-zero, negatively signed relationship between contact and outgroup judgments, $r = -.0028$, $z(r) = -.0028$, $Z = -.13$, $p = .8962$, 95% $z(r)$ CIs [−.04, .04], $\tau = 0.40$, $I^2 = 93.6$, $k = 238$ (see Figure 1 right panel's vertical unbroken line). These results indicate that, when collating data from studies of varied and known contact valence, with an emphasis on highest quality operationalizations for the contact-prejudice relationship (purest effects), but disregarding contact valence during analyses (unvalenced contact), our results are very different from Pettigrew and Tropp's: Intergroup contact is neither beneficial nor detrimental; it neither improves nor worsens generalized outgroup prejudice. The overall benefits of unvalenced contact for outgroup judgments based on independent/purest effect sizes are therefore negligible in size.

Nested/Varied Purity Effects

We provided a conceptual replication of these analyses with all 936 nonindependent effect sizes of varied purity. We modeled a three-level intercept-only multilevel meta-analysis, accounting for the random effect of articles [$\tau^2 = 0.0120$, $\tau = 0.11$, levels = 169], studies nested within articles [$\tau^2 = 0.0414$, $\tau = 0.20$, levels = 197], and samples nested within studies [$\tau^2 = 0.0041$, $\tau = 0.06$, levels = 229], $I^2 = 98.4$. The Akaike information criterions (AICs) confirmed that the three-level hierarchical structure provides the best fit to the data, AIC(2) = 27,964.47; AIC(3) = 27,567.65; AIC(4) = 27,509.71, respectively; this hierarchical structure was thus used in all our multilevel meta-analyses. The multilevel model returned a significant, negatively signed relationship between unvalenced contact and outgroup prejudice, $r = -.16$, $z(r) = -.16$, $SE = .018$, $Z = -8.68$, $p < .0001$, 95% $z(r)$ CIs [−.12, −.19], $k = 936$. This averaged effect size is displayed by the vertical, unbroken line on Figure 1's middle panel. These results indicate that, when collating indiscriminately all data from varied quality operationalizations (pure and nonpure effects), and again disregarding contact valence during analyses, the results get closer to Pettigrew and Tropp's original results: Intergroup contact is typically associated with small-size reductions in generalized prejudice.

Importantly, our analyses also revealed that these overall unvalenced contact effects carried significant degrees of heterogeneity, independent/purest effects, $Q(235) = 14{,}390.32$, $p < .0001$, $I^2 = 98.4$; nested/varied purity effects, $Q(935) = 41{,}497.18$, $p < .0001$, $I^2 = 96.3$; forest plots are in OSF Appendices H1 and H2 (https://osf.io/38rpj/). Hence, a significant portion of variance within our effect size data is nonrandom[3] and must reflect the impact of moderators. Together, the difference in overall unvalenced contact effects between independent/purest and nested/varied purity effects and evidence of heterogeneity point towards nuanced patterns in the data that require further investigation.

Moderation by Contact Valence and Valenced Contact Effects

Next, we looked at the intergroup contact-prejudice relationships coded again *a la Pettigrew and Tropp*, but this time through the lenses of our focal independent variable (contact valence), once again separately for independent/purest and nested/varied purity effect sizes. In both sets of analyses, contact valence significantly moderated the overall unvalenced contact effect, independent/purest, $Q(3) = 356.07$, $p < .0001$, $\tau = 0.29$, $I^2 = 97.93$; nested/varied purity, $Q(3) = 20{,}297.72$, $p < .0001$; articles [$\tau^2 = 0.0076$, $\tau = 0.087$, levels = 169]; studies nested within articles [$\tau^2 = 0.0395$, $\tau = 0.1987$, levels = 197], and samples nested within studies [$\tau^2 = 0.0053$, $\tau = 0.0725$, levels = 229]; $I^2 = 95.86$. Therefore, the contact-prejudice link was significantly affected by the valence of the contact experience.

Metaregression analyses were undertaken to further explore these moderations; the results are summarized in Figures 2 and 3 and the top and middle panels of Table 3. With both independent/purest and nested/varied purity effect sizes, we found meaningful valenced contact effects on outgroup prejudice: Negative intergroup contact was associated with significant increases in prejudice—detrimental individual-to-group generalizations. Positive and ambivalent contact were associated with significant reductions in prejudice—beneficial individual-to-group generalizations. These effects were not statistically different in size along the independent/purest effects ($Z = -0.65$, $p = .52$; all pair-wise comparisons were significant for the more powered nested/varied purity effects). The effect of unclassifiable contact is difficult to interpret theoretically and was empirically inconsistent between data sets. All the pair-wise comparisons, including negative contact, were significant, $ps < .0001$; yet, their meaning is conflated by opposite-signed effects. Hence, we carried out extra data manipulations to achieve neater valence asymmetry tests.

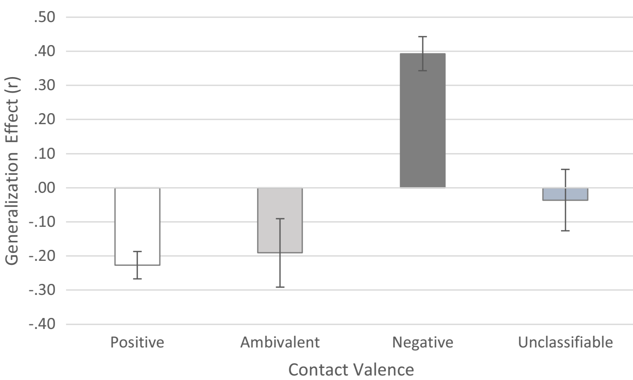
Overall Negativity Bias or Negative Valence Asymmetry in Impact

Both Paolini et al.'s (2010) and Denrell's (2005) models hypothesize the existence of an overall negative valence asymmetry

[3] Traditional publication bias tests, like the fail-safe *N* test and the Tweedie–Duval trim-and-fill method, have been conventional for many years, but do not perform well under conditions of high between-studies heterogeneity. Under these circumstances, regression-based bias adjustment methods or some form of Vevea–Hedges selection model are typically more appropriate (see Harrer et al., 2021, Chapter 9; Johnson & Hennessy, 2019). Encouraged by a reviewer, we endeavored to replace the results of traditional publication bias tests with this newer generation tests. Yet, we discovered that these might also be inappropriate for this meta-analysis. As indicated by the I^2, our models with $k = 238$ independent/purest effects had an $I^2 = 97.9\%$ for the overall unvalenced effect and an $I^2 = 98.4\%$ for the model including contact valence; our model with $k = 936$ nested/varied purity effects had an $I^2 = 95.86\%$ for the overall unvalenced effect and an $I^2 = 96.3\%$ for the model including contact valence. This large between-studies variance is very obvious in the associated forest plots in OSF Appendices H1 and H2 (https://osf.io/38rpj). As Harrer et al. (2021) explained, under these circumstances "publication bias analyses of meta-analyses with very high heterogeneity should at best be avoided altogether" (end of section 9.3; see also Johnson & Hennessy, 2019, pp. 15–16, but with notes of caution regarding using the size of I^2 as diagnostic due to its relative and sometimes biased nature). Efforts should instead be directed to identify theory-driven moderators capable of explaining such between-studies heterogeneity (see also Johnson, 2021). This approach is exactly what we used in the sections that follow, where we draw on sequential models of impression formation to identify moderators of valenced contact effects that, in conjunction with contact valence, can explain some of such heterogeneity. For readers still interested in traditional publication bias tests, we refer to the OSF Supplemental Documents at https://osf.io/38rpj (appendix M) for results and notes of caution in interpretation.

Figure 2

Averaged Valenced Contact Effects Drawing From All Independent/Purest Effect Sizes (k = 238) and Expressed With Pettigrew and Tropp's Scoring Method



Note. Evidence that contact valence moderates the contact-prejudice relationship. (Error bars indicate 95% CIs.) Effects are expressed as a function of contact valence. Positively signed effects indicate more outgroup prejudice with increased contact (a detrimental effect); negatively signed effects indicate less outgroup prejudice with increased contact (a beneficial effect). CI = confidence interval. See the online article for the color version of this figure.

in impact, reflecting significantly larger worsening of outgroup prejudice following negative intergroup contact than attenuations of outgroup prejudice following positive intergroup contact. Hence, incisive tests of intergroup valence asymmetry require comparisons between the magnitudes of valence-congruent individual-to-group generalizations associated with intergroup contact of differing valence. The contact-prejudice effect sizes *à la Pettigrew and Tropp* thus require screening for valence-incongruent individual-to-group generalizations (and if needed, recoding).

Valence-incongruent individual-to-group generalizations are examples of contrast, rebound, or boomerang effects (Kunda & Oleson, 1995, 1997). Contrast effects have been discussed and documented in attitude and stereotype change research (McIntyre et al., 2016; Mussweiler, 2003), but to our best knowledge, they are disregarded in the contact literature (however, see Birtel & Crisp, 2012). This blind spot most likely reflects both a late theoretical appreciation that intergroup contact has a direction of change "embedded" in its valence and a delay in empirically coding simultaneously for the valence of contact and the outgroup judgment, something that we do in this research. Our sharp interest in valenced contact toward stringent tests of valence asymmetry led us to first check for the existence and appraise the prevalence of contrast effects amidst eligible intergroup contact studies; this appraisal could not be achieved by Pettigrew and Tropp's meta-analysis of unvalenced contact.

Seventeen of the 238 independent/purest effect sizes (7.2%) in our data set displayed valence-incongruent relationships (some significant and some nonsignificant) between the valence of the contact experience and the outgroup judgment. Fourteen of the independent/purest positive contact effect sizes (12%) produced a positively signed effect (rs range = .04/.64; $M = .31$, $Mdn = .29$), thus unexpectedly revealing prejudice exacerbation (see effects denoted with "+" in the right panel of Figure 1, falling in the positively signed section of the funnel plot). Three of the (independent/purest) negative contact effect sizes (4%) produced a negatively signed effect (rs range = −.07/−.35; $M = -.24$, $Mdn = -.31$), thus displaying prejudice attenuation (see effects denoted with "−" in Figure 1's right panel falling in the negatively signed section of the funnel plot). These effect sizes needed recoding for neat tests of valence asymmetry. Of the 19 ambivalent contact effect sizes, we found that 15 produced an effect indicative of prejudice reduction and four produced an effect indicative of prejudice exacerbation (see effects denoted with "o" in the right panel of Figure 1). As these effects are evaluatively consistent with at least some valenced features of the ambivalent contact experience (Zingora et al., 2021), we treated them in our recoding (see in the following section) as valence congruent. Finally, of the 31 unclassifiable contact valence samples, 18 displayed prejudice reduction and 12 displayed prejudice exacerbation (see effects denoted with "?" in the right panel of Figure 1). The interpretation of valence unclassified effects is difficult: The lack of clarity over the valence of the contact prevents us to discerning whether these effects are valence congruent or incongruent. As such valence unclassifiable samples "pollute" neat tests of valence asymmetry; for this reason, they were excluded from our follow-up analyses centered around contact valence (for a similar approach, see Paolini & McIntyre, 2019).

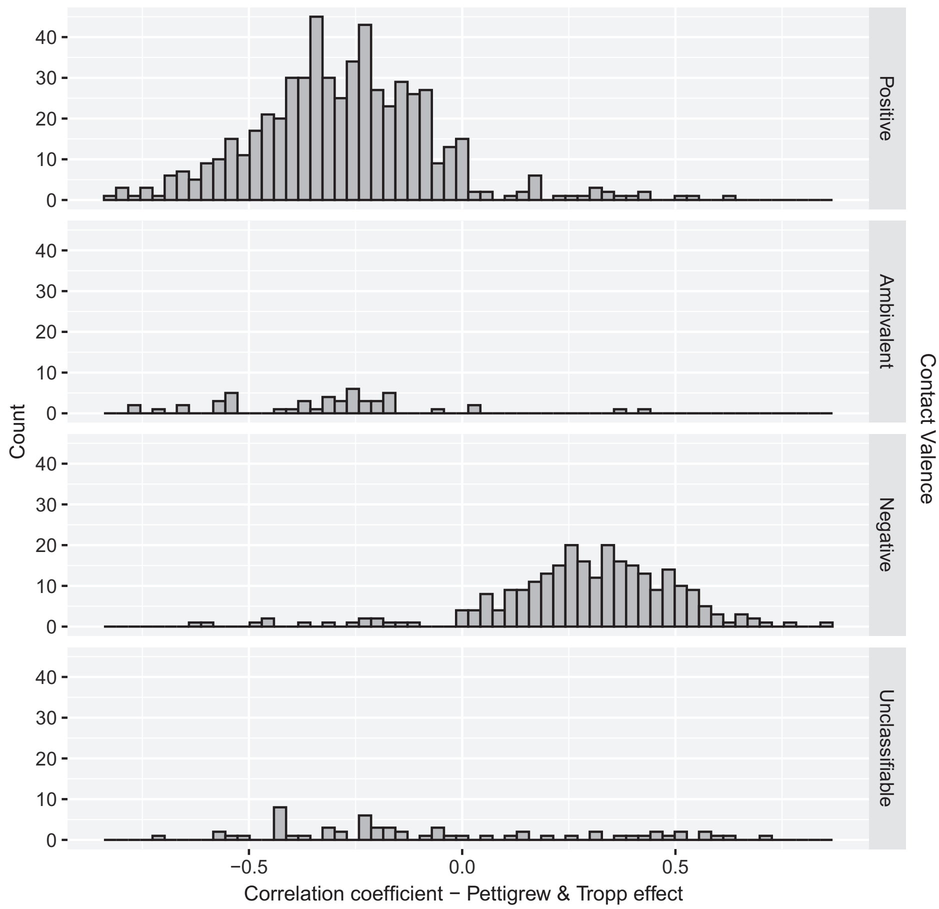
Following the method of Paolini and McIntyre's meta-analysis, which also tested intergroup valence asymmetries but in experimental data, we recoded the signs of the contact-prejudice effects à la Pettigrew and Tropp so that a positive effect always indicates a valence-congruent generalization, that is, changes in outgroup evaluations in the direction of the valenced contact experience with the outgroup members (i.e., an assimilation effect; e.g., outgroup attenuations following positive contact, prejudice exacerbation following negative contact) and a negative effect always indicates a valence-incongruent generalization, that is a change in outgroup evaluations in an opposite direction of the valenced contact experience with the outgroup members (i.e., a contrast effect; e.g., prejudice exacerbation following positive contact, prejudice reduction following positive contact). Considering this recoding along the line of valence-congruent effects, the larger the effect size, the larger the relationship between intergroup contact and outgroup prejudice in absolute terms in directions congruent with the contact valence. So, for example, if contact was positive in nature, the larger the effect size (and the taller the bars in our upcoming graphs) the larger the improvements in outgroup judgments or positive generalizations. If contact was negative in nature, the larger the effect size (and the taller the bars in the graphs), the larger the worsening in outgroup judgments or the negative generalizations. With the data recoded in this fashion to reflect valence-congruent effects and the unclassifiable contact valence effects removed from the analyses, we were left with 207 effect sizes in the independent/purest analyses and with 875 effect sizes in the nested/varied purity analyses.

