

**The Interaction of Positive and Negative Intergroup Contact**

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Thesis submitted for the degree of

Doctor of Philosophy

Department of Experimental Psychology

University of Oxford

October 2015

## ACKNOWLEDGEMENTS

Firstly, I would like to thank my supervisor Miles Hewstone, for his constant support and encouragement since our first tutorials during my undergraduate degree. I consider myself extremely lucky to work in such an intellectually stimulating and collaborative lab group, and owe it to Miles for inculcating such a positive research environment.

Speaking of the lab group, I would like to thank all the members of OxCSIC who have made my time as a DPhil student an absolute pleasure. In particular, I must thank Katharina Schmid for her frequent assistance and tireless proof-reading; Rachel New, without whom the lab would have long since fallen into abject chaos; Simon Lolliot, for teaching me everything I know about statistics, the Dutch language, and the importance of alcohol in the academic process; and Christina Floe, for her unequivocal support and partnership throughout the whole process.

I would like to extend my warmest thanks to my office-mates in C110, Swati Kanoi, Natasha Phiri and Nils Reimer (as well as Bhavana Kohli and Felix Heise), for their great friendship, encouragement, and capacity to compromise on matters of office temperature. In particular I must thank and congratulate Swati and Natasha, who started the DPhil with me, submitted on the same day, and without whom the office will not be the same (cooler, certainly).

I must thank my collaborators, Miklos Biro, Oliver Christ, Angel Gomez, Nicole Tausch, and Alberto Voci, for their instrumental contributions to this thesis and the work it describes. I would also like to thank the research assistants who have helped me run my experimental studies, Alice Leake, Sara Caccinelli, Hannah Hobson, and again Christina Floe.

Of course, I could never have reached this point without my parents. This thesis is dedicated to them, for their love and tireless support, and for instilling in me the values of curiosity and hard work. I hope that this 50 000 word document will go some way towards persuading them that the latter has finally sunk in.

Finally, I would also like to dedicate this thesis to my primary school headmaster, Nick Stanley. Though as an experimental psychologist I am always cautious about inferring causality, I can safely say that without Nick, I would not be where I am today.

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## ABSTRACT

In the sixty years following Allport's (1954) formulation of the contact hypothesis, very little research has tested the effect of negative intergroup contact. In recent years, several authors (e.g., Barlow et al., 2012; Pettigrew, 2008; Pettigrew & Tropp, 2013) have expressed concern over this positivity bias within the contact literature. In particular, Barlow et al. (2012) presented evidence that negative contact may have a larger effect on prejudice than positive contact. Barlow et al. argue that this contact valence asymmetry could mean that in socially diverse environments (which provide opportunities for both positive and negative contact), negative contact could counteract (or even reverse) the beneficial effects of positive contact. However, a number of studies have shown that rather than combining additively, positive and negative contact may in fact interact (i.e., the effects of negative contact may change depending upon the level of positive contact, and vice versa, e.g., Birtel & Crisp, 2012; Christ, Ullrich, & Wagner, 2008; Paolini et al., 2014). Unfortunately, the extent of evidence for these valenced contact interactions (and indeed for valenced contact effects in general) is severely limited, making it difficult to build any degree of theoretical (or methodological) consensus. The aims of this thesis are therefore twofold: first, to expand the body of evidence for the effects of negative intergroup contact; and second, to test the possible interaction between positive and negative contact as predictors of outgroup attitudes. With these aims in mind, this thesis presents four survey studies and three experiments testing the main effects and interactions of positive and negative contact across six different contact settings. In so doing, it reports strong evidence for the existence of valenced contact interactions. Based on the profiles of these interaction effects, the thesis ends by discussing possible causal explanations, and their implications for the field of valenced contact research.

## DETAILED ABSTRACT

In the years since Allport (1954) first formalised the contact hypothesis, research into the effects and mechanisms of positive intergroup contact has been extensive. However, amongst the hundreds of studies investigating the effects of contact, very few have tested the effects of negative interactions with outgroup members. In recent years, several authors have drawn attention to this omission in the contact literature, and have called for a dedicated research effort to quantify the effects of negative contact (e.g., Barlow et al., 2012; Pettigrew, 2008; Pettigrew & Tropp, 2013). Although this call for research has produced a number of studies comparing the effects of positive and negative contact (herein also referred to as ‘valenced’ contact), the results and methods used to obtain them have been inconsistent. The first goal of this thesis is therefore to expand upon the body of available valenced contact data. In doing so, I hope to provide a stronger foundation upon which to base any claims regarding the comparison of positive and negative contact effects.

In addition to comparing their effects, conceptualising positive and negative contact as discrete independent variables allows researchers to test their interaction. A small number of studies have provided initial evidence that the effects of negative contact may be moderated by positive contact (Paolini et al., 2014), and vice versa (Birtel & Crisp, 2012). Such interaction effects could not only have significant theoretical implications for contact theory as a whole, but could also severely limit the value of accounts of valenced contact that treat positive and negative contact as additive variables (i.e., determining outgroup attitudes by subtracting the effects of negative contact from those of positive contact). The second goal of this thesis is therefore, where possible, to test for interactions between positive and negative contact. With this in mind, I propose a series of four distinct testable interaction hypotheses: the buffering hypothesis (positive contact reducing the effects of negative

contact); the poisoning hypothesis (negative contact reducing the effects of positive contact); the positive augmentation hypothesis (negative contact increasing the effect of positive contact); and the negative augmentation hypothesis (positive contact increasing the effect of negative contact). These hypotheses can be grouped into two mutually exclusive pairs (buffering/positive augmentation, and poisoning/negative augmentation), and may require distinct theoretical explanations. In order to test these hypotheses (as well as to achieve the previously stated goal of increasing the volume of valenced contact data), this thesis presents a total of seven studies, including four survey studies and three experiments.

In order to introduce the topic of valenced contact in detail, in Chapter One I aim to establish the importance of negative contact as a key component of intergroup contact theory. After providing a brief introduction to the development and current state of contact theory, I review existing evidence for valence asymmetries in other areas of Psychology, and discuss how mechanistic explanations from this body of literature could apply to valenced contact. I then summarise the existing evidence for the effects of negative contact, with particular emphasis on the contact ‘caveat’ proposed by Barlow et al. (2012), that the effects of negative contact may be ‘stronger’ than those of positive contact. Finally, I discuss why it may be more appropriate to conceptualise positive and negative contact as interacting (rather than additive) constructs, and present the small number of studies that have tested valenced contact interactions in some way. This leads me to the four specific interaction hypotheses described above, upon which many of the key analyses in the thesis will be based.