Independent/Purest Effects

The average valence-congruent individual-to-group generalization in our pool of independent/purest effect sizes was significant and positively signed, $r = .29$, $z(r) = .30$, $Z = 21.23$, $p < .0001$, 95% $z(r)$ CIs [.27, .33]. The overall effect for valence-congruent generalizations was significantly heterogeneous, $Q(208) = 5095.59$,

Figure 3

Distributions of Contact-Prejudice Effects Drawing From All Nested/Varied Purity Effects S (k = 936), Expressed as a Function of Contact Valence and Using Pettigrew and Tropp's (2006) Scoring Method



Note. Positively signed effects indicate more outgroup prejudice with increased contact (a detrimental effect); negatively signed effects indicate less outgroup prejudice with increased contact (a beneficial effect). Evidence that contact valence moderates the contact-prejudice relationship also with the fuller data set.

$p < .0001$, $I^2 = 95.94$, and thus once again pointed towards the meaningful influence of moderators. Contact valence significantly moderated the size of these generalizations, $Q(2) = 33.59$, $p < .0001$. All types of valenced contact produced significant individual-to-group generalizations (full statistics in Table 3's bottom panel): As predicted, negative contact was associated with the largest effect size ($r = .37$, $p < .0001$), followed by ambivalent contact ($r = .34$, $p < .0001$), and last by the smallest effect size for positive contact ($r = .23$, $p < .0001$). Pairwise comparisons indicated that ambivalent contact was not statistically different from negative contact, $Z = 0.92$, $p = .3582$, and was associated with significantly larger generalizations than positive contact, $Z = 2.50$, $p = .0125$. This pattern was unexpected but is consistent with findings reported in attitude research experiments (Brauer et al., 2012; see Graf, Paolini, & Rubin, 2020).

More critically for the focus of the present research, as expected based on Paolini et al.'s (2010) model and expectations of an overall negative valence asymmetry in intergroup contact data allowing for self-selection (e.g., Denrell, 2005), negative contact was associated with significantly larger generalization effects than positive contact, $Z = 5.69$, $p < .0001$. These results are displayed in Figure 4; they were substantially replicated by the analyses on nested/varied purity effects.

Nested/Varied Purity Effects

A test of negativity bias was carried out on our pool of all nested/varied purity effect sizes using a post hoc test of differences in mean effects. This approach allowed us to directly compare the magnitude of generalizations across averaged effect sizes that were differently signed along the Pettigrew and Tropp's scoring method. These analyses indicate that negative intergroup contact produces significantly larger generalization effects than their ambivalent counterparts, $z(r)_d = 0.52$, $SE = .02$, $Z = 30.19$ $p < .0001$. More importantly, these analyses confirm that negative contact produces larger generalizations than positive contact, $z(r)_d = 0.69$, $SE = 0.0048$, $Z = 142.18$, $p < .0001$, consistent with the existence of a negativity bias also when

Table 3

Contact-Prejudice Mean Effect Sizes Scored With Pettigrew and Tropp's Method (Top and Middle Panels) and as Evaluatively Congruent Generalizations (Bottom Panel) Across Independent/Purest Effect Sizes (Top/Bottom) and Nested/Varied Purity Effect Sizes (Middle), as a Function of Contact Valence

								Pairwise comparison *p*		
Contact valence	Mean *r*	*z*(*r*)	Z	95% CI	95% PI	*p*	*k*	2	3	4
Independent/purest effects scored per Pettigrew and Tropp										
1. Positive contact	**−.23**	**−.23**	−10.67	[−.27, −.19]	[−.65, .19]	<.0001	117	.5164	<.0001	.0001
2. Ambivalent contact	**−.19**	**−.19**	−3.72	[−.29, −.09]	[−.62, .24]	.0002	19		<.0001	.0244
3. Negative contact	**.39**	**.39**	15.10	[.34, .44]	[−.03, .81]	<.0001	71			<.0001
4. Unclassifiable contact	−.04	−.04	−.80	[−.13, .05]	[−.46, .39]	.4222	31			
Nested/varied purity effects scored per Pettigrew and Tropp										
1. Positive contact	**−.29**	**−.30**	−17.24	[−.26, −.33]	[−.75, .15]	<.0001	564	<.0001	<.0001	.0002
2. Ambivalent contact	**−.14**	**−.14**	−5.91	[−.09, −.18]	[−.59, .32]	<.0001	48		<.0001	<.0001
3. Negative contact	**.37**	**.39**	21.93	[.35, .42]	[−.06, .84]	<.0001	263			<.0001
4. Unclassifiable contact	**−.23**	**−24**	−10.50	[−.19, −.28]	[−.69, .21]	<.0001	61			
Independent/purest effects scored as evaluative congruent generalizations										
1. Positive contact	**.23**	**.23**	−9.32	[.18, .27]	[−.26, .71]	<.0001	117	.0125	<.0001	
2. Ambivalent contact	**.34**	**.35**	5.93	[.23, .46]	[−.15, .85]	<.0001	19		.3582	
3. Negative contact	**.37**	**.39**	13.18	[.33, .45]	[−.10, .88]	<.0001	71			

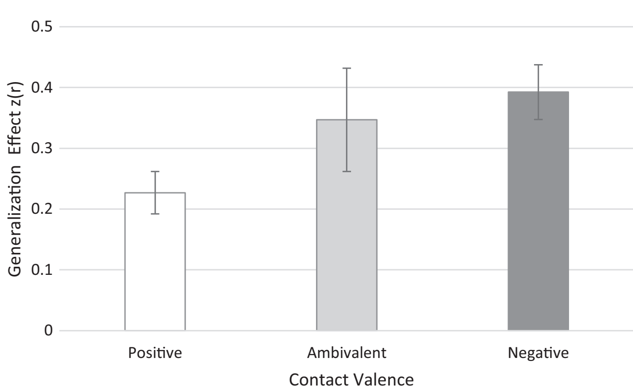
Note. Bolded coefficients are significant at $p < .05$. Coefficients on the right are *p* values for pair-wise comparisons between mean effect sizes. Effects are expressed with Pettigrew and Tropp's scoring method (top and middle panels) and can be positively and negatively signed. Positively signed effects indicate more outgroup prejudice with increased contact (a detrimental effect); negatively signed effects indicate less outgroup prejudice with increased contact (a beneficial effect). Effects expressed in terms of valence-congruent individual-to-group generalizations (bottom panel) can only be positively signed. The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). CI = confidence interval; PI = prediction interval.

looking across the whole pool of purest and less pure operationalizations of the contact-prejudice link.

Across all analyses, we found consistent evidence for the existence of a negativity bias in intergroup contact effects. As predicted by Paolini et al. (2010) and by the novel interplay between intergroup contact theory and sequential models of impression formation (Denrell, 2005), the detrimental effects of negative contact on generalized prejudice are larger than the beneficial effects of positive contact.

Figure 4

Averaged Valenced Contact Effects Drawing From All Independent/Purest Effect Sizes and Expressed in Terms of Valence-Congruent Individual-to-Group Generalizations



Note. Evidence that the magnitude of individual-to-group generalizations associated with intergroup contact is significantly moderated by the contact valence. (Error bars indicate 95% CIs.) The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). CI = confidence interval.

Moderation by Focal Variables

The remainder of the analyses used the independent/purest effect sizes expressed as valence-congruent generalizations. This pool of effects provides a more balanced representation of positive and negative contact effect sizes on which to build more complex moderation analyses, and the specific scoring method allows for direct comparisons of effect sizes across valences. We prepared these data by aggregating ambivalent contact effects with positive contact effects on the grounds that, on average, ambivalent contact was associated with similar prejudice reductions to positive contact (top panel of Table 3) and produced effects that could be reasonably classed as evaluatively congruent. The positive and ambivalent effects together were associated with an average effect very close to that of positive contact effects alone, $r = .24$, $z(r) = .24$, $Z = 14.44$, $p < .0001$, 95% $z(r)$ CIs [.21, .28] (vs. $z(r) = .23$), due to ambivalent contact contributing a small fraction of the new aggregated positive contact set (19 out of 136). Importantly, negative contact effects were still associated with significantly larger generalizations than this enlarged set of positive and ambivalent effects, $Z = 5.26$, $p < .0001$, again confirming the existence of an overall negative valence asymmetry in impact across contact studies.

Our key moderation tests are organized around three classes of factors informed by the novel interfacing between intergroup contact theory and sampling models of impression formation, including (a) opportunity/affordances for self-select, (b) valenced expectations about various aspects of the contact experience as proxies of motivation to self-select, and (c) contact intimacy. The analytical approach employed was identical across all tests; we showcase some indicators to familiarize the reader with our analytical approach. Also, for simplicity of exposure, we provide all results in main text and associated tables and figures for the first class of moderators; for the remainder classes of moderators, we refer the reader to the online Supplemental Tables S4–S9 for the tables of results.

Moderation by Opportunity to Self-Select

We examined the moderating effect of self-selection on the overall negativity bias we detected on the valence-congruent individual-to-group generalization index. Based on sampling models, we expected ability to self-select to be associated with amplified valenced contact effects and negativity bias. We had coded the studies in our pool of eligible data for the synthesis along several design, setting, and procedural parameters with implications for self-selection (see Table 1). Eight of these indicators displayed adequate spread once dichotomized in two-level moderators (no self-selection vs. self-selection) for inclusion in a factorial design with the focal independent variable, contact valence (positive vs. negative).

We used Pearson χ^2 tests to check the convergent validity between self-selection indicators and their relative independence from the contact valence factor. Seventeen tests out of the 28 carried out with the self-selection indicators were significant or marginal, all χ^2s(1) ≥ 2.92, $p \leq .088$, indicating a good and spread-out degree of convergent validity between the (parallel) indicators. Four self-selection indicators significantly correlated with contact valence (able to opt out of data collection, able to opt out of contact, having to actively opt in to contact, having to actively opt in to data collection), all $\chi^2(1) \geq 8.58$, $p \leq .003$. The other four indices were not associated with contact valence, all *p*s > .47; hence, there was some desirable independence between the focal independent variable and many of these moderator indices. Covariations between predictors in our meta-analytical analyses would not be problematic, however, as they are managed well analytically by the statistical program's algorithm in a manner analogous to multiple regression. Participants' ability to opt out of data collection was selected for exemplification. Participants in the no self-selection condition were recruited in studies that severely limited participants' ability (if wishing) to opt out of data collection, for example, because the study was carried out in an institutional or organizational setting (e.g., Naor & Milgram, 1980; Vezzali & Giovannini, 2012). Studies in the self-selection condition used methods that allowed participants (if wishing) to opt out of data collection (e.g., Hewstone et al., 2006; Kotzur & Wagner, 2021).

Our first step consisted of a metaregression analysis carried out, including contact valence (coded 0 = *positive*, 1 = *negative*) and ability to opt out of data collection (0 = *not able to opt out/no self-selection*, 1 = *able to opt out/able to self-select*) as simultaneous predictors in the analyses. This first step detected a significant main effect of contact valence, $z(r) = .13$, $p < .0001$, 95% CIs [.07, .18], while controlling for ability to opt out of data collection. This result is reported under the "main effects/contact valence" field on the left-hand side of Table 4, in the top panel for the moderator index "able to opt out of data collection." The effect sizes of the conditions contributing to this significant main effect of contact valence can be found on the right-hand side of the table. This result indicates that the negativity bias identified earlier across all independent/purest effects in our overall test remained significant while accounting for variations in whether participants could self-select or not out of data collection.

A main effect of ability to opt out of data collection was significant, $z(r) = .10$, $p = .022$, 95% CIs [04, .17], while controlling for contact valence (see "main effects/self-selection" field in Table 4's left-hand side for "able to opt out of data collection" index). This significant effect indicates that individual-to-group generalizations following contact and across contact valences were larger when individuals were able to freely choose to engage as opposed to when data collection was mandated. Interestingly, this meta-analytical result replicates experimental evidence for larger generalizations for voluntary valenced intergroup imagery, as opposed to involuntary valenced imagery (Husnu et al., 2024). It has quite poignant implications as it suggests that the impact of contact experiences with specific individual outgroup members is appreciably larger when individuals have some freedom about engaging or not in the intergroup experience, as compared to when they do not have such freedom.

In a second step, we introduced a multiplicative term to the metaregression model to capture the contact valence by self-selection interaction. This second step returned a significant interaction, $z(r) = .25$, $p = .0049$, 95% CIs [.08, .42], hence, as predicted by sampling models, self-selection significantly moderated the size of valenced contact effects. This interaction effect is reported under the "interaction" field in the middle of Table 4 and displayed in panel A, at the top-left-hand corner of Figure 5.

Next, follow-up analyses provided a formal test of the significant contact valence by self-selection interaction. The effect sizes of the four conditions underpinning the 2 × 2 design and interaction were all statistically significant (see four right-hand columns in Table 4), meaning that positive and negative contact produced significant individual-to-group generalizations at each level of the self-selection factor. The results of the simple effects analysis for the interaction can be found in the top panel of Table 5. The simple effects analysis revealed that the difference in effect size between positive and negative contact was significant under self-selection ($Z = 5.20$, $p < .0001$; see fourth row), but not under no self-selection ($Z = .96$, $p = .34$; see third row). Consistent with a strong version of the hypotheses derived from Denrell's model, these results suggest that the negativity bias was present exclusively under conditions in which participants were able to self-select to contact. Following up the interaction the other way (see first and second rows), we detected a significant difference in effect size between the self-selection and the no self-selection conditions among the negative contact effect sizes ($Z = 3.99$, $p = .0001$), but not among the positive contact studies ($Z = 1.60$, $p = .11$). These results indicate that the contact valence by self-selection interaction was driven by differences in the effects of negative contact as a function of self-selection.