In Chapter Two, I present preliminary evidence for the existence of valenced contact interactions in the form of two cross-sectional survey studies. Study 1 involves a survey of positive and negative contact as predictors of readiness for post-war reconciliation amongst samples of Serbs, Bosniaks and Croats from three cities in former Yugoslavia. The survey used in Study 2 also measures positive and negative contact, but instead tests their effects on

outgroup attitudes in Catholic and Protestant respondents from six towns in Northern Ireland. In both studies, I find significant interactions between positive and negative contact (although these are limited to Serbian participants in Study 1). Decomposing the interaction effects indicates that they support either the buffering or the positive augmentation hypotheses, however the cross-sectional nature of the data makes it impossible to specify which. Studies 1 and 2 therefore demonstrate the potential for valenced contact interactions to exist (though the significant group differences also indicate that they are by no means universal phenomena), but more advanced study designs are required to test specific interaction hypotheses.

Chapter Three details one such study based on data from a two-wave longitudinal survey of positive contact, negative contact and outgroup attitudes, once again gathered from Catholic and Protestant respondents in Northern Ireland. Using this two-wave design, I am able to model the effects of negative contact at time 1 on the relationship between positive contact and outgroup attitudes at time 2 (in order to test the buffering/negative augmentation hypotheses), and the effects of positive contact at time 1 on the relationship between negative contact and outgroup attitudes at time 2 (in order to test the poisoning/positive augmentation hypothesis). Following this approach, I report evidence for both the buffering and positive augmentation hypotheses, although once again qualified by significant group differences (the significant interaction effects are limited to Protestant respondents only). These results are similar to those of Paolini et al. (2014) who reported buffering effects of positive contact on the relationship between negative contact and group category salience, and Birtel and Crisp (2012) who found that the effects of an imagined positive outgroup interaction on intergroup anxiety were augmented when preceded by an imagined negative outgroup interaction.

In Chapter Four, I approach the question of valenced contact interactions from a multilevel perspective using a representative national sample of UK residents who completed

a survey on positive contact, negative contact and outgroup trust towards Black and Asian minority groups. Respondents were coded according to residential area, allowing me to model valenced contact at both the individual and the neighbourhood level. In doing so, I report the first evidence for contextual interaction effects. Specifically, I find that neighbourhood-level negative contact augments the effect of individual-level positive contact on trust towards both outgroups (a positive contextual augmentation effect). I also find that neighbourhood-level positive contact augments the effect of individual-level negative contact on trust towards the Black, but not the Asian outgroup. Study 4 therefore demonstrates that positive and negative contact can interact not only across time, but also across level of exposure (direct vs. contextual).

In Chapter Five I follow up my survey studies with a series of three experiments, with the general aim of improving the strength of claims of causality for valenced contact effects. In Experiment 1, undergraduate participants engaged in positive, negative or neutral imagined contact interactions with a homeless person, as well as answering pre-, immediately post- and seven days post-test questions on attitudes towards the homeless as an outgroup. Analysing these data, I find that although participants in the negative contact condition report significantly less positive attitudes towards the outgroup immediately following the imagined interaction, these differences are no longer present at the follow-up test seven days later. Furthermore, I find no significant difference in the pre-/post-test attitude change between the positive and negative conditions. The results of Experiment 1 are therefore rather limited, most probably by the small sample size, and limited strength of imagined contact effects themselves.

In Experiment 2, I employed an extended contact manipulation in which Spanish students were presented with a newspaper article describing a study of attitudes towards immigrants amongst the native population. The article either described the contact as mostly

positive or mostly negative (as well as a no-contact control stating that most Spaniards do not have contact with immigrants). Following the extended contact manipulation participants provided ratings of outgroup attitudes, intergroup anxiety, behavioural intentions, and desire for contact with the outgroup. From these data, I find that participants in the negative extended contact condition show significantly less positive attitudes, higher intergroup anxiety, less contact desire and less positive behavioural intentions towards immigrants than those in the positive or no-contact control conditions. Participants in the positive contact condition also show more positive attitudes towards immigrants than those in the no-contact control condition. I find no significant differences in the effect sizes for the positive-neutral and negative-neutral comparisons for outgroup attitudes. Having also taken pre-test measures of positive and negative direct contact, I report a significant interaction between prior direct positive contact and experimental condition, which matches the profile of the negative augmentation hypothesis (although the effect size for the interaction effect is very small).

In Experiment 3, I present a paradigm for experimentally manipulating direct contact, based on a study by Wilder (1984). In this experiment, Oxford University students were recruited to participate in a study of ‘indirect communication and cooperation’ in which they ostensibly collaborated with a member of a rival university to complete a series of persuasive writing tasks. Based on the feedback apparently provided by the outgroup member (a confederate), I was able to manipulate the valence of the contact interaction to be positive, negative or neutral. In the final section of Chapter Five, I report that attitudes towards the outgroup significantly increase for participants in the positive direct contact condition, and significantly decrease for those in the negative and neutral condition from pre- to post-test. There are no significant differences in the size of these effects.

Finally in Chapter Six I summarise the findings from my seven studies, and discuss their relevance to both contact valence asymmetry and interaction hypotheses. With regards

to the contact valence asymmetry hypothesis, I place my data alongside a collated list of effect sizes gathered from all the studies I have identified that report positive and negative contact effects. Of the forty comparisons I find, only ten are statistically significant, and of those ten only four show negative contact as the stronger predictor of an outgroup-related dependent variable. This, coupled with the presence of significant interaction effects in five of my seven studies, leads me to argue that although negative contact may be ‘stronger’ than positive contact under certain conditions, this is by no means guaranteed. Indeed, given that the interaction effects I found variously supported three distinct hypotheses (buffering, positive augmentation and negative augmentation), I conclude that valenced contact requires a great deal more dedicated research (and replication in particular) before it will be possible to build a consensus on its nature and mechanisms.

## CHAPTER ONE: NEGATIVE INTERGROUP CONTACT

*“It has sometimes been held that merely by assembling people without regard for race, color, religion, or national origin, we can thereby destroy stereotypes and develop friendly attitudes. The case is not so simple.”*

*(Allport, 1954, p. 261)*

### Introduction

By its very nature, social psychology is a field of scientific study that lends itself towards practical, often curative application. The concept of the social psychological intervention is a well established pillar of the discipline and a common product of many avenues of social psychological research, to the extent that several academic journals explicitly dedicate themselves to applied work of this kind (e.g., *Journal of Applied Social Psychology*, *Basic and Applied Social Psychology*, *Journal of Community and Applied Social Psychology*). The natural propensity for effecting positive change is one of the field's greatest strengths, and lends it a high degree of political and social relevance. An exemplary case of this relevance and positive focus may be found in the study of intergroup contact, which has occupied a central position within social psychology ever since the formulation of the contact hypothesis in 1954 by Gordon Allport.