Altogether, these first meta-analytical results about the moderation of valenced contact effects by ability to self-select indicate that self-selection processes amplify negative valence asymmetries in impact, specifically by amplifying the magnitude of valenced generalization effects following negative contact. These results imply that the disproportionately larger impact of negative contact on intergroup responding is more likely to materialize under conditions of unstructured, unmonitored, and unsanctioned contact, where variability in contact valence is also likely more pronounced (see Paolini et al., 2010).

We replicated this analytical approach with the other seven self-selection indicators, as described in detail in Table 1. These results

Table 4
Main Results for the Models Including Contact Valence and Self-Selection Affordances Indicators

				Self-selection			
				No self-selection		Self-selection	
	Main effect			Contact valence			
Moderator index	Contact valence	Self-selection	Interaction	Positive	Negative	Positive	Negative
Able to opt out of data collection							
z(*r*) (*Z*)	**.13** (4.30)	**.10** (3.07)	**.25** (2.81)	**.20 (6.41)**	**.11 (1.41)**	**.26 (12.92)**	**.42 (17.75)**
95% CIs	[.07, .18]	[.04, .17]	[.08, .42]	[.14, .26]	[−.04, .26]	[.22, .30]	[.37, .47]
p (*k*)	<.0001(208)	.0022 (208)	.0049 (208)	<.0001 (41)	.16 (7)	<.0001 (92)	<.001 (68)
Use of concealment							
z(*r*) (*Z*)	**.15** (5.21)	**.12** (2.13)	**.32** (2.49)	**.22** (3.22)	**.07** (.63)	**.25** (14.14)	**.41** (17.87)
95% CIs	[.09, .20]	[01, .24]	[.07, .56]	[.09, .35]	[−.14, .27]	[.21, .28]	[.36, .45]
p (*k*)	<.0001(208)	.0330 (208)	.0127 (208)	.0013 (9)	.53 (4)	<.0001 (124)	<.0001 (71)
Study design							
z(*r*) (*Z*)	**.15** (5.30)	.07 (1.68)	.16 (1.86)	**.24** (4.59)	**.24** (3.86)	**.25** (13.61)	**.41** (17.17)
95% CIs	[.09, .20]	[−.01, .15]	[−.01, .33]	[.12, .37]	[.12, .37]	[.21, .28]	[.37, .46]
p (*k*)	<.0001 (208)	.0934 (208)	.06 (208)	<.0001 (15)	<.0001 (9)	<.0001 (118)	<.0001 (66)
Having to actively opt in to contact							
z(*r*) (*Z*)	**.12** (4.01)	.06 (1.90)	.04 (0.62)	**.23** (11.13)	**.33** (7.56)	**.27** (8.98)	**.41** (15.62)
95% CIs	[.06, .18]	[−.000, .12]	[−.08, .16]	[.19, .27]	[.25, .42]	[.21, .33]	[.36, .47]
p (*k*)	.0001 (208)	.06 (208)	.53 (208)	<.0001 (91)	<.0001 (21)	<.0001 (42)	<.0001(54)
Having to actively opt in to data collection							
z(*r*)/*r* (*Z*)	**.12** (3.81)	**.07** (2.47)	.01 (0.10)	**.22** (9.61)	**.33** (6.28)	**.29** (10.58)	**.41 (16.03)**
95% CIs	[.06, .18]	[.02, .13]	[−.13, .14]	[.17, .26]	[.23, .43]	[.23, .34]	[.36, .46]
p (*k*)	.0001 (208)	.0136 (208)	.92 (208)	<.0001 (81)	<.0001 (15)	<.0001 (52)	<.0001 (60)
Participant incentive							
z(*r*) (*Z*)	**.15** (5.22)	.02 (0.56)	.01 (−0.18)	**.24** (12.93)	**.39** (16.47)	**.27** (6.41)	**.40** (8.11)
95% CIs	[.09, .20]	[−.05, .09]	[−.13, .15]	[.20, .28]	[.34, .44]	[.18, .35]	[.30, .50]
p (*k*)	<.0001 (208)	.58 (208)	.86 (208)	<.0001 (110)	<.0001 (59)	<.0001(23)	<.0001(16)
Sampling procedure							
z(*r*) (*Z*)	**.15** (5.14)	.02(.83)	.06 (.96)	**.21** (7.18)	**.40** (9.85)	**.26** (12.18)	**.39** (13.96)
95% CIs	[.09, .20]	[−.03, .08]	[−.06, .18]	[.16, .27]	[.32, .48]	[.22, .30]	[.33, .44]
p (*k*)	<.0001 (208)	.41 (208)	.34 (208)	<.0001 (42)	<.0001 (23)	<.0001(91)	<.0001 (52)
Able to opt out of contact							
z(*r*) (*Z*)	**.14** (4.81)	.04 (1.11)	.17 (1.86)	**.19** (5.72)	**.49** (6.02)	**.26** (13.13)	**.38** (16.20)
95% CIs	[.08, .20]	[−.03, .11]	[−.01, 36]	[.13, .26]	[.33, .65]	[.22, .30]	[.34, .43]
p (*k*)	<.0001(208)	.27 (208)	.06 (208)	<.0001 (35)	<.0001 (7)	<.0001(98)	<.0001(68)

Note. Coefficients in boldface are significant at $p < .05$. Average effects expressed in terms of valence-congruent individual-to-group generalizations (right-hand side of the table) can only be positively signed. The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). CI = confidence interval.

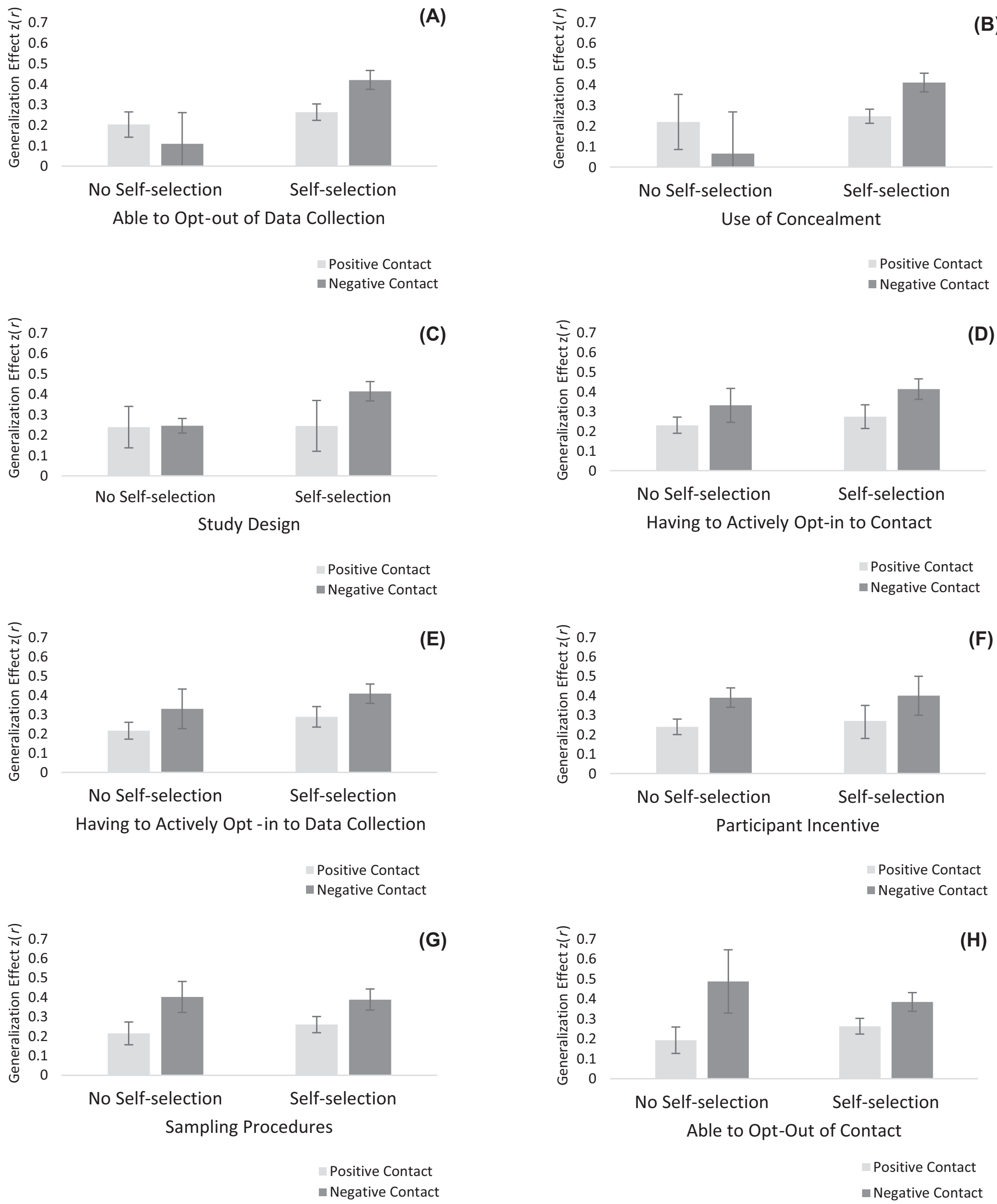
are also summarized in Tables 4 and 5 and displayed in Figure 5 (Panels B–H). All metaregression models, inclusive of the contact valence and self-selection factors, returned a significant main effect of contact valence when accounting for the diverse proxies of self-selection. Hence, the negativity bias detected across all independent/purest effect sizes held significant when controlling for variations on a multiplicity of procedural and design parameters with implications for self-selection, including use of concealment, study design, having to actively opt in to contact, having to actively opt in to data collection, participant incentive, sampling procedure, and able to opt out of contact.

These analyses also returned significant/marginal main effects of self-selection in four of the seven additional models, indicative (as for the earlier indicator) of larger individual-to-group generalizations in the studies where participants were able to self-select (vs. not able to) while controlling for contact valence. Therefore, in most cases, the ability to self-select in or out of intergroup contact amplified the impact of valenced intergroup contact on outgroup judgments. In addition to our example indicator, a significant/marginal interaction between contact valence and self-selection was detected for use of concealment, study design, and able to opt out of contact. When looking at the results of the simple effects (see Table 5), we found that seven tests out of seven displayed a significant negativity bias under self-selection conditions (see last row within each indicator's panel), whereas five tests out of seven displayed a negativity bias in impact in the no self-selection conditions (see third row within each indicator's panel). Looking at the designs the other way, we found that the difference in effect sizes between the selection and the no self-selection conditions was significant or marginal 3 times out of seven under negative contact studies (see second row within each indicator's panel) and 2 times out of seven under positive contact studies (see first row within each indicator's panel). Altogether, these results, with those described earlier for the index showcased, suggest that the interaction pattern implicating contact valence and ability to self-select are driven by both negative and positive contact effects.

Overall, the negativity bias was larger or present exclusively in studies allowing for self-selection than it was in studies not allowing

Figure 5

Graphs for the Interplay of Contact Valence With Each of the Self-Selection Affordances Indicators



Note. Effects are expressed in terms of valence-congruent individual-to-group generalizations: The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). In all graphs, error bars indicate 95% CIs. (A) Panel displays moderation tests for contact valence and able to opt out of data collection. (B) Panel displays moderation tests for contact valence and use of concealment. (C) Panel displays moderation tests for contact valence and study design. (D) Panel displays moderation tests for contact valence and having to actively opt in to contact. (E) Panel displays moderation tests for contact valence and having to actively opt in to data collection. (F) Panel displays moderation tests for contact valence and participant incentive. (G) Panel displays moderation tests for contact valence and sampling procedure. (H) Panel displays moderation tests for contact valence and able to optout of contact. CI = confidence interval.

Table 5
Simple Effects for Models Including Contact Valence and Self-Selection Indicators

Design section	Condition compared	z(r)	SE	95% CI	Z	p
Able to opt out of data collection						
Positive contact	Self-selection—no self-selection	.06	.04	[−.01, .13]	1.60	.11
Negative contact	Self-selection—no self-selection	**.31**	.08	[.16, .46]	3.99	.0001
No self-selection	Negative—positive contact	−.09	.10	[.10, −.29]	0.96	.34
Self-selection	Negative—positive contact	**.16**	.03	[.10, .22]	5.20	<.0001
Use of concealment						
Positive contact	Self-selection—no self-selection	.03	.07	[−.11, .17]	0.39	.69
Negative contact	Self-selection—no self-selection	**.34**	.10	[.14, .54]	3.35	.0008
No self-selection	Negative—positive contact	−.16	.20	[−.55, .23]	−0.82	.41
Self-selection	Negative—positive contact	**.16**	.03	[.11, .22]	5.70	<.0001
Study design						
Positive contact	Self-selection—no self-selection	.001	.06	[−.10, .11]	0.13	.90
Negative contact	Self-selection—no self-selection	**.17**	.07	[.04, .30]	2.50	.01
No self-selection	Negative—positive contact	.0003	.10	[−.19, .10]	0.00	.99
Self-selection	Negative—positive contact	**.17**	.03	[.11, .23]	5.65	<.0001
Having to actively opt in to contact						
Positive contact	Self-selection—no self-selection	.04	.04	[−.03, .12]	1.17	.24
Negative contact	Self-selection—no self-selection	.08	.05	[−.02, .18]	1.63	.10
No self-selection	Negative—positive contact	**.10**	.05	[.01, .19]	2.14	.03
Self-selection	Negative—positive contact	**.14**	.04	[.06, .22]	3.28	.001
Having to actively opt in to data collection						
Positive contact	Self-selection—no self-selection	**.07**	.04	[.003, .14]	2.04	.04
Negative contact	Self-selection—no self-selection	.08	.06	[−.03, .19]	1.38	.17
No self-selection	Negative—positive contact	**.11**	.05	[.01, .22]	2.07	.04
Self-selection	Negative—positive contact	**.12**	.04	[.04, .20]	3.07	.002
Participant incentive						
Positive contact	Self-selection—no self-selection	.02	.05	[−.06, .11]	0.55	.59
Negative contact	Self-selection—no self-selection	.01	.05	[−.10, .12]	0.20	.84
No self-selection	Negative—positive contact	**.15**	.03	[.09, .21]	4.80	<.0001
Self-selection	Negative—positive contact	**.14**	.07	[.01, .27]	2.07	.04
Sampling procedure						
Positive contact	Self-selection—no self-selection	.05	.04	[−.03, .12]	1.22	.22
Negative contact	Self-selection—no self-selection	−.01	.05	[−.11, .08]	0.29	.78
No self-selection	Negative—positive contact	**.19**	.04	[.10, .27]	4.41	<.0001
Self-selection	Negative—positive contact	**.13**	.04	[.05, .21]	3.14	.002
Able to opt out of contact						
Positive contact	Self-selection—no self-selection	.07	.04	[−.01, .15]	1.78	.07
Negative contact	Self-selection—no self-selection	−.10	.08	[−.26, .06]	−1.24	.21
No self-selection	Negative—positive contact	**.30**	.09	[.11, .48]	3.18	.0015
Self-selection	Negative—positive contact	**.12**	.03	[.06, .18]	3.96	.0001

Note. The focal independent variable contact valence was coded as 0 = positive contact, 1 = negative contact. The self-selection indicators were coded as 0 = no self-selection, 1 = self-selection. Coefficients in boldface are significant at $p < .05$. The sign of the $zs(r)$ indices reflects the result of the comparison (i.e., subtraction) between the two conditions implicated in the test (see conditions compared field). *SE* = standard error; CI = confidence interval.

for self-selection, along four indicators (see Figure 5A–5D; *able to opt out of data collection, use of concealment*, *study design,* and *having to actively opt in to contact*). The negative bias was similar in size irrespective of self-selection along two self-selection indicators (see Figure 5E–5F; *having to actively opt in to data collection* and *participant incentive*). In only two instances, the negativity bias was larger in studies not allowing for self-selection than in studies allowing for it (see Figure 5G–5H; *sampling procedure* and *able to opt out of contact*). Hence, ability to self-select moderated the magnitude of valenced contact effects and the negativity bias, most times in ways consistent with Denrell's model.

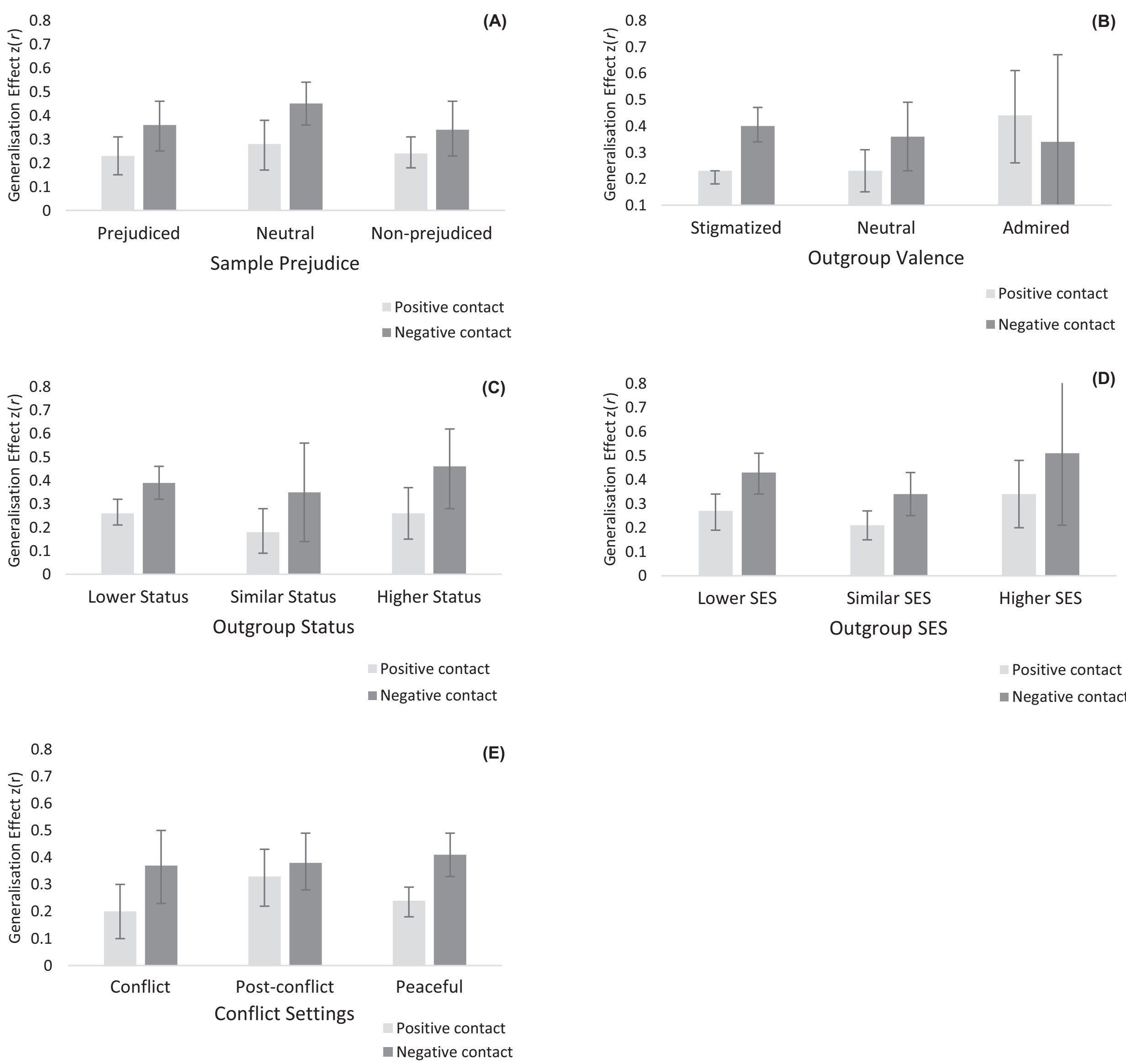
Moderation by Valenced Expectations

This section of analyses tested moderation by motivation/willingness to self-select in and out of contact; the five available indicators (*sample prejudice, outgroup valence, outgroup status, outgroup SES,* and *conflict setting*) were subjected to 2 Contact Valence × 3 Moderator between-subjects factorial designs. We treated these variables as three-level moderators in light of conceptual considerations and preliminary results indicating that this granularity unveiled nuances that were otherwise masked by their two-level counterparts. The analytical and reporting approach used was similar to that in the earlier section: Step 1 included exclusively the main effects, and Step 2 added the interaction term (contact valence by moderator); for increased agility, the detailed tables of results are available in the online Supplemental Materials (see online Supplemental Tables S4–S9). The model statistics for effects involving three-level moderators are expressed as *Q tests*; for consistency of reporting, we extended this test statistics to all main effects. These results are summarized in online Supplemental Table S4. Given the conceptual heterogeneity of this class of moderators, we limited our tests of convergent validity to the three parallel indices of valenced outgroup stereotypes (see below). We used again Pearson χ^2 tests to check for the moderator indices' relative independence from contact valence. All valenced expectancies indices displayed no empirical association with contact valence, all $\chi^2 < 2.02$, $ps \geq .365$.

Based on sampling models, we expected prejudiced samples to display larger negativity biases than nonprejudiced samples due to more pronounced differential resampling as a function of contact valence. Sample prejudice did not significantly moderate the main effect of contact valence (see interaction field at the top of online Supplemental Table S4), yet the simple effects analysis displayed the predicted pattern: The negativity bias was larger and marginally significant for prejudiced samples, $Z = 1.91$, $p = .06$. It was fully significant for neutral attitude samples, $Z = 2.46$, $p = .014$. It was smaller and nonsignificant for nonprejudiced samples, $Z = 1.48$, $p = .14$ (full stats at the top of online Supplemental Table S5). This pattern of effects is displayed in Figure 6A. Hence, samples with

Figure 6

Graphs for the Interplay of Contact Valence With Each of the Valenced Expectation Indicators



Note. Effects are expressed in terms of valence-congruent individual-to-group generalizations: The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). In all graphs, error bars indicate 95% CIs. (A) Panel displays moderation tests for contact valence and sample prejudice. (B) Panel displays moderation tests for contact valence and outgroup valence. (C) Panel displays moderation tests for contact valence and outgroup status. (D) Panel displays moderation tests for contact valence and outgroup SES. (E) Panel displays moderation tests for contact valence and conflict settings. CI = confidence interval; SES = socioeconomic status.

prejudiced and neutral outgroup attitudes displayed a negativity bias, whereas nonprejudiced samples did not.

We had three parallel indicators for valenced outgroup stereotypes (*outgroup valence, outgroup status,* and *outgroup SES*). As expected, these three indicators were significantly associated, all $\chi^2(4)\text{s} \geq 38.68$, $ps < .001$, thus showing signs of convergent validity: Studies focusing on stigmatized outgroups also often looked at outgroups of lower status and lower SES, and vice versa (i.e., admired, higher status, higher SES outgroups). We expected the three indicators to display a similar interactive pattern with contact valence. Based on Denrell's (2005) sequential model as well as explanations of valence asymmetry based on risk aversion (Baumeister et al., 2001; Neuberg et al., 2011), we expected ordinal interactions reflective of larger negativity biases in studies of contact with stigmatized outgroups, low status/minority outgroups, and low SES outgroups and slimmer and possibly nonsignificant negativity biases in studies of contact with admired outgroups, high status/majority outgroups, and high SES outgroups. Yet, in light of Paolini and McIntyre's (2019) meta-analysis, we were also open to disordinal interactions reflective of negative valence asymmetries for studies of contact with stigmatized, low status/minority, and low SES outgroups and positive valence asymmetries (i.e., reversals) for studies of contact with admired, majority/high status, and high SES outgroups, consistent with cognitive (Abelson et al., 1968; Fiedler et al., 2013; Rothbart et al., 1979) and social psychological (Coats et al., 2007; Reynolds et al., 2000; J. C. Turner et al., 1987) explanations for valence asymmetries. We had no a priori expectations about the results for contact with neutral valence outgroups, status, or SES similar to those of the participants/ingroup. The three indicators of valenced outgroup stereotypes displayed two slightly distinct patterns: The results for outgroup valence were indicative of a disordinal interaction (albeit with nonsignificant follow ups for the reversal); the results for outgroup status and SES were indicative of an ordinal interaction (cf. Figure 6B–6D).

The moderation analyses involving outgroup valence detected a marginal main effect of outgroup valence, $Q(2) = 4.99$, $p = .0825$, indicative of a tendency for larger generalizations for admired as opposed to stigmatized or neutral outgroups. The contact valence by outgroup valence interaction was not significant, yet the averaged effect sizes by conditions showed a pattern for negativity bias in studies investigating stigmatized and neutral valence outgroups and a pattern for positive valence asymmetries in the (small number of) studies investigating admired outgroups. The simple effects analyses (online Supplemental Table S5) indicated that the difference in effect size between positive and negative contact studies was significant only for contact with stigmatized outgroups, $Z = 3.88$, $p = .0001$. Predictably, the largest share of studies focused on using contact to revise responses to stigmatized outgroups, and significantly more research is needed to understand the effects of contact with admired and neutral valence groups in society and thus enable a deepening of our understanding of basic processes in intergroup contact (Paolini & McIntyre, 2019).

The results we detected for outgroup status and outgroup SES aligned directly with Denrell's and risk aversion explanations (Figures 6C and 6D). In both sets of analyses, we detected only a significant main effect of contact valence, $Q(1) = 8.11$ and 7.98, both $ps < .005$, while controlling for valenced expectations about the outgroup. The generalizations were always in the direction of a negativity bias at all levels of the outgroup status and SES moderators. These negativity biases were consistently reliable for low status/minority and low SES groups, both $ps < .005$, but also presented for contact with groups of similar SES, $Z = 2.35$, $p = .0189$, and marginally for higher status/majority groups, $Z = 1.89$, $p = .0597$.

Hence, although we detected some minor inconsistency between the results for the three indicators of valenced outgroup stereotypes we had, invariably across indicators, the negativity bias was the largest and statistically most robust in contact with negatively valenced outgroups, irrespective of whether these evaluations drew from their perceived valence, status, or SES.

We coded the eligible contact studies also for whether they were carried out in settings with active intergroup conflict, postconflict, or peaceful societies. The main effect of contact valence was marginally significant when accounting for conflict setting, $Q(1) = 3.61$, $p = .0574$. Although the interaction term was nonsignificant, $p = .21$, simple effects analysis suggested that the quality of the broader intergroup context did matter (see Figure 6E). Negativity biases in generalizations were of statistical relevance in conflict and peaceful societies, $Z = 1.90$, $p = .0574$, and $Z = 3.39$, $p = .0007$, respectively, but not in postconflict settings, $Z < 1$. Unexpectedly, in postconflict settings (e.g., Northern Ireland, Cyprus), positive and negative face-to-face contact had the same capacity to affect group-level responses. Future research should establish what unique qualities make postconflict societies less vulnerable to negativity biases in intergroup contact.

Moderation by Intimacy

We also examined moderation by intimacy/familiarity. Based on sampling models, we expected larger generalizations under nonintimate/unfamiliar (vs. intimate/familiar) contact, reflecting buffering of valenced contact by intimacy (Graf, Paolini, & Rubin, 2020). We also expected intimacy to interact with contact valence in the form of an ordinal fashion indicative of larger negativity bias under nonintimate/unfamiliar contact due to muted valence asymmetries under intimate/familiar conditions in lieu of repeated resampling (Denrell, 2005). Of the six study parameters coded, four mapped onto closeness features of intimacy (number of contact partners in contact, number of contact partners in measure, intimacy of contact partners in contact, intimacy of contact partner in measure, and contact length), and two onto informality features of intimacy (contact location, location of data collection; for full details, see Table 1). All 15 Pearson $\chi 2$ tests between pairs of intimacy indicators were significant/marginal, all $\chi^2(1) \geq 3.83$, $p \leq .050$, confirming widespread convergent validity between all indicators (no obvious tighter clustering of these associations was found separately for closeness and informality indicators). Four intimacy indicators significantly covaried with contact valence (contact length, location of data collection, number of contact partners in contact, intimacy of contact partner in measure), all $\chi^2(1) \geq 8.65$, $p \leq .003$; the other two did not, $ps > .27$.