In his book, *The Nature of Prejudice*, Allport argued that under certain conditions (equal status, common goals, institutional support, the opportunity to experience shared humanity, and a lack of personal predisposition towards prejudice) direct, face-to-face contact between members of conflicting groups has the potential to reduce prejudice. Allport's contact hypothesis has provided the foundation for hundreds of published studies detailing the processes, conditions and extent to which contact can improve intergroup

relations. In 2006, Pettigrew and Tropp produced a comprehensive meta-analysis of published and unpublished contact research between 1940 and 2000, comprising 515 studies and a combined sample size of nearly 200,000. This revealed a small but significant and highly reliable negative effect of contact on prejudice (average  $r = -.21$ ,  $p < .001$ ), and it is in part due to the robustness of this basic effect that intergroup contact has developed from a straightforward hypothesis into a rich and widely influential psychological theory (Hewstone & Swart, 2011).

Beyond the boundaries of research literature, the principles of intergroup contact have also formed the basis for numerous policy initiatives. In their book *Advances in Intergroup Contact*, Hodson and Hewstone (2012) provide several examples of contact theory informing political policy, from the post-World War II UNESCO constitution (Besterman, 1951), to desegregation in America (Pettigrew, 1974) and peace initiatives in Northern Ireland (Hewstone et al., 2005). However, Hodson and Hewstone (2012) also point out that, although well-intentioned and usually based upon empirical evidence, attempts to enforce intergroup contact have often been less than effective, even counterproductive (for example the 'colour-blind' approach to desegregated schools in the US - see Cook, 1957; 1979). Furthermore, recent work in contexts such as post-apartheid South Africa has shown that even in societies that have ostensibly achieved social integration at a national or even municipal level, actual face-to-face positive contact between conflicting group members is by no means guaranteed (see Dixon & Reicher, 1997; Durrheim & Dixon, 2010). The notion that contact is not a 'panacea for prejudice' (see Hewstone, 2003) has been present within the contact hypothesis since its foundation, as evidenced by Allport's own assertion that when dealing with the possible positive effects of contact, "The case is not so simple" (Allport, 1954, p. 261). However, when taking the corpus of contact literature as a whole, there remains to this day a

marked lack of research into the potential negative consequences of intergroup encounters, direct or otherwise (see Pettigrew, 2008).

Somewhat ironically for a discipline so often concerned with the identification and reduction of negative psychological biases, the inclination towards positive outcomes has the potential to bias social psychological research in a manner detrimental to its overall goals. In the case of intergroup contact, concerted efforts to maximise the effectiveness of the contact process as a means of reducing prejudice have recently led some commentators to criticise the research literature for its positivity bias (see Paolini, Harwood, & Rubin, 2010; Pettigrew & Tropp, 2006; Pettigrew, 2008). In the aforementioned meta-analysis by Pettigrew and Tropp (2006), only 34 of the 515 studies dealt with instances of contact that were associated with increased levels of prejudice. Furthermore, studies of contact occurring under naturalistic conditions (i.e., without active efforts to promote positive contact outcomes) have shown mixed results (e.g., Christ et al., 2008; Dijker, 1987; Plant & Devine, 2003). Several early studies of contact theory also specifically investigated the outcomes of contact when Allport's optimal conditions were violated (e.g., unequal status, competitive goals etc), and found the levels of prejudice were increased (e.g., Brooks, 1975; Sims & Patrick, 1936).

Concerns regarding the theoretical oversight of a positively focused contact literature are compounded when taking into account the consequences of negative contact in real-world settings. With ever-increasing levels of societal diversity in countries such as Britain (Vertovec, 2007) and the US (Fong & Shibuya, 2005), and an ever-growing number of international migrants (Koser & Laczko, 2010), the opportunities for intergroup contact have never been greater, at least in principle. According to the positive view of contact represented by much of the research literature, one might reasonably predict that this unprecedented level of opportunity for contact should lead to a general decrease in global levels of group conflict and prejudice. Unfortunately, the empirical (and anecdotal) support for this global

improvement in intergroup relations is inconsistent at best, and several recent studies have in fact shown a positive relationship between prejudice and diversity (e.g., Cernat, 2010; Putnam, 2007; Quillian, 1995; Stein, Post, & Rinden, 2000; however, see Meer & Tolsma, 2014, for a review of the literature on diversity and social cohesion including its caveats and methodological issues).

Barlow et al. (2012) argue that the failure of contact models to account for this 'attitudinal homeostasis' can be traced back to their omission of negative contact effects, since diversity promotes opportunities for both positive *and* negative experiences with outgroup members. At an interpersonal level, those in closest proximity tend to include both our closest friends, and our worst enemies (Festinger, Schachter, & Back, 1950; Ebbesen, Kjos, & Konečni, 1976). It is plausible that the 'hidden' effects of negative contact may in some way counteract the benefits of positive contact, particularly given evidence from the broader psychology literature for a widespread 'negativity effect' or 'valence asymmetry' (see Kanouse & Hanson, 1987; Peeters & Czapinski, 1990) whereby negative stimuli tend to have greater impact than similarly intense positive stimuli (for a review, see Baumeister, Bratslavsky, Finkenauer, & Vohs, 2001). However, at present the contact literature is limited in its ability to contend with this positive-negative asymmetry given the lack of empirical research into 1) the consequences and mechanisms of negative contact and, critically, 2) the manner in which positive and negative contact experiences interact when they co-occur. The aim of my thesis is therefore to respond to this lacuna in the intergroup contact literature. The seven studies presented here are intended to provide a comprehensive investigation into the effects of negative contact, and the nature of its interaction with positive contact.

## **Intergroup Contact Theory**

### **Allport's contact hypothesis**

The concept of positive interaction between conflicting groups that would eventually form the basis of the contact hypothesis emerged from the American civil rights movement in the early 20<sup>th</sup> century. Between the 1920s and 50s, several authors began to discuss the idea of intergroup contact as a means of reducing racial tensions in the US (e.g., Baker, 1934; Bogardus, 1928; Watson, 1947; Williams, 1947). In the opening to his chapter on the effects of contact, Allport (1954) provides a quote from a contemporary analysis of the events of the Detroit riots in 1943, “People who had become neighbours did not riot against each other. The students of Wayne University – white and black – went to their classes in peace throughout Bloody Monday. And there were no disorders between white and black workers in the war plants...” (Lee & Humphrey, 1943, p. 130). Allport also provides details of a number of empirical studies which demonstrate the ameliorative effects of contact on racial tensions. For example, Deutsch and Collins (1951) compared attitudes and stereotypes towards black residents amongst white housewives from segregated and desegregated housing projects in New York. Participants from desegregated neighbourhoods were more likely to support the idea of desegregated housing, and tended to perceive less difference between themselves and their black neighbours than participants from segregated areas. Several other early studies provided empirical support for the existence of positive consequences of frequent interactions with racial outgroup members, including white sailors (Brophy, 1945), soldiers (Stouffer, 1977), and police officers (Kephart, 1954) serving alongside black colleagues.

Although much of the early evidence for the effects of contact was optimistic, in setting out his hypothesis Allport repeatedly emphasised the caveat that mere exposure to

outgroup members would not necessarily guarantee positive effects. Indeed, pre-Allport contact theorists such as Williams (1947) tended to assume that most day-to-day contact experiences would not lead to a reduction in prejudice. Allport also points out that even after relative group harmony has been achieved, conflict may still re-emerge (for example, the repeal of anti-Semitic legislation in Germany in 1869 pre-dating the Holocaust, and the occurrence of race-riots in the US continuing in the years following official desegregation). This basic assumption coupled with social and political pressures at the time of writing meant that the primary goal of authors such as Williams (1947) and Allport (1954) was to identify the optimal conditions, under which contact would have positive effects. Exemplifying this goal, Allport's seminal contact hypothesis, as cited by countless social psychologists since its formulation, refers specifically to the final paragraph of chapter 16 in *The Nature of Prejudice*, which runs as follows:

“Prejudice (unless deeply rooted in the character structure of the individual) may be reduced by equal status contact between majority and minority groups in the pursuit of common goals. The effect is greatly enhanced if this contact is sanctioned by institutional supports..., and provided it is of a sort that leads to the perception of common interests and common humanity between members of the two groups.” (Allport, 1954, p. 281).

Allport's optimal conditions were born out of his own systematic investigation, and have received a high degree of subsequent empirical support. In particular, Pettigrew and Tropp's (2006) meta-analysis showed a significantly larger effect size for the contact-prejudice relationship amongst studies in which Allport's conditions were met ( $r = -.29$ ) compared to other samples ( $r = -.20, p < .001$ ). However, the four factors investigated by Pettigrew and Tropp (2006) and most commonly cited as Allport's optimal conditions' (equal status, common goals, intergroup cooperation and institutional support - see Pettigrew, 1998) are by no means the only optimal conditions to have been proposed. In the years following

the publication of *The Nature of Prejudice*, a plethora of such conditions were identified by other authors, including (but not limited to) the presence of a common language, voluntary engagement in contact, and a strong economic context (Wagner, Machleit, Hewstone, & Brown, 1986), relatively low levels of initial prejudice (Ben-Ari & Amir, 1986), stereotype-disconfirming experiences (Cook, 1978), but also the perception that the outgroup member in question is typical of their group (Hewstone & Brown, 1986). This rapidly expanding laundry list of optimal conditions raised concerns amongst several commentators (Pettigrew, 1986; Pettigrew, 1998; W. G. Stephan, 1987) who argued that the failure to define a clear and specific independent variable to represent contact limited the practical application and falsifiability of the hypothesis.

### **Moderators and mediators of contact**

In recent years, intergroup contact research has moved away from what might be termed the 'conditional' approach. In line with general trends in social psychology, contact researchers have progressed from defining specific optimal conditions to more generalisable questions of *when* and *how* contact can reduce prejudice (see Brown & Hewstone, 2005), as embodied by the statistical concepts of moderation and mediation respectively (for a detailed discussion, see Baron & Kenny, 1986). Research into the moderators of contact has built upon our understanding of the enhancing effects of Allport's conditions as investigated in Pettigrew and Tropp's (2006) meta-analysis. For example, Hewstone and Brown have reviewed substantial evidence that contact is most effective when group category salience is high (Brown & Hewstone, 2005; Hewstone & Brown, 1986). Category salience generally describes the extent to which individuals engaged in an interaction are aware of their group memberships, but is also sometimes operationalised as the perceived typicality of the outgroup member. Hewstone and Brown argue that in order for any positive feelings experienced when encountering an outgroup member to be generalised to the outgroup as a

whole, the individuals in question must be aware of their group identities. In other words, they must regard the interaction as an intergroup experience, rather than just an interpersonal one.

The other main focus area for research into the moderators of contact relates to a central but often overlooked element of Allport's original hypothesis, the notion of a prejudicial personality. Although contemporary approaches have taken a less deterministic view than prejudice being "deeply rooted in the character structure of the individual" (Allport, 1954, p. 281), substantial evidence has been collected regarding certain group-directed personality traits which significantly impact upon individuals' reaction to contact. In particular, two personality constructs have been identified as moderators of contact: social dominance orientation (SDO – Sidanius & Pratto, 1999) and right wing authoritarianism (RWA – Altemeyer, 1981). Individuals high in SDO favour the existence of group hierarchies, and are more likely to support social inequality (Sidanius & Pratto, 1999). However, somewhat contrary to Allport's position of prejudicial disposition as a hindrance to contact, Hodson (2008) found that when white prison inmates with high SDO scores experienced positive, authority-endorsed contact with black inmates, they showed greater reductions in in-group bias than those low in SDO. This augmenting effect of SDO has also been found for RWA. RWA represents a convergence of several personality traits, specifically conventionalism, and authoritarian aggression/submission (Altemeyer, 1981). In two Flemish samples, Dhont and Van Hiel (2009) found that participants who scored highly on RWA (and SDO) showed a greater reduction in prejudice following positive contact than low RWA/SDO individuals. Interestingly, Dhont and Van Hiel also tested the effects of negative contact, and found a similar augmentation effect in the opposite direction (i.e., high RWA/SDO participants showed a greater increase in prejudice following negative contact than low RWA/SDO individuals).

As for possible mediators of the contact process, Pettigrew and Tropp once again provide a comprehensive meta-analysis of the three main variables to receive empirical investigation: intergroup anxiety, empathy/perspective taking, and knowledge about the outgroup (Pettigrew & Tropp, 2008). Historically, outgroup knowledge was considered one of the most important mediating processes of contact, and formed a central part of Allport's (1954) hypothesis. Subsequent evidence for this process has been mixed however, with some researchers finding strong general effects of learning (e.g., Eller & Abrams, 2004; Gardiner, 1972; W. G. Stephan & Stephan, 1984), whilst others suggest that only very specific kinds of information, presented in specific contexts can significantly disrupt stereotypes (Rothbart & John, 1985, though note the change in dependent variable from prejudice to stereotypes; contact has shown to influence a large number of dependent variables, and the processes behind these effects are not necessarily the same).

In addition to the cognitive factor of outgroup knowledge, Allport (1954) also discusses the importance of affective variables, focusing on the possibility of building affective ties with outgroup members (later referred to by Cook, 1962, as 'acquaintance potential'). This has certainly been borne out by subsequent contact research, with both empathy and anxiety receiving strong evidential support as mediators of the contact process. The reduction of intergroup anxiety (W. G. Stephan & Stephan, 1985) in particular has received a great deal of attention, with numerous studies showing contact reducing prejudice via reduced anxiety (e.g., Blascovich, Mendes, Hunter, Lickel, & Kowai-Bell, 2001; Islam & Hewstone, 1993; Page-Gould, Mendoza-Denton, & Tropp, 2008; Paolini, Hewstone, Cairns, & Voci, 2004; W. G. Stephan et al., 2002; Voci & Hewstone, 2003). Empathy has received rather less attention, perhaps due to its complexity as a psychological construct and variable definitions (for a discussion of empathy subtypes, see Batson & Ahmad, 2009). Nevertheless, empathy has been identified as a significant mediator of the contact process in several contact

models, usually operationalised as intergroup 'perspective taking' (e.g., Aberson & Haag, 2007; Hewstone, Cairns, Voci, Hamberger, & Niens, 2006).