Number of contact partners in contact was used for exemplification purposes. Studies in the *no intimacy condition* had participants engaged in (nonintimacy-inducing) contact with a large number of outgroup members (whole group; e.g., Abrams et al., 2017); studies in the *intimate condition* had participants engaged in contact with one or a small number of outgroup members (one or multiple group members; e.g., Brown et al., 2007) and thus were expected to experience contact as more personable and intimacy-inducing. We performed a metaregression analysis (see online Supplemental Table S6), including contact valence (coded 0 = *positive*, 1 = *negative*) and number of contact partners (0 = *large number/no intimacy*, 1 = *small number/intimate*) as simultaneous predictors. This first step detected a significant main effect of contact valence, $z(r) = .13$, $p < .0001$, 95% CIs [.07, .19], indicating that the negativity bias identified across all independent/purest effect sizes held significant while accounting for variations in intimacy associated with the number of contact partners involved in the contact experience. As expected, a main effect of intimacy was also significant, $z(r) = .11$, $p = .004$, 95% CIs [.04, −.18]: The contact-prejudice relationship was larger when individuals had nonintimacy-inducing or impersonal contact with a larger number of outgroup members than in intimacy-inducing contact with one or a small number of contact partners. This meta-analytical result replicates the results of earlier meta-analytical evidence for number of outgroup members drawn from experimental tests of individual-to-group generalization in the stereotype change literature (McIntyre et al., 2016). Hence, individuals are readier to generalize from specific outgroup members to the outgroup as a whole when in nonintimate, impersonal contact with many outgroup members.

When introduced a multiplicative term in the metaregression model, this second step returned a marginal interaction, $z(r) = .20$, $p = .05$, 95% CIs [.0004, .40], hence, as predicted by sampling models, intimacy significantly moderated the size of valenced contact effects (see Figure 7A and online Supplemental Tables S6 and S7). The effect sizes of the conditions underpinning the 2 × 2 design and interaction were all statistically significant (all *p*s < .0001), with the exclusion of the negative/intimate condition, most likely due to limited power ($p = .15$; cf. four right-hand columns in online Supplemental Table S6). Simple effects analysis (online Supplemental Table S7) revealed that, as expected, the difference in effect size between positive and negative contact was significant under nonintimate contact ($Z = 4.87$, $p < .0001$) but was not significant under intimate contact ($Z = -.53$, $p = .60$). Consistent with a strong version of the hypotheses derived from Denrell's model, we found that the negativity bias was present exclusively under conditions in which participants had impersonal, nonintimate contact with outgroup members. Following up the interaction the other way, we detected a significant difference between the non-intimate and the intimate conditions among the negative contact studies ($Z = -2.96$, $p = .003$), but only a marginal one among the positive contact studies ($Z = -1.89$, $p = .06$). These results indicate that the contact valence by intimacy interaction was driven by the negative/nonintimate condition.

Altogether, this first set of meta-analytical results for moderation by intimacy indicates that the intimacy of the contact experience mutes valenced contact effects by reducing the magnitude of valenced generalization effects and buffers against negativity biases. This pattern was driven by the nonintimate/negative condition. Based on sampling models, the disproportionately larger impact of negative contact is more likely under conditions of impersonal, nonintimate contact because, under these conditions, individuals are unlikely to resample experiences with outgroup members and thus are unduly influenced by the valence of early contact experiences.

We used the same analytical approach with the other five indicators of intimacy (online Supplemental Tables S6–S7 and Figures 7B–7F). All metaregression models returned a significant main effect of contact valence (all *p*s < .0001); hence, the negativity bias holds across variations in intimacy. These analyses also returned significant/marginal main effects of intimacy 3 times out of five additional models, but the direction of these effects was not homogeneous across indicators: They were in the direction of intimacy "muting" valenced contact effects on other two closeness features (*intimacy of contact partner in measure, contact length*), and as for the showcased indicator, they were in the direction of intimacy "amplifying" valenced contact effects on an informality feature (*contact location*). This dissociation between conceptually different operationalizations of intimacy was also evident in the results of the simple effects (see online Supplemental Table S7; see also Figure 7B–7F). All three extra indices tapping closeness features displayed a significant difference between negative and positive contact (i.e., a negativity bias) under nonintimate conditions (all *p*s < .0005), whereas the two indices tapping informality features did not, *p*s > .27. Along informality (*contact location, location of data collection*), a negativity bias presented under intimate/informal conditions (*p*s < .0001), but never under intimate/closeness proxies, all *p*s > .06. Looking at the designs the other way, the buffering effects of intimacy for closeness indices and the amplifying effects of intimacy for informality indices were driven by a mixture of positive and negative contact conditions: Difference in effect sizes between the intimate and nonintimate conditions were significant/marginal 4 times out of five in negative contact and 3 times out of five in positive contact.

These results, with those showcased earlier, suggest that, in line with early work (Graf, Paolini, & Rubin, 2020; Fuochi et al., 2020), intimacy moderates valenced contact effects. Consistent with sampling models, intimacy muted positive and negative generalizations and the negativity bias along intimacy-as-closeness of the interaction. On the other hand, intimacy amplified valenced effects and bias along intimacy-as-informality of the interaction and its locations; hence, formal contact settings where contact is often mandated through structural roles (Harwood, 2021) might be desirable for broad intergroup relations.

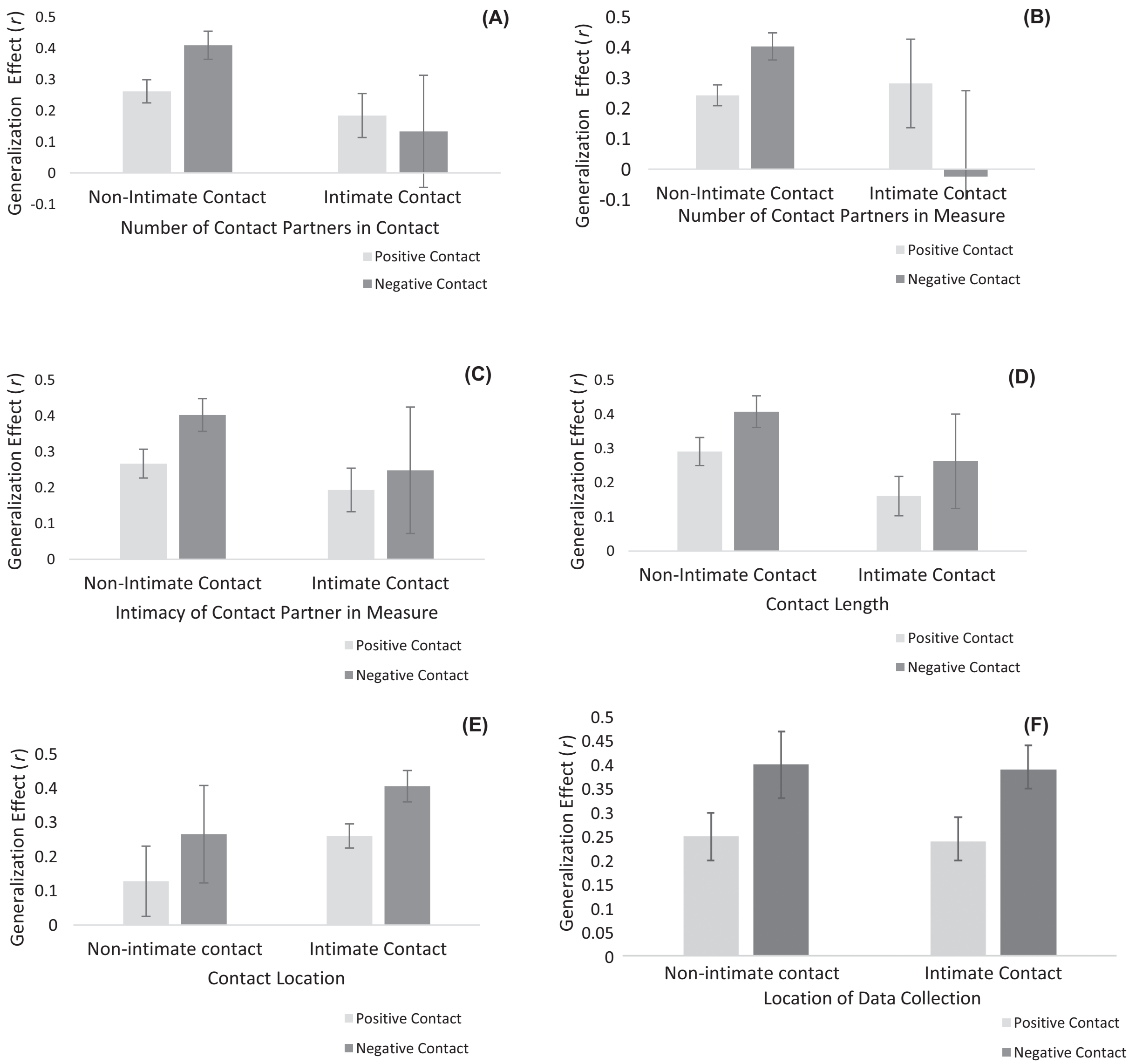
Moderation by Ancillary Variables

Eight ancillary indicators captured variations along theoretically relevant variables investigated in past moderation tests of unvalenced contact effects, established correlates of outgroup prejudice, and sample background variables in best meta-analytic practice (American Psychological Association, 2010; see Table 1. Given the indicators' conceptual heterogeneity, we did not carry out tests of convergent validity. Chi-square tests indicated that most indicators were uncorrelated with contact valence, all $\chi^2 < 1$, *p*s > .36; only significant or marginal effects are commented below in this section. Results of ancillary moderators implicated in 2 × 2 models are in online Supplemental Tables S8 and S9; those implicated in 2 × 3 models are in online Supplemental Tables S4 and S6; all graphs are in Figure 8.

Gender was coded in terms of the proportion of female participants within the overall study sample. On average, eligible studies displayed a slight prevalence of female-dominated samples ($M = .59$; $Mdn = .55$). A 2 (gender) × 2 (contact valence) design found a significant main effect of contact valence and a marginal main effect of gender (online Supplemental Table S8), $Z = 1.80$, $p = .0721$, indicative of a tendency for individual-to-group generalizations to

Figure 7

Graphs for the Interplay of Contact Valence With Each of the Intimacy Indicators



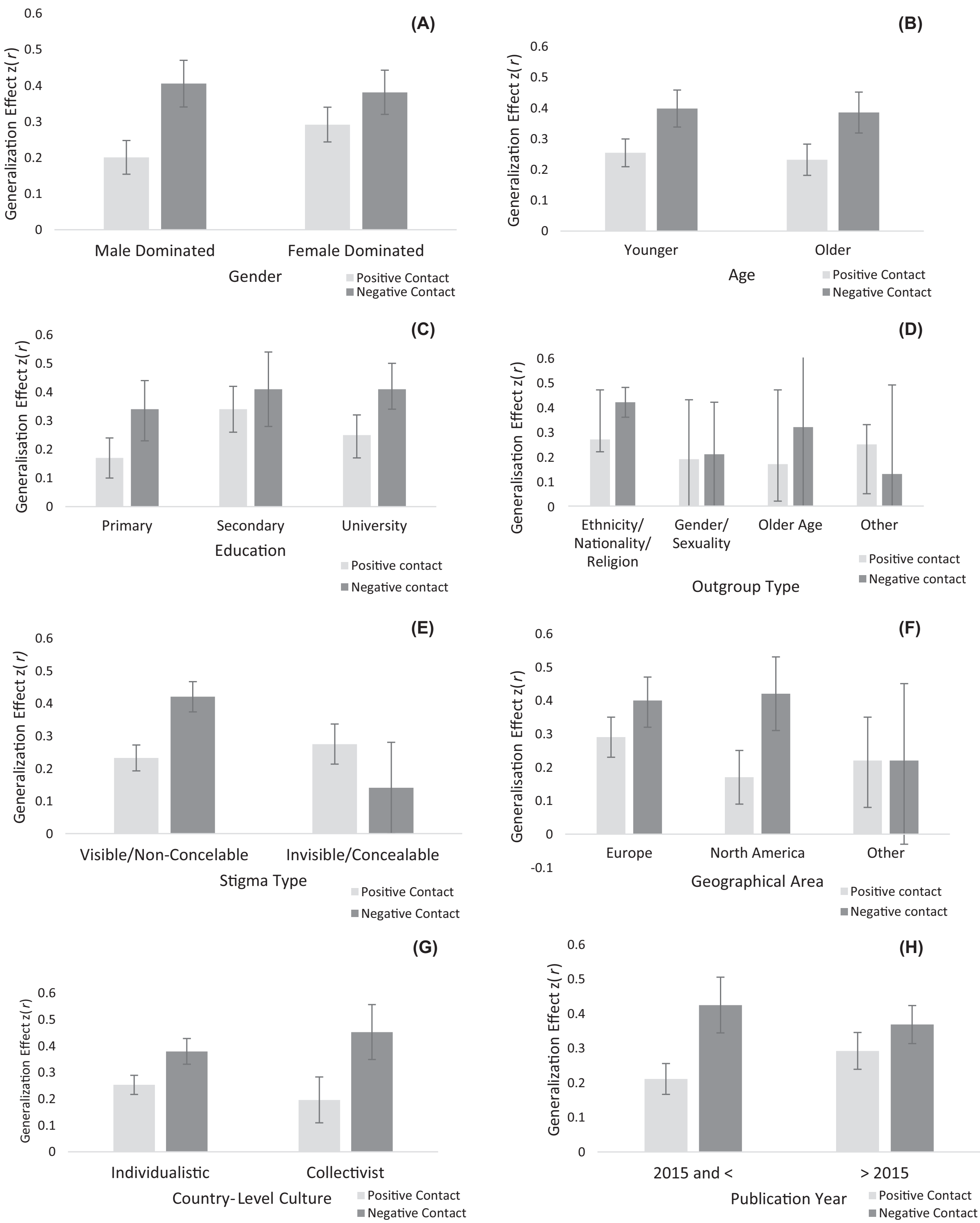
Note. Effects are expressed in terms of valence-congruent individual-to-group generalizations: The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). In all graphs, error bars indicate 95% CIs. (A) Panel displays moderation tests for contact valence and number of contact partners in contact. (B) Panel displays moderation tests for contact valence and number of contact partners in measure. (C) Panel displays moderation tests for contact valence and intimacy of contact partner in measure. (D) Panel displays moderation tests for contact valence and contact length. (E) Panel displays moderation tests for contact valence and contact location. (F) Panel displays moderation tests for contact valence and location of data collection. CI = confidence interval.

be larger for female-dominated samples than male-dominated samples. Although the contact valence by gender interaction was not significant, simple effects analysis and means patterns (see Figure 8A) suggested that negativity biases were stronger for male-dominated samples, $Z = 5.19$, $p < .0001$, than female-dominated samples, $Z = 2.11$, $p = .0350$ (online Supplemental Table S9). This gender difference was driven by responses to positive ($p = .0082$) rather than negative contact ($p = .59$). These results suggest that well-established gender differences in prejudice (e.g., Dozo, 2015; Ekehammar et al., 2003) might reflect males being disproportionately susceptible to negativity biases due to reduced responsiveness to positive contact.