In their meta-analysis of contact mediators, Pettigrew and Tropp (2008) found that knowledge, anxiety and empathy all significantly mediated the contact-prejudice relationship. Although these three variables are the most commonly studied, several other contact mediators have been proposed, including group threat (both realistic and symbolic, C. W. Stephan, Stephan, Demitrakis, Yamada, & Clason, 2000), intergroup emotions such as anger or disgust (Mackie, Devos, & Smith, 2000), self-disclosure (R. N. Turner, Hewstone, & Voci, 2007), re-appraisal of ingroup attitudes (Pettigrew, 1997), and the complexity of one's cognitive taxonomy of social identities ('social identity complexity', Roccas & Brewer, 2002).

In the years since its foundation, the identification of an array of mediating processes and moderating factors has helped advance intergroup contact from a simple hypothesis into a fully-fledged and richly detailed psychological theory (Hewstone & Swart, 2011). However, the vast majority of research into these subcomponents of the contact process is still firmly couched in terms of positive contact. The mediators of 'contact' as investigated by Pettigrew and Tropp (2008) and many others are strictly speaking mediators of *positive* contact. Although there is, for example, some evidence that anxiety may mediate the effects of negative contact on prejudice (Aberson & Gaffney, 2009; Birtel & Crisp, 2012; W. G. Stephan et al., 2002), relatively few studies have tested mediation hypotheses relating to negative contact. As for the moderators of contact, although it is theoretically (and statistically) possible for a moderator to reverse the valence of a relationship, in practice most research deals with factors which change the magnitude of the effect of positive contact. Such research builds on the post-Allport conditional approach, and remains focused on determining when contact succeeds and when it fails (failure, in this case, implying a lack of positive effects, not the existence of negative effects).

However, even in the early days of contact research, many studies found that contact under suboptimal conditions could result in a worsening of relations, and an intensification of prejudice (e.g., Brooks, 1975; Jackman & Crane, 1986; Sims & Patrick, 1936). There is a conceptual difference between contact that fails to achieve optimal conditions, or in modern terminology, contact under the effect of detrimental moderating conditions (e.g., low category salience), and negative contact. Negative contact is not necessarily just contact in the absence of optimal conditions - it may be qualitatively different from positive contact. This may be intuitively demonstrated through a simple hypothetical: is it possible for an individual to experience both positive and negative contact? If the answer is yes, and the extent of our positive and negative contact can vary independently, then as thorough methodologists we should measure them independently. Although it is likely that many of the moderators and mediators of the positive effect of contact will be shared with negative contact, this is by no means guaranteed, and is as yet unstudied.

### **Positive-Negative Asymmetry**

#### **Evidence for a pervasive negative bias**

There exists a well-established and broadly applicable effect within the wider psychology literature that negative stimuli exert a stronger influence over our thoughts, feelings and attitudes than similarly intense positive stimuli. This is known as the ‘positive-negative asymmetry effect’, ‘valence asymmetry’, or simply the ‘negativity effect’, and has accrued a large body of empirical evidence from a diverse range of psychological disciplines (Kanouse & Hanson, 1987; Peeters & Czapinski, 1990). In their review, Baumeister et al. (2001) discuss numerous examples of the negativity effect including studies showing that negative events have longer lasting and more influential consequences than positive ones (e.g., Brickman, Coates, & Janoff-Bulman, 1978; David, Green, Martin, & Suls, 1997;

Nezlek & Gable, 1999), that negative interactions are more predictive of relationship (dis)satisfaction than positive interactions (e.g., Gottman & Krokoff, 1989; Huston, Caughlin, Houts, Smith, & George, 2001; Wills, Weiss, & Patterson, 1974), and that negative feedback is more memorable, and more likely to influence individuals' self-perceptions than positive feedback (e.g., Coleman, Jussim, & Abraham, 1987; Kuiper & Derry, 1982; Leary, Tambor, Terdal, & Downs, 1995). Negative emotions have been found to influence cognitive processing more than positive emotions (Esses & Zanna, 1995), and are also more readily recalled (specifically, people tend to underestimate the frequency of positive emotions - Thomas & Diener, 1990). Furthermore, many languages have been shown to contain more descriptors of negative emotions than positive emotions (e.g., Averill, 1980; Van Goozen & Frijda, 1993).

At the cognitive level, negative stimuli have been frequently shown to receive more processing (e.g., Abele, 1985; Fiske, 1980; Klinger, Barta, & Maxeiner, 1980), to draw more attention (e.g., Graziano, Brothen, & Berscheid, 1980; Marshall & Kidd, 1981; Öhman, Lundqvist, & Esteves, 2001), and to more readily disrupt cognitive performance when presented as distractors than positive stimuli (Pratto & John, 1991). There also exists evidence for a negative memory bias (e.g., Bless, Hamilton, & Mackie, 1992; Robinson-Riegler & Winton, 1996; Skowronski & Carlston, 1987), although this may be counterbalanced by people's motivation to diminish the impact of negative memories (e.g., through self-enhancing biases – Skowronski, Betz, Thompson, & Shannon, 1991). Further limiting the generalisability of the mnemonic negativity effect, research has shown that the intensity of negative memories decays more rapidly than for positive memories (e.g., Holmes, 1970; W. R. Walker, Vogl, & Thompson, 1997), and that recall is generally more successful for memories whose content is congruent with one's current emotional state, with

a slight bias towards positive information (at least in non-depressed individuals - see review by Matt, Vázquez, & Campbell, 1992).

One area in which the negativity effect has been most consistently demonstrated is social judgement. Numerous studies have shown that negative information has a greater impact on our impressions of others (e.g., Anderson, 1965; Fiske, 1980; Peeters & Czapinski, 1990), and even increases confidence in person-judgements more than positive information (e.g., Hamilton & Zanna, 1972; Yzerbyt & Leyens, 1991). This effect appears to be particularly strong in relation to moral information. Several studies have shown that negative moral information carries disproportionately more weight than positive moral information (e.g., Risky & Birnbaum, 1974; Skowronski & Carlston, 1992). Although the effect is far less pronounced for other kinds of information (e.g., information about an individual's level of intelligence or proficiency with a particular skill - Skowronski & Carlston, 1992), when receiving a range of information about a person the presence of any negative moral behaviours seems to retain the power to negatively skew impression formation (Martijn, Spears, Van Der Pligt, & Jakobs, 1992).

Extending the effects of the negativity bias in interpersonal impression formation, some research has considered the intergroup analogue of stereotype formation. In particular, Mullen and Johnson (1990) conducted a meta-analysis of stereotype formation via distinctiveness-based illusory correlations. Illusory correlations refer to instances where the co-occurrence of unrelated events, particularly unusual or distinctive events, leads them to be falsely causally linked (Chapman, 1967). In their meta-analysis, Mullen and Johnson (1990) found that negative behaviours were more likely to generate illusory correlations than positive behaviours. Of further relevance to stereotype formation, Hamilton and Gifford (1976) presented evidence from two experiments suggesting that the relative infrequency of minority group members within a population leads them to be seen as more distinctive, and

thus more susceptible to stereotype formation via illusory correlations. Finally, Rothbart and Park (1986) found that negative traits require fewer presentations to become associated with a particular target than positive traits. Negative traits also required more presentations before the association was disconfirmed. Baumeister et al. (2001) summarise these findings succinctly, "...bad reputations are easy to acquire but difficult to lose, whereas good reputations are difficult to acquire but easy to lose" (p. 344). If the same were to hold true for contact experiences, this could prove problematic for the positive view of contact, as negative interactions with outgroup members might more readily generalise to negative attitudes towards the outgroup as a whole. The vast majority of the evidence reviewed by Baumeister et al. (2001) suggests that negative experiences will be more salient, more memorable, and more influential over individuals' attitudes than positive experiences. Given this weight of evidence, the need for research into the effects of negative contact becomes extremely pressing.

### **Explanations for the negativity effect**

In addition to summarising the widespread empirical evidence for the negativity effect, Baumeister et al. (2001) also review theoretical explanations for its ubiquity. The broadest explanation offered by Baumeister et al. (2001) is that a universal negative bias is adaptive. The authors argue that negative states indicate a need for the individual in question to make some sort of change, to their behaviour, environment, or attitudes. Positive states require no such change, and can instead be passively absorbed and appreciated. A bias towards the processing of negative stimuli is therefore useful, because it allows for faster adaptation to adverse conditions. S. E. Taylor (1991) develops this line of reasoning further, arguing that negative events elicit two possible response tendencies: mobilisation (activating resources in order to meet a threat head on) and minimisation (attempting to avoid damage or trauma resulting from the threat). It is interesting to note that group conflict researchers have

used concepts similar to S. E. Taylor's (1991) mobilisation and minimisation to describe intergroup action tendencies (Cuddy, Fiske, & Glick, 2007; see also Paladino & Castelli, 2008). Although Baumeister et al. (2001) and S. E. Taylor's (1991) couch their discussions in terms of evolutionary advantage to the individual organism, the selective adaptation account is equally applicable to social groups. Negative stimuli emanating from social outgroups are likely to denote hostility and conflict, requiring action on the part of the ingroup to maintain its status and resources. Positive outgroup behaviour, on the other hand, does not usually demand an instant response. As social organisms, a negative information processing preference is equally adaptive at the group as at individual level.

As for the mechanisms of the negative processing preference, Baumeister et al. (2001) review several hypothesised explanations. Amongst a number of possible theories, Kellermann (1984) proposes that since negative stimuli are less frequent in our day-to-day existence, they may accrue more salience than the more frequent (and thus normative) positive stimuli. Support for this view may be found in studies demonstrating that positive events are judged to be more likely than negative events (e.g., Irwin, 1953; Marks, 1951), with a similar pattern emerging for positive interpersonal relationships (De Soto & Kueth, 1959). Various authors have argued that the world we inhabit is, in actuality or perception, generally positive (Katz, Gutek, Kahn, & Barton, 1975; Lau, 1982; Sears & Whitney, 1973). Note that this positive perception bias does not undermine evidence for the negative processing bias. Indeed, the salience explanation for the negativity effect requires our subjective norm to be positive, in order for negative stimuli to accrue greater salience (in Gestalt terms, negative information stands out as the 'figure' against the positive 'ground').

The importance of contextual norms in driving salience effects is also emphasised by B. A. Wright (1991) in relation to the negativity bias in person judgements, particularly in relation to social 'labels' such as those applied to individuals diagnosed with psychological

disorders. Wright argues that the impact of negative labels like, 'mental hospital patient' is reduced when they are presented alongside rich contextual information. Wright points to a number of factors which contribute to the omission of environmental information when making person judgements, including the well-established fundamental attribution error (whereby we tend to attribute others' behaviour to internal personality factors rather than external environmental factors - Ross, 1977), the high degree of correspondence between person and behaviour (except in situations where anonymity is possible, a person is almost always present when their behaviour is observed, increasing the level of person-behaviour association; but the behaviours themselves are often enacted across numerous different environments, diminishing the level of environment-behaviour association), and even the presence of possessive ties in the linguistic structures used to describe behaviour (we talk about 'a person's behaviour', with no such automatic reference to the environment). When Wright's normative mechanisms are placed alongside the aforementioned positive frequency bias described by Kellermann (1984), this produces a model of the negativity effect in which negative stimuli stand out as the 'figures' set against a 'ground' or context of positive stimuli. In the case of interpersonal or intergroup behaviours, these positive contextual factors will receive less attention, and be less strongly associated with the actors (individuals or groups) than the negative behaviours they enact.

The negative salience/sparse positive context explanation is useful because it allows for some interesting testable predictions. For example, B. A. Wright (1991) suggests that the impact of negative behaviours on person judgements might be mitigated by actively emphasising and enriching contextual information. For example, one might predict that individuals living in neighbourhoods with high contextual levels of positive contact might suffer less detrimental consequences of personal negative interactions with outgroup members. Baumeister et al. (2001) also extend Kellermann's (1984) positive norm theory by

suggesting that people from affluent Western societies might exhibit a more extreme form of the negativity bias (due to the overabundance of positive stimuli) compared to those from conflict- and disaster-afflicted regions in which the frequency of positive and negative experiences may be more balanced (or even tipped towards the negative). Operationalising this in terms of group conflict, one might predict that higher levels of contextual negative contact should diminish the impact of individual negative contact experiences.

However, B. A. Wright (1991) and Kellermann's (1984) approaches to the negativity effect are not without their limitations. Kellerman herself notes that a limitation of the salience approach is that it does not explain *why* statistically infrequent stimuli should receive preferential processing. The selective adaptation account discussed by Baumeister et al. (2001) provides some degree of response to this, but the only empirical evidence supporting the adaptability argument is the general ubiquity of the negativity effect (the reasoning being that if an effect is so prevalent within a particular organism, it is unlikely to be maladaptive). However, more specific hypotheses have been proposed to explain the psychological value of negative stimuli, in particular emphasising the increased level of information and predictive power they provide.