Age was coded based on the age category where the sample age's central tendency fell. To test for moderation, we aggregated studies with children, adolescents, and young adults (attending or not attending college/university) into the "younger" condition and contrasted it against studies with adults and older adults as the "older" condition. The main effect of contact valence was significant

Figure 8

Graphs for the Interplay of Contact Valence With Each of the Ancillary Indicators



Note. Effects are expressed in terms of valence-congruent individual-to-group generalizations: The larger the (positive) effect, the larger the individual-to-group generalization in evaluative congruent directions (e.g., reduced prejudice associated with positive contact; exacerbated prejudice associated with negative contact). In all graphs, error bars indicate 95% CIs. (A) Panel displays moderation tests for contact valence and gender. (B) Panel displays moderation tests for contact valence and age. (C) Panel displays moderation tests for contact valence and education. (D) Panel displays moderation tests for contact valence and outgroup type. (E) Panel displays moderation tests for contact valence and stigma type. (F) Panel displays moderation tests for contact valence and geographical area. (G) Panel displays moderation tests for contact valence and country-level culture. (H) Panel displays moderation tests for contact valence and publication year. CI = confidence interval.

(online Supplemental Table S8); no other effects were significant, both *p*s > .48. Simple effects analysis (online Supplemental Table S9) and visual display (Figure 8B) confirmed that negativity biases were uniform across age conditions: younger, $Z = 3.25$, $p = .0012$; older, $Z = 3.87$, $p = .0001$, so we found no evidence of age moderating valenced contact effects and negativity biases.

We coded the education level undertaken or completed by the majority of participants in the study sample as primary (or unspecified) education, secondary education, undergraduate, or postgraduate education; for moderation analysis, we recoded this factor as a three-level moderator (primary vs. secondary vs. university). Education was marginally correlated with contact valence across studies, $\chi^2(2) = 5.08$, $p = .079$. Positive contact studies tended to be underrepresented among university-level samples and overrepresented among primary and secondary school samples (the opposite for negative contact). This pattern reflects a tradition of structured (positive) contact-based interventions in schools (for overviews, see Cameron & Turner, 2016, R. N. Turner & Cameron, 2016). Despite the lack of independence between the two predictors, the (unique) main effect of contact valence was significant, $Q(1) = 6.54$, $p = .0105$; the main effect of education was also significant, $Q(2) = 9.50$, $p = .0087$, and indicative of larger generalizations for secondary education and university than primary education. All education conditions displayed a pattern for a negativity bias (see Figure 8C). Although the contact valence by education interaction was not significant, the simple effects analysis (online Supplemental Table S5) and visual inspection suggested that the negativity bias was significant and roughly equivalent in size in the primary education and university sample studies, $Z = 2.56$, $p = .0105$, $Z = 3.06$, $p = .0022$, respectively, but slimmer and not significant in the secondary education samples, $Z = .89$, $p = .37$. Hence, there was some evidence of moderation by education, but this pattern was not linear in nature. Direct, prospective, within-study tests of moderation within the same sociocultural context will need to confirm this.

Outgroup type was organized for moderation analysis in four distinct groups: ethnicity/nationality/religion, gender/sexuality, age, and other. The "other" category was a mixed bag of outgroups; the ethnicity/nationality/religion category was disproportionately represented in the pool of eligible studies (81.25%; full descriptives appear in Table 2). As a result, the other levels were largely underpowered, and thus the moderation analyses should be treated as exploratory. Contact valence was once again significant; no other effects were significant (see online Supplemental Table S8). Visually inspecting the effect sizes for the factorial design (Figure 8D), we found patterns indicative of negativity biases for studies focusing on ethnicity, nationality, and religion (.27 vs. .42); gender and sexuality (.12 vs. .21), and age (.17 vs. .32). There was a pattern for positive valence asymmetry in the "other" category (.25 vs. 13). Due to the limited power for most of the contact valence comparisons, the only significant simple effect was found for ethnicity (online Supplemental Table S9).

The variable outgroup type was also recoded to explore broader differences in stigma type, as a function of whether the basis of the stigma was visible/nonconcealable (e.g., ethnicity, age) or invisible/concealable (e.g., sexuality, mental health). We then checked whether this higher order parameter moderated the valenced contact effects and negativity bias. The main effect of contact valence was significant; the main effect of stigma type was not significant but the contact valence by stigma type interaction was, $Z = -3.75$, $p = .0002$ (online Supplemental Table S9). The pattern was for a negativity bias for studies on visible/nonconcealable stigma (.23 vs. .42) and a pattern for a positive asymmetry for studies on concealable/invisible stigma (.27 vs. .14); see Figure 8E. Only the former effect, however, was statistically reliable, $Z = 6.32$, $p < .0001$ versus $Z = 1.46$, $p = .15$ (online Supplemental Table S9). Hence, the concealability of the stigma moderated the valenced contact effects: visible forms of stigma displayed a negativity bias, but invisible stigma did not and instead tended to display a direction consistent with positive valence asymmetries.

The country of data collection was categorized based on broad geographical areas (Europe, North America, other). This moderator was significantly associated with contact valence in our pool of studies, $\chi^2(2) = 6.73$, $p = .035$: Negative contact studies were relatively overrepresented in European and North American studies; positive contact studies were overrepresented in Asia, Oceania, and Africa. Despite this indication for nonindependence, the effect of contact valence was still significant when accounting for geography, $p = .02$ (see online Supplemental Table S4). A marginal main effect of geography was also detected, $p = .06$, reflecting larger generalizations in European and North American studies than in studies from other geographical locations. The interaction was, on the other hand, nonsignificant. More research from non-Western, educated, industrialized, rich, democratic countries settings is needed to confirm these findings.

The country of data collection was also coded to test for the impact of country-level culture. We classified countries based on Načinović Braje et al. (2019; see also Hofstede, 1980). Examples of individualistic countries were the United States, the United Kingdom, and the Netherlands; examples of collectivistic countries were China, Malaysia, and Greece. The culture factor was uncorrelated to contact valence; $\chi^2(1) = 2.17$, $p = .14$. Contact valence was significant when controlling for country-level culture (online Supplemental Table S8); this effect was qualified by a marginal contact valence by culture interaction, $Z = 1.72$, $p = .085$ (see Figure 8G). This reflected a pattern of larger negativity biases for collectivistic than individualistic countries, although both effects were statistically reliable, $Z = 3.94$, $p < .0001$; $Z = 4.06$, $p < .0001$ (online Supplemental Table S9). Hence, culture moderated the bias, with collectivistic countries showing larger negativity biases.

The publication year was recorded as a proxy of research recency and publication bias; more recent research was expected to use more rigorous measurements and designs and to include a larger number of unpublished studies ($Mdn = 2015$; $M = 2008$, $SD = 14.14$). This variable was significantly associated with contact valence, $\chi^2(1) = 5.68$, $p = .017$: As expected, positive contact studies were overrepresented in past studies and negative contact studies in recent studies. This pattern is indicative of a well-established historical positivity bias in the contact literature (Graf & Paolini, 2016; Pettigrew & Tropp, 2006). Despite this nonindependence, the main effect of contact valence was still significant when controlling for publication year ($p < .0001$; online Supplemental Table S8); publication year did not affect the magnitude of the individual-to-group generalizations on its own or in combination with contact valence. While both old and more recent publications displayed a significant negativity bias (both *p*s < .05), its magnitude was visibly larger in the older studies set (.21 vs. .43) than recent studies set (.29 vs. .37; see Figure 8H). Overall, these results suggest that, although tests of negative contact were less represented in older studies, this older research did not display weaker negativity biases. Hence, it is improbable that the overall negativity bias we detected in this synthesis reflects a gross publication bias of our pool of studies.

Overall, we detected ample evidence of moderation of valenced contact effects and the negativity bias by focal moderators derived by sequential models of impression formation and ancillary moderators. We discuss the implications of these findings for theory, intervention, and policy in the next section.

General Discussion

With this research, our intention was to demonstrate the generative power of seeing individuals as active agents in the construction of their social reality (Dardenne et al., 2000; Denton, 2018; Yzerbyt & Leyens, 1991), through intergroup contact, and under naturalistic conditions that allow some freedom in seeking out and avoiding experiences with outgroup members. We built novel theoretical bridges between intergroup contact theory and contemporary experience sampling models (Denrell, 2005; Fiedler, 1996, 2000; Kashima et al., 2000; Van Rooy et al., 2003), thus taking stock of underutilized but relevant research innovations in impression formation research. We established our theoretical and empirical work on an explanation for negativity bias in intergroup contact in terms of differential resampling of experiences with outgroup members following positive (vs. negative) contact under conditions in which some self-selection is possible (Denrell, 2005). From this single simple premise, we predicted the detection of an overall negativity bias in impact across the contact literature, as well as advanced a unified set of novel predictions for moderation by opportunity and motivation to self-select. In extending the scope of moderation predictions this way, we offered an integrative framework for past, contemporary, as well as future research on the valenced contact-prejudice link in ecological settings. We summarize our key results and their implications for theory, practice, and policy in the pages that follow.

Updating Early Syntheses With Contact Across the Valence Spectrum

We expected very different results for our updated test of unvalenced contact effects from those originally documented by Pettigrew and Tropp's (2006) meta-analysis. These expectations were corroborated: Although Pettigrew and Tropp's synthesis of mostly positive contact studies returned an averaged, small-to-medium negative effect size ($r = -.21$), our overall effect size of independent and purest evaluations of the contact-prejudice relationship, which was inclusive of clearly classifiable studies along valence, was a heterogeneous, nonsignificant, near-zero, negatively signed effect size. Hence, when contact fully samples the whole breadth of contact valence—and our selection rules were set up to achieve that—intergroup contact is not necessarily associated with lower outgroup prejudice.

This difference in results is a vivid reminder of historical changes in zeitgeist within this literature (Paolini et al., 2021), now more than before actively encouraging openings to analyses of negative contact (Paolini et al., 2010; Pettigrew & Tropp, 2006; Schäfer et al., 2021). It is also a clear practical warning for practitioners and policy makers to ensure that society is structured and functions in such a way to make contact experiences that resemble Pettigrew and Tropp's set most likely and readily available and stay away from more valence variegated experiences, like those in our primary synthesis.

The ultimate outcome of any synthesis of intergroup contact effects obviously reflects the valences of the contact experiences investigated and included (i.e., a special case of the garbage-in-garbage-out principle; Goel, 2021). When our analyses extended beyond the independent/purest effects to all nested effect sizes of varied evaluative purity and the balance for positive versus negative contact shifted back to favor positive contact, our overall unvalenced contact effect also went back closer to Pettigrew and Tropp's original finding ($r = -.16$). This meaningful variance of results within our own data set signals that intergroup contact on its own is not enough to produce benefits for intergroup relations. Its valence is critical to predict what direction its generalized ripple effects will go.

We see our null/nonsignificant overall unvalenced contact effect from a more balanced set of positive and negative independent/purest effects of higher internal validity; we regard Pettigrew and Tropp's original result (negative/significant) and our unvalenced effect from all nested/varied purity effects superior in ecological validity. Our pool of independent effect sizes reflects purer, higher quality evaluations of valenced contact and prejudice and provides a fuller range and balanced set of contact valences, and thus improved stimulus sampling (Wells & Windschitl, 1999), conducive to firmer conclusions about contact effects across varying valences. At the same time, growing evidence for positive asymmetries in prevalence (Graf et al., 2014; Schäfer et al., 2021) suggests that most ecologies of direct, face-to-face contact display larger frequency of positive than negative contact. If positive asymmetries in prevalence are true, robust, and widespread, then the more optimistic conclusions from Pettigrew and Tropp (and our ancillary multilevel analyses) might generally apply more often (see also MacInnis & Page-Gould, 2015) than our more muted conclusion from the more balanced pool of purest positive and negative contact effects.

Evidence of Valenced Contact Effects and Overall Negativity Bias

Our results do not diminish and should not diminish our trust in or commitment to positive intergroup contact as a key tool for reducing prejudice: We found the same beneficial effects of positive contact that Pettigrew and Tropp did. Our results for valenced contact were consistent with expectations: In both analyses with independent/purest and nested/varied purity effect sizes, positive contact was systematically associated with lower outgroup prejudice and negative contact with higher outgroup prejudice. This pattern proves that contact valence is a powerful moderator of the contact-prejudice link and an important boundary condition for generalized benefits of intergroup contact. It tells us that contact is not invariably associated with reduced prejudice; instead, on average, it produces changes in the direction of its valence—or valence-congruent individual-to-group generalizations.

Critically, being able to discriminate between investigations with qualitatively different modal experiences along the contact valence factor allowed us to carry out unprecedented tests of negative valence asymmetry. Because contact research has significant inbuilt self-selection, we expected it to provide the ideal basis for individuals' differential resampling following valenced contact, as contemplated by Denrell's sequential sampling model, and thus to return a significant overall negativity bias. Consistent with predictions by Paolini et al.'s (2010) model, early prospective tests (Barlow et al., 2012), and Denrell's (2005) sequential sampling model, negative contact showed larger generalization effects than positive contact in both analyses with independent/purest and nested/varied purity effect sizes, meaning that negative contact typically deteriorates intergroup judgments more than positive contact improves them. This generalization advantage of negative contact held even when controlling for a multiplicity of moderators, thus contributing to our confidence that bad is stronger than good in intergroup contact.