In her review, Kellermann (1984) describes the 'informativeness hypothesis', an extension of correspondent inference theory (E. E. Jones & Davis, 1965), which proposes that behaviour in accordance with social norms is less informative than non-normative behaviour. If an individual is behaving according to social norms, observers cannot be sure whether said behaviour results from the individual's personality, or their inclination to behave in a socially desirable manner. Counter-normative behaviour on the other hand can most readily be attributed to personality factors (though other attributions are possible), and thus provides more predictive information regarding the individual's future behavioural tendencies. The informativeness hypothesis has been cited as an explanation for negativity effects by several

authors (e.g., Fiske, 1980; Gray-Little, 1973; Hamilton & Zanna, 1972), and has been supported by a number of empirical studies demonstrating the increased weight given to infrequent (non-normative) information (Lay, Burron, & Jackson, 1973), in particular negative non-normative information (Hamilton & Huffman, 1971).

There are a number of variations on the informativeness hypothesis describing different ways in which negative information can be more informative. Fiske (1980) proposes the ‘extremity hypothesis’, which states that the informativeness of any given piece of information is determined by its distance from the psychological neutral point, i.e., the more extreme its valence (positive or negative), the more informative it will be. Critically, this approach implies that extreme positive information will also receive preferential processing, provided that it departs sufficiently from the psychological neutral point. Thus, in the context of group conflict, one might predict that individuals exposed to high levels of negative contact would have their outgroup-related neutral point shifted in the negative direction, and would therefore be more susceptible to the effects of positive outgroup information or experiences (which then deviate further from the neutral point).

Another explanation for the informativeness of negative stimuli is provided by Skowronski and Carlston (1989), who theorise that the power of negative information comes from its increased category diagnosticity. Category diagnosticity refers to the ease with which stimuli can be categorised based on the available information. Skowronski and Carlston argue that negative behaviours are more useful in determining category labels than positive behaviours. For example, it is easier to categorise someone as a liar if they are caught lying than to categorise someone as honest if they are seen telling the truth. The reasoning being, according to Skowronski and Carlston, that positive categories have greater requirements of consistency than negative categories (see also Rozin & Royzman, 2001). It takes only a single lie to categorise someone as untruthful, but continuous truthfulness to categorise them

as honest. The category diagnosticity account provides a potential explanation for the ease with which negative behaviours can produce stereotypic group judgements via illusory correlations (see previous discussion of Mullen & Johnson, 1990). In an intergroup context, the category diagnosticity hypothesis would suggest that negative behaviours performed by outgroup members will more readily produce generalisable labels than positive behaviours. This would create a propensity for negative stereotype formation that could only be broken down by repeated exposure to predominantly (or even exclusively) positive outgroup-related information.

In summary, explanations for the general negativity effect focus on three key factors: salience, context and informativeness. Negative stimuli are generally less frequent, and thus more noticeable when they occur. Contributing to this, the context in which negative stimuli are observed is generally positive, but sparsely detailed. Thus negative stimuli not only draw more attention, but are unlikely to have their impact reduced by contextualising information. Finally, the counter-normativity of negative stimuli means that they contain more intrinsic information, particularly with regards to the traits or labels applied to individuals who are seen performing negative behaviours. Although very little of the literature on the negativity effect has specifically dealt with its consequences for intergroup relations, the insights provided by this body of research provide the first steps in building a theoretical foundation for the study of negative contact.

## **The Effects of Negative Contact**

### **Early experimental evidence**

To my knowledge, the earliest experimental manipulation of negative contact was carried out by Wilder in 1984. Wilder was interested in the effect of perceived typicality in interactions with outgroup members. The importance of perceived outgroup typicality

subsequently formed a central theme of Brown and Hewstone's integrative theory of intergroup contact (Brown & Hewstone, 2005; Hewstone & Brown, 1986). Perceived typicality has also been used as a measure of group category salience, a concept which has been proposed to explain differences in magnitude between the effects positive and negative contact (Paolini et al., 2010; Paolini et al., 2014). Beyond this rather prescient contribution to the valence asymmetry debate (i.e., is negative contact stronger than positive contact?), Wilder's studies also provide a valuable first pass at several of the key methodological considerations relevant to the measurement of valenced intergroup contact.

Wilder's experimental paradigm was set up in order to test the interaction between outgroup typicality and contact valence. In his 1984 article Wilder provides three hypotheses as to the outcomes of positive and negative intergroup interactions with typical and atypical outgroup members. Firstly, positive contact will only improve attitudes towards the outgroup as a whole if the partner is perceived as typical of that group. Secondly, and relatedly, positive contact with an atypical outgroup member will have no effect on overall outgroup attitudes, since the partner will be 'subtyped' (a term coined by Weber & Crocker, 1983). Thirdly, negative contact will have an adverse effect on outgroup attitudes regardless of partner typicality because, as Wilder argues, the negativity of the experience will itself be regarded as typical, and thus reinforcing to pre-existing negative attitudes.

All three of Wilder's studies employed the same basic methodology. Participants were female undergraduates from two closely situated American colleges (Rutgers and Douglass), whose rivalry and consistently negative stereotype content were confirmed through pretest questionnaires. During the experiment, participants were brought to a testing room and asked to introduce themselves to their partner whilst waiting for the study to begin. The partner was an experimental confederate, who introduced herself as a member of the outgroup institution. Typicality was manipulated through the confederate's dress (a skirt, blouse and 'moderate'

amount of makeup for Douglass students, faded jeans, oversized plaid shirt and no makeup for Rutgers students), major subject (home economics for Douglass, economics for Rutgers), and behaviour in the waiting room (mention of needing to study and membership of a conservative political club meeting for Douglass; mention of attending a party, and membership of a liberal political club for Rutgers). In the atypical condition, confederates adopted the dress, subject and behaviour of the participant's own college (i.e., confederates from Rutgers dressed like Douglass students, and vice versa).

After completing basic introductions, the participants were brought to a pair of separate booths, and told that they would be taking part in a 'two-person heterogeneous cooperation task. Both 'participants' were then given a timed set of problem-solving tasks, the answers for which were exchanged between participant and confederate between each question. In the positive contact condition, the confederate would give correct or common responses to 60% of the questions, and provide encouraging or commiserating feedback on the subject's responses. In the negative contact condition, the confederate response accuracy was the same as in the positive condition, but the feedback provided on the subject's answers was unhelpful and critical. There was also a no-interaction control condition in which participants completed the problem solving tasks alone. After the first session of tasks, participants returned to a second, shorter set of tasks in order, according to Wilder, to refresh the participants' impressions of the outgroup member. Finally, after completing all the problem solving tasks and providing feedback ratings for their partner, participants were offered the chance to complete a short questionnaire from an ostensibly unrelated study about college life which included questions on attitudes towards various outgroups including members of their partner's college, and endorsement of stereotypic outgroup traits.

The results of Wilder's experiment were generally optimistic. Contrary to his hypotheses, there was no typicality-invariant effect of negative contact. No difference was

observed in favourability ratings of the outgroup college between the negative contact and control conditions. The only condition whose favourability ratings did differ significantly from the control group was the positive-typical condition. Studies 2 and 3 did not include the negative contact conditions, and were intended to break down the effects of positive typical vs. positive atypical contact. The results from Study 1 however, provide the first indication that the negativity effect may not be quite so universal in the context of intergroup contact.

### **Support for Wilder**

Following Wilder's experiment in 1984, the next direct test of valenced contact effects emerged more than twenty years later. In 2008, Pettigrew reported preliminary data on the frequency and effects of positive and negative contact drawn from Heitmeyer's (2005) project on prejudice in Germany (see also, Pettigrew, Tropp, Wagner, & Christ, 2011; Pettigrew & Tropp, 2013). Part of this ten-year project involved a national probability telephone survey carried out in 2004 ( $n = 1383$ ), which included questions on positive and negative contact experiences with immigrants. Specifically, participants were asked four questions on positive contact: "How often has a foreigner helped you?", "How often do you have interesting conversations with a foreigner?", "Now think about encounters with foreigners in Germany. How often have you experienced the following feelings – [satisfied]/[cheerful]?" ( $\alpha = .78$ ), and four questions on negative contact: "How often has a foreigner pestered you?", "Now think about encounters with foreigners in Germany. How often have you experienced the following feelings – [angry]/[irritated]/[fearful]?" ( $\alpha = .78$ , all response scales = 'never', 'seldom', 'sometimes', 'often'). In their chapter on negative contact, Pettigrew and Tropp (2013) report that positive contact was moderately but significantly correlated with negative contact in this sample ( $r = -.18, p < .01$ ), lending credence to the idea that the two variables represent independent phenomena (as opposed to polar ends of a single construct). The sum score of negative intergroup experiences (range 4-

16) was significantly lower ( $M = 6.04$ ,  $SD = 2.2$ ) than the sum score for positive experiences ( $M = 9.30$ ,  $SD = 2.6$ ,  $p < .001$ ), indicating that positive contact was more frequent than negative contact. Furthermore, Pettigrew and Tropp (2013) report that the effect of positive contact on anti-Muslim prejudice ( $r = -.45$ ) was significantly greater in magnitude than the effect of negative contact ( $r = .28$ ,  $p < .001$ )<sup>1</sup>. Although the size of this difference is relatively small, the direction of the effect replicates the results of Wilder (1984).

The increased frequency of positive contact experiences reported by Pettigrew and Tropp's sample supports the previously discussed principle of a positive perception bias (e.g., Katz et al., 1975). The positive perception bias has been suggested as the foundation of salience-based explanations for the negativity effect (since negative stimuli stand out against the generally positive contextual background), which would predict a greater effect of negative contact on prejudice (i.e., negative contact is less frequent relative to positive contact, and thus more salient). However, this prediction is contradicted by Pettigrew et al.'s (2011) results in which positive contact is the stronger predictor of prejudice, suggesting that frequency-derived salience cannot be the sole process at work here.

Alternative theories from the negativity literature may provide explanations for the dominance of positive contact in Pettigrew et al.'s (2011) data, in particular Fiske's (1980) extremity hypothesis. Since attitudes towards outgroups are generally negative, one might argue that the resting point (in Fiske's terminology, the 'neutral' point) of expectations regarding outgroupers' behaviour would skew towards the negative. According to the extremity hypothesis, this would place positive contact further from the neutral point, thus increasing its impact relative to negative contact. Unfortunately, I have been unable to find descriptive statistics for Pettigrew et al.'s data with which to confirm the mean levels of prejudice towards Muslims, and thus provide supporting evidence for the extremity

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<sup>1</sup> This effect has been confirmed using Steiger's (1980) method,  $z = 5.43$ ,  $p < .001$ .

interpretation. In general however, Pettigrew et al.'s results indicate that models of negative and positive contact may need to reflect both objective frequency of past experiences and subjective attitudes towards the target outgroup.

### **The contact 'caveat'**

Barlow et al. (2012) published the first major investigation of the relative effects of positive and negative contact on prejudice. In their introduction, Barlow et al. criticised Pettigrew et al.'s (2011) methodology, in particular their use of only a single negative contact item, "How often has a foreigner pestered you?". Barlow et al. argued that the scope of possible negative contact cannot be represented by a single kind of (fairly common) experience. It should be noted that although this criticism is accurate for the reporting of Pettigrew et al.'s data in their 2011 publication, their 2008 article presented a seemingly identical data set in which negative contact is measured using four variables (detailed above), with the same pattern of results (i.e., positive contact having a larger effect than negative contact).

For Barlow et al.'s (2012) own investigation, they argued that one way to overcome the difficulty of measuring specific instances of negative contact is to ask participants for the overall valence of their contact experiences. In forming their hypotheses, Barlow et al. pointed out that that despite the extensive and reliable evidence for the benefits of positive contact, numerous case studies have shown that areas of high social diversity (and thus, in principle, high opportunities for positive contact) often also show high levels of intergroup conflict (e.g., Ayers, Hofstetter, Schnakenberg, & Kolody, 2009; Cernat, 2010; Durrheim & Dixon, 2010; Putnam, 2007; Quillian, 1995, 1996; Stein et al., 2000). Barlow et al. posited that since such diverse contexts provide opportunities for both positive and negative intergroup interactions, the greater levels of prejudice may be explained by an asymmetry in the effects of positive and negative contact. If the detrimental effects of negative contact do

indeed outweigh the benefits of positive contact (and if these two processes combine in an additive fashion), one would indeed predict a stagnation, or even a worsening of intergroup relations concomitant with increasing levels of diversity. Barlow et al. argued that this positive-negative asymmetry hypothesis would explain the fact that despite increasing diversity at a global level, we have not observed a corresponding decrease in intergroup antipathy such as might be predicted by a purely positive reading of the contact hypothesis. They proposed a positive-negative asymmetry in the effects of contact as a possible mechanism behind this ‘attitudinal homeostasis’.

In order to test the relative strengths of positive and negative contact as predictors of prejudice, Barlow et al. (2012) reported the results of two studies. In Study 1, the authors aggregated data from seven surveys conducted by separate labs in three different Australian states, measuring white Australians' contact with, and prejudice towards black Australians (defined as either Aboriginal Australians or African Australians) as well as Muslim Australians in one sample, and asylum seekers in another. This provided a total sample of 1476 participants drawn from university undergraduate and general community populations between 2003 and 2010. In order to homogenise the coding of the various contact measures employed by the 7 samples, Barlow et al. created standardised indices of contact quantity (1 = *low contact quantity* – 10 = *high contact quantity*), contact valence (1 = *negatively valenced contact* – 10 = *positively valenced contact*), and prejudice (1 = *low prejudice* – 10 = *high prejudice*). These variables were then submitted to a multilevel analysis in which participants (level 1) were nested within samples (level 2) to control for between-sample differences