These findings help realign intergroup contact effects with the broader psychology literature on negativity bias. Negativity biases are documented in a large range of psychological domains (Baumeister et al., 2001; Rozin & Royzman, 2001). In all of these areas of inquiry, it is well established that negative entities like events, objects, personal traits, and emotions are more influential for judgment and behavior. In social psychology alone, we know that several aspects of information processing, memory, person perception, and attributions key to intergroup relations are also disproportionately affected by negative information (e.g., Fiske, 1980; Ohira et al., 1998; Pratto & John, 1991; Ybarra & Stephan, 1999). This meta-analysis confirms that the fundamental psychological processes that occur in intergroup contact are not at odds with psychological processes investigated in other areas of psychology. These results also reduce some disconnects between a social psychological outlook on intergroup contact and less optimistic research and less optimistic analyses of intergroup contact documented in brother disciplines like sociology, political sciences, and human geography.

The null finding in the moderation test for year of publication suggests that negativity biases were equally present in early contact investigations as they are in more recent research. In the past, we were just not paying attention and equipped conceptually and methodologically to detect them. In expanding our understanding of valenced contact, it is, however, important to recognize that negativity biases in intergroup contact effects are not logically or empirically incompatible with the established notion that intergroup contact typically benefits intergroup relations, as routinely derived from Pettigrew and Tropp's (2006) original tests and now our update with nested/varied purity effects. As long as positive intergroup contact remains more prevalent than negative contact, even the modest beneficial effects of (frequent) positive contact could counteract and progressively erode the more pernicious effects of (rare) negative contact experiences and ultimately lead to overall net benefits for intergroup relations. As Baumeister et al. (2001) eloquently put it, "good may [still] prevail over bad by superior force of numbers" (p. 323).

The negativity bias we detected in our fresh meta-analytical data for direct, face-to-face contact complements the results of early meta-analytical tests of valence asymmetry carried out collating highly controlled, internally valid experiments in stereotype change research (Paolini & McIntyre, 2019). This earlier work detected a significant negativity bias when assessing the impact of indirect contact with members of stigmatized outgroups. It dissipated doubts about direction of causality and confirmed that negative outgroup experiences cause larger detrimental changes in judgments of stigmatized outgroups, than positive experiences cause beneficial changes. The present meta-analysis now tells us that these asymmetrical patterns of valenced generalizations are not limited to static, detached (indirect/non-face-to-face), cognitive-laden experiences with outgroup members away from self-selection pressures. A similar pattern presents in more immersive, dynamic, affect-laden experiences of face-to-face contact, often taking place in naturalistic settings where there is plentiful affordance for self-selection. Our hope is that these fresh meta-analytical results will help settle debates about whether negative valence asymmetries in impact exist (Árnadóttir et al., 2018; Schäfer et al., 2021) and shift the conversation to when (and why) these effects are more/less pronounced—and whether they can possibly even be reversed (see Fiedler et al., 2013).

Negativity Biases Are Heterogeneous and Amplified by Affordances for Self-Selection

Evidence of an overall negativity bias, of course, does not negate logically or empirically the existence of meaningful variations. The overall bias we detected was significantly heterogeneous, which means that it reflects sizeable degrees of nonrandom variability associated with the influence of moderators. This heterogeneity was expected based on similar evidence in the broader psychological literature (Baumeister et al., 2001) and on theoretical ground (Paolini et al., 2010; see also Graf & Paolini, 2016; Paolini & McIntyre, 2019; Schäfer et al., 2021).

In this research, we delved into pointed and systematic tests of moderation derived from Denrell's sequential model of experience sampling. With some variations across indicators, we found that environmental affordances for self-selection amplified the valenced contact effects and the negativity bias in ways consistent with Denrell's model. Simple effects analyses indicated that, more often than not, the negativity bias was either present exclusively or especially under high environmental affordances for self-selection (i.e., more opportunity to opt in and out of contact). According to Denrell's model, affordances for self-selection should create opportunities to differentially resample experiences with outgroup members as a function of contact valence (i.e., resampling a lot after positive and not at all or much less affect negative; see also Fiedler et al., 2013; Thorndike, 1898). These results suggest that people's ability to exert autonomy around intergroup contact, if not harmful, could be risky for broad intergroup relations (cf. Bagci et al., 2021): It would increase the chances of negative—rather than positive—spiraling of intergroup relations following valenced contact through larger negativity biases in generalizations.

Looking at this from the applied perspective of optimizing social cohesion outcomes, practitioners and policy makers would need to stay mindful that affordances for volitional or autonomous contact makes it for intergroup contact that is more pernicious when negative and/or less beneficial when positive. In contrast, mandated contact, like that in structured contact-based interventions or highly structured organizational and educational settings, might be a safer option because it limits individuals' ability to opt out when contact has suboptimal outcomes, thus increasing the chances that resampling will continue and lead to less polarized and more differentiated view of the other. Although mandated contact might not be the most positive and might not associate with the largest generalizations, its practical (but perhaps not political) appeal is in not carrying high risks for negative generalizations and thus offering the ground for perhaps slow and modest but sustained accumulation of benefits for broad intergroup relations.

Our work expands on and departs markedly from other psychological analyses of volition by identifying potentially pernicious societal consequences of individual volition. Cognitive neuroscience decisively emphasizes the benefits of individual autonomy and highlights the dangers associated with others' social influences for both individual wellbeing and social order, reflecting a very individualistic cultural outlook on social relationships and social influence (Bush & James, 2020; Yu et al., 2018; see also Chirkov et al., 2003; Howard et al., 2021). The feeling of being in control over one's own behavior (or sense of agency) is regarded as a fundamental biological need (Leotti et al., 2010), adaptive for survival, as scarcity in agency would reduce one's motivation to actively engage with the physical and social environment (see, e.g., "learned helplessness"; Abramson et al., 1978). Hence, the effort humans expend to regulate their behavior would depend heavily on the belief that we have agency; the mere exertion of choice and even possibility of choice would thus be both subjectively (Shapiro et al., 1996) and objectively rewarding (Tanaka et al., 2008; Tricomi et al., 2004). On the contrary, confusions between sources of one's goals/tasks (self- vs. other-) would be associated with reduced emotional self-awareness, increased rumination (Diefendorff et al., 2000; McGregor et al., 2006; Sheldon, 2014), and cortisol release typical of stress responses to uncontrollable threats (Quirin et al., 2009). Our results underscore the possibility that the host of psychological benefits of individual-level autonomy might come at the cost of risks or drawbacks for group-level and community social cohesion.

Partner Intimacy and Settings With Limited Intimacy Potential Are Best for Integration

Because contact partner intimacy and intimacy-inducing features of the contact setting imply positive expectancies about the upcoming contact, these factors should also moderate contact valence effects, attenuating their magnitude and the negativity bias (Denrell, 2005). We consistently detected larger generalizations under nonintimate and impersonal contact (see also McIntyre et al., 2016). As expected, we also found frequent patterns of valence by intimacy interactions that were not uniform: They were in the direction of intimacy muting negativity biases along closeness indicators but amplifying them along informality indicators. Hence, for social integration, engaging in contact in formalized settings and having achieved closeness with outgroup members provide a superior solution to having contact with distant/unfamiliar others in unstructured and informal settings with just potential for intimacy building. This dissociation between kinds of intimacy brings fresh conceptual clarity and helps reconcile otherwise contrasting results in past research on intimacy and contact valence (Fuochi et al., 2020; Graf, Paolini, & Rubin, 2020).

Although this meta-analysis lacks dynamic information about the possible interplay between these intimacy features, Denrell's model elucidates ways in which the constraints to self-selection posed by formalized settings, due to often clear and restricted roles (e.g., workplace, educational settings, family expectations), might in fact produce positive outcomes in the longer run. These benefits would manifest because people have to stick to intergroup contact under conditions of suboptimal contact (e.g., contact that starts with negative impressions/feelings or mixed valences), which would discourage contact resampling when free self-selection is possible. In other words, involuntary contact would lead to positive outcomes by virtue of "forcing" contact under conditions that would otherwise deter further contact (see also Hodson, 2008). Future research should check the point in time at which contact under formalized settings might need to become voluntary for contact to continue accruing benefits for broad intergroup perceptions.

Valenced Outgroup Expectations Also Moderate Contact Effects via Hot-Stove Drives

Drawing from Denrell (2005), we expected ordinal interactions between contact valence and valenced expectations about the outgroup and the contact setting because of these factors' associations with individuals' motivation to self-select in/out of contact. We anticipated more pronounced valenced contact effects and negativity biases among prejudiced samples, in contact with stigmatized, low status, and low SES outgroups, and in conflict settings; these predictions were all corroborated except for conflict setting. Hence, from a self-selection standpoint, negativity biases are larger under conditions in which individuals are motivated to avoid rather than engage in contact.

From a majority–minority difference lens, the enhanced influence of intergroup contact, especially of the negative kind, among majority members reporting on contact with stigmatized, low status, and low SES is troubling: It suggests that Majority individuals, renowned to benefit the most from positive intergroup contact (Tropp & Pettigrew, 2005), are also those more prone to the destructive influences of negative contact. Future research should check whether this happens because majority individuals enjoy greater contact volition, that is, they are freer to choose whether to engage or not in contact, whereas minority members have these opportunities severely restricted, for instance, by being in service roles (Harwood, 2021).

Factors fostering valenced expectations about the outgroup are not only practically significant for the tailoring of prejudice reduction interventions to specific contexts and populations; they are also theoretically central to contrast alternate explanations for negativity biases (Baumeister et al., 2001; Denrell, 2005; Fiedler et al., 2013; Paolini & McIntyre, 2019). For example, from the onset, we had not ruled out the logical and empirical compatibility of our focal explanation for an overall negativity bias (differential resampling driven by hedonic considerations/risk aversion; Denrell, 2005) with explanations of greater diagnosticity/informativeness of rare negative experiences driven by epistemic considerations (Fiske, 1980; Reeder & Brewer, 1979; Ybarra, 2002; for a review, see Skowronski & Carlston, 1989). Concurrent evidence in the literature of positive valence asymmetries in prevalence (Graf et al., 2014; Schäfer et al., 2021) confirms that, for most people, negative direct contact is a relatively infrequent and thus a salient occurrence. Therefore, the greater diagnosticity of negative contact due to its rarity and thus its greater informativeness could contribute to the overall negativity bias we detected and possibly work in parallel to the differential resampling we focused on (see, e.g., Fiedler et al., 2013).

This meta-analytical evidence does not allow for direct tests of mediation or competing tests of mechanisms. In these circumstances, we can only infer mechanisms indirectly from the shape of interactions between contact valence and preexisting valenced expectations for contact (see Paolini & McIntyre, 2019; see also Fiedler et al., 2013). Ordinal moderations of negativity biases are more consistent with hedonic and risk-aversion explanations than with explanations that invoke concerns about epistemic accuracy, epistemic defense, or ingroup self-enhancement. This meta-analysis found limited, and never significant, evidence for disordinal interactions or reversals of bias. As such, our results suggest that intergroup contact effects, as we captured with this meta-analysis, are dominated by hot-stove motivations (hedonic considerations/risk aversion), placing a premium on stimulus valence rather than on colder motivations (epistemic, schema defense, ingroup enhancement), placing an emphasis on fit between contact valence and preexisting valenced expectancies. As we wait for prospective and direct tests of mediation (especially experimental), we are thus satisfied that Denrell's differential resampling offers to date the most parsimonious and thus a superior single explanation for our valenced contact effects, overall negativity bias, valence by self-selection interactions, and contact valence by valenced expectations interactions.

This prominence of affect over cognitions in direct, face-to-face contact experiences has been recognized before (Esses & Dovidio, 2002; Paolini et al., 2004; Tropp & Pettigrew, 2005). It is inconsistent with an original emphasis (Allport, 1954) on using contact to dispel ignorance about the outgroup (Paolini, Harris, & Griffin, 2016). For example, a meta-analysis of mediation findings (Pettigrew & Tropp, 2008) shows that affective mechanisms, like reduced intergroup anxiety and increased empathy, are superior in explaining contact effects than cognitive levers, like increased knowledge or reduced ignorance.

This prominence of hot-stove motivations over colder motivations does not need to be context invariant. For example, it is worth noting that our results for face-to-face contact are part inconsistent with patterns of generalization detected in the stereotype change literature (Paolini & McIntyre, 2019). In this earlier meta-analysis, positive indirect contact with admired outgroups displayed a generalization advantage over negative indirect contact with these outgroups (a reversal of negativity bias). Different types of outgroup experiences (face-to-face vs. indirect) and settings might instigate different motivational concerns (Paolini & McIntyre, 2019). Face-to-face contact might be more intransigent against the tyranny of negativity because, especially in the wild, it triggers concerns of risk that sharpen valence stimulus appraisals. Less immersive forms of contact, like media contact, parasocial contact, and outgroup storytelling (Harwood, 2021; Vezzali et al., 2014; White et al., 2021), might have greater potential to revert things for the better because they trigger colder motivations that favor cooler appraisals of fit between stimulus valence and preexisting valenced expectancies (see, e.g., Fiedler et al., 2013).

Future research has the important (and exciting) responsibility of establishing whether these ideas of context dependency of qualitatively different motivations can be harnessed to increase the efficacy of prejudice reduction and peace-building interventions. We are not alone to think they might: In the literature, we have already observed calls for individual and community-level shifts from intergroup orientations dominated by fear (Amodio et al., 2003; Phelps et al., 2000) to orientations dominated by hope (Cohen-Chen et al., 2014; Jarymowicz & Bar-Tal, 2006) and higher order cognitions (Amodio & Devine, 2006; Amodio & Ratner, 2011; Fiedler et al., 2013).

Limitations and More Ideas for Future Research

The broad scope of this research helped us highlight evident blind spots in the contact literature and missing research, along conceptually and practically important moderators. The intergroup contact literature remains overrepresented by studies on contact along ethnicity, nationality, and religion (Pettigrew & Tropp, 2006). There were noticeable differences in valenced contact effects as a function of outgroup type, with negativity biases likely to present (with sufficient power) in contact along ethnicity, gender, sexuality, and age. The miscellanea "other" category showed signs of positive valence asymmetries. Hence, prospective tests are needed to check if these trends are true and negativity biases extend beyond ethno-religious contact. Importantly, we found that the concealability of the stigma produced marked dissociations: Negativity biases emerged for visible, nonconcealable forms of stigma, whereas (nonsignificant) positive valence asymmetries were detected for invisible, concealable forms. These results confirm that concealable and nonconcealable stigma do not produce uniform impressions (Chaudoir & Fisher, 2010; Herek & Capitanio, 1996) and downstream consequences (Jonzon & Lindblad, 2005; Lane & Wegner, 1995). These results suggest that individuals use visual cues of stigmatization for their decisions regarding resampling contact with members of stigmatized outgroups or not. When these cues are not available, they would have a weaker basis for differential resampling and bias formation. Why concealable stigma would produce positive valence asymmetries, however, remains unclear and something for future research to establish. Country-level culture also moderated valenced effects: Negativity biases were more pronounced in collectivistic than individualistic societies. This might reflect greater attendance of group memberships in these societies, facilitating especially negative generalizations (Paolini et al., 2010). Yet, our confidence in these results is limited by our focus on research published in English-speaking journals. Hence, although we were able to test for geography and culture, our data are highly skewed toward Western, educated, industrialized, rich, democratic countries samples and societies (Henrich et al., 2010; Rad et al., 2018). More prospective contact research in these evident research blind spots will also increase confidence in this meta-analysis' findings.

This meta-analysis was not preregistered and thus might be more vulnerable to reproducibility issues (Lakens et al., 2016). Also, we discussed earlier how differences/similarities in eligibility screenings and selection of effect sizes for inclusion likely underpin differences/similarities between our overall unvalenced contact effects (for independent/purest and nested/purity varied effects) and Pettigrew and Tropp's and, in so doing, affect trade-offs between findings' internal and ecological validity. We do not see this variance as troublesome because such variance is entirely meaningful and predictable when read in the context of valenced contact effects and wide-spread positive valence asymmetries in prevalence. Yet this variance in overall unvalenced effects urges a note of caution about any underspecified statements about the consequences of contact that fail to spell out the type of contact one refers to.

This meta-analysis' core strength in testing hypotheses relevant to self-selection processes is also this work's main weakness. We stressed the importance of synthesizing a literature inclusive of a large body of correlational field data and primary research with a range of research designs, settings, ways of recruiting participants, and deploying contact with implications for self-selection. This correlational and mixed method quality of the reference literature injected healthy and desirable variability in self-selection in the bulk of the data we synthesized, but with that also significant uncertainty about the actual direction of causality of the effects we isolated. Based on the theoretical springboard we used (Denrell, 2005) and with the confidence that comes from longitudinal (Kotzur & Wagner, 2021) and experimental evidence of negativity biases (Hayward, Tropp, et al., 2017; Paolini & McIntyre, 2019), we interpreted the contact-prejudice links we extracted as indicative of the causal impact of valenced contact and self-selection pressures on generalized outgroup prejudice. The reality is that these empirical links could equally reflect the impact that outgroup prejudice exerts on contact valence appraisals and self-selection. Together with a growing number of contact scholars (e.g., Binder et al., 2009; Kauff et al., 2021; Paolini, Harris, & Griffin, 2016, 2018; R. N. Turner et al., 2020), we regard both the contact-prejudice and the prejudice-contact links theoretically, empirically, and practically important for a fuller understanding of the ecology and dynamics of intergroup contact. At the same time, these complexities and uncertainties carry an implicit invite for future research on the impact of self-selection in intergroup contact to disentangle the two processes with controlled designs (e.g., experimentation, multiwave designs; see Husnu et al., 2024, for initial quasi-experimental tests). These future tests could draw more actively from methods and designs in social neuroscience analyses of volition (Denny et al., 2012; Northoff et al., 2006; Northoff & Bermpohl, 2004; Qin & Northoff, 2011). This meta-analysis says also little about the possible time-dependency between the valence and self-selection factors in naturalistic settings. We coded eligible studies concurrently for modal contact valence and for procedural parameters with implications for opportunity and motivation to self-select. Yet, for example, we do not know whether study participants were blind to the valence of the contact opportunity that was presenting to them (i.e., self-selection precedes valence; see, e.g., Fiedler et al., 2013) or had some clues about the prospective valence of the accessible or avoidable contact when they decided to take part (i.e., valence precedes self-selection). Some sophisticated experimental work on the interplay between these two factors could impose by design a clear temporal articulation, starting from self-selection and continuing with experience valence (Fiedler et al., 2013). The valence of self-selected contact experiences might be particularly salient (and potentially more salient than that of mandated contact; Husnu et al., 2024; Tricomi et al., 2004) by virtue of their uncertain status under conditions in which valence is revealed only after one has actively elected to engage in that experience (O'Doherty et al., 2003; Tanaka et al., 2008; see, e.g., Fiedler et al., 2013). Individuals should actively opt into a contact experience (or elect not to avoid it) only when they have reasonable expectations for a sufficiently productive and safe experience. Also, people should be particularly motivated to discover that valence after they have "gambled" on it under high uncertainty at decision time (e.g., see Stange, 2021). Moreover, the very act of pondering and deliberating about whether to engage in contact or not should increase attention, encoding, and retrieval of those experiences, relative to experiences that are unconditionally available. Future experimental research could systematically vary the temporal relationship between volition and contact valence manipulations (see, e.g., Husnu et al., 2024) or study their natural dynamic occurrence with experience sampling methods or longitudinal designs.

While not an explicit aim of this research, our sharp focus on valenced contact has offered an opportunity to unearth the hidden benefits and potentially superior positive consequences of ambivalent (vs. positive) contact and the existence of contrast or rebound effects in intergroup contact. Ambivalent or mixed valence contact studies constituted only a minority of our eligible studies (8%). Yet, given their novelty (Zingora et al., 2021), they provide valuable, preliminary knowledge. Ambivalent contact was typically prejudice-reducing and was associated with larger valence-congruent effects than positive contact. These results map very closely to experimental evidence in attitude research (Brauer et al., 2012). Think-aloud data from that work suggest that ambivalent outgroup information might be especially effective because it is perceived as more acceptable and truthful. Exclusively positive portrayals of outgroups might instead come across as unrealistic and noncredible on the ground of their evenness or extremity (Kunda & Oleson, 1995, 1997), in ways similar to one-sided (vs. two-sided) persuasive messages (Hovland et al., 1949). The practical significance of these results cannot be underestimated: They suggest that positive-plus-negative might be better than positive-only contact (Árnadóttir et al., 2018; Birtel & Crisp, 2012). Future research should orient some fresh attention to perceived outgroup variability and check if valence mixed experiences lead to more complex outgroup representations (Ostrom et al., 1993) and, as a result, less polarized judgments (Linville, 1985).

Our attention to contact valence helped us also make strides into another unexplored dimension of intergroup contact effects and find first evidence of contrast, rebound, or boomerang effects. Broadly, contrast effects reflect changes in a target judgment in the direction opposite to that implied by judgment-relevant information provided or immediately accessible to the individual at the time of constructing the judgment (e.g., valence-incongruent effects; McIntyre et al., 2016). Contrast effects, together with assimilation effects, have been suggested as possible explanations for negativity biases in some of the psychological literature (e.g., Helson, 1964; Sherif & Sherif, 1967). They have been discussed and documented in the attitude (Bless & Wänke, 2000) and stereotype change literatures (Kunda & Oleson, 1995). Yet, theoretical analyses of contrast effects have been surprisingly absent in the intergroup contact literature, even though there are no obvious theoretical or logical reasons to consider them implausible in intergroup contact. We see this lagging as both metatheoretical and metamethodological: It likely reflects a traditional inattention to fundamental, basic processes in this literature, bringing with that a late appreciation of directions of change as "embedded" in any contact valence. Detecting contrast effects also requires the simultaneous consideration of the valence of contact and the outgroup judgment; this is something that was until now missing (and we just catered for). Consistent with early analyses suggesting that assimilation effects (or valence-congruent effects) are the rule and contrast effects are the exception (McIntyre et al., 2016; Schwarz & Bless, 1992), we found evidence of evaluative incongruent effects in only 7% of our independent/purest effects sizes. Although some of these effects were weak and statistically unreliable, their median value was noticeable, ranging between .29 and .31. Although relatively infrequent, the detection and magnitude of these contrast effects are theoretically noteworthy, as it suggests that—contrary to what has been implicitly assumed so far—we cannot take for granted that contact experiences will inevitably affect downstream processes in evaluatively congruent ways. Hence, we need to be prepared for positive contact, under some circumstances, to lead to worsened intergroup responding and negative contact to improved intergroup responding. The challenge for future research will be to understand the circumstances that make contrast effects more likely (for promising starting points, see Bless & Schwarz, 2010; McIntyre et al., 2016; Mussweiler, 2003; Schwarz & Bless, 1992).

Coda and Conclusions

A large part of 70 years of research on intergroup contact demonstrates that intergroup contact typically reduces prejudice and increases social cohesion. Extant syntheses, however, have not considered the full breadth of contact valence (positive/negative) and have treated self-selection as a threat to validity.

To ease direct comparisons with earlier syntheses, we started our tests disregarding contact valence by design and analysis. Our preliminary analyses assessed the overall effect of intergroup contact on outgroup prejudice coding effects and treating them analytically in the same way as Pettigrew and Tropp's (2006) original meta-analysis. Yet, because our data sets provided representation to newer data on negative intergroup contact that had not entered early syntheses, our unvalenced effect is different from Pettigrew and Tropp's. It reflects the added influence of negative contact. From a fresh outlook on the whole contact valence spectrum, we can now see how Pettigrew and Tropp's early results resemble those for positive and ambivalent contact in our expanded design. As such, our findings corroborate empirically interpretations of early results in the contact literature as being driven by positive contact effects (see Pettigrew & Tropp, 2006, p. 767; see also Graf & Paolini, 2016).

This research rehabilitates self-selection processes: It bridges intergroup contact theory with emerging sequential sampling models of impression formation to assess intergroup contact across contact's full ecologies. From the simple premise that valenced contact instigates differential resampling of outgroup experiences when self-selection is possible (Denrell, 2005), we predicted, meta-analytically tested, and found evidence for positive and negative generalizations, an overall negativity bias, moderation of valenced contact effects, and negativity biases by people's opportunity and motivation to self-select in and out of contact. Consistent with Denrell's model, the negativity bias is particularly pronounced under conditions in which there are opportunities for self-selection and individuals are motivated to avoid (vs. engage in) contact. The bias is larger among male-dominated and prejudiced samples. It is larger in contact with nonintimate contact partners in informal settings. It is larger in contact with stigmatized, low status, low-SES outgroups, and within collectivistic societies.

When understood against a backdrop of widespread positive valence asymmetries in prevalence (Graf et al., 2014; Schäfer et al., 2021), our fresh meta-analytical evidence for positive and negative generalizations and for a negativity bias in intergroup contact effects offers a significant integrative and coherent platform to bring together and understand better intergroup contact research to date. Having established meta-analytically positive generalizations following positive contact and amid highly prevalent positive contact in society, we pointed out that Pettigrew and Tropp's (2006) original meta-analytical results remain highly ecologically sound and an appropriate synthesis of modal consequences of intergroup contact across most settings and populations. Negative generalizations and negative biases in contact effects, however, are also real. Luckily, the rarity of negative contact for most people and contexts means that, as a society, we are most often able to keep these pernicious effects in check. Scholars, practitioners, and policy makers will need to work together to identify and provide the best conditions for positive valence asymmetries to continue doing their work in keeping social order safe.

This meta-analysis also makes a contribution in this important practical direction. As research turns to predictors of contact (Kauff et al., 2021; Paolini et al., 2021; R. N. Turner et al., 2020), intergroup contact researchers feel more at ease with the notion that contact-prejudice associations might be bidirectional and reflect both the effects of contact on prejudice and the effects of prejudice on self-selected contact (Binder et al., 2009; Herek & Capitanio, 1996; Wilson, 1996). Our moderation results for self-selection affordances, consistent with sequential sampling models, elevate self-selection from an unwanted methodological confound to a meaningful process in naturalistic contact settings that is worthy of targeted investigation (see also Paolini et al., 2022). Intergroup contact researchers' early desire to defend the value of contact as a prejudice reduction tool encouraged a view of self-selection to contact as a methodological artifact, potentially polluting "neat" causal inferences for the contact-prejudice relationship (Binder et al., 2009; Herek & Capitanio, 1996; Wilson, 1996), and thus something to control and rule out as threatening internal validity (see, e.g., Pettigrew & Tropp, 2006, pp. 753, 758). Reflecting this traditional view, Pettigrew and Tropp checked for moderation by self-selection in ancillary analyses of their influential meta-analysis and also found that studies' procedural features associated with participants' ability to opt in to contact moderated contact effects. These scholars discounted the value of these ancillary results on the ground that their moderation effect for self-selection disappeared when controlling for variations in research design quality: In their synthesis (as in ours), forced contact studies were more often fully randomized experiments, whereas free choice studies were quasi-experiments, cross-sectional surveys, and field studies. Unlike Pettigrew and Tropp, we see their results as further compelling meta-analytical evidence that self-selection shapes (i.e., moderates) and contributes to explaining (mediates) contact-prejudice relationships.

Bringing sequential models into the mix, our work demonstrates that there are now both solid theoretical and empirical bases to consider self-selection as a key player in explaining valenced contact effects and negativity biases. Self-selection is not only a driving force determining whether contact will happen or not. Now we know that it is also capable of shaping intergroup contact's downstream consequences on outgroup judgments and group-level responding.

Considering opportunities and motivation to self-select, together with contact valence, offers a much more nuanced and integrated platform to design effective contact-based interventions and policies across varied contact ecologies. This work tells us that, from a practical perspective, it is smart to continue taking stock of structured contexts that restrict people's ability to self-select (e.g., workplace, educational and organizational settings) to increase their exposure to diversity while working on creating a climate that motivates to engage in intergroup contact. These conditions best protect intergroup relations from sharp deterioration after rare negative occurrences while ensuring that positive changes can still be accrued, albeit perhaps at a modest but steady rate. Diversity practitioners and policy makers should instead be mindful of contexts that allow self-selection or motivate to avoid intergroup contact because especially these contexts increase the chances of negative spiraling of broad intergroup dynamics following negative contact. To best manage these contexts and mitigate these risks, everything should be done to foster procontact-seeking attitudes as well as individual and collective openness to engage with "the other" (see R. N. Turner & Cameron, 2016; Stevenson et al., 2021; Paolini et al., 2018, 2021) across a multiplicity of group divides. More research on these dynamics is needed. We look forward to future investigations on the downstream consequences of forced, mandated-but-desired, accidental, and voluntary intergroup contact for individuals, groups, and communities.

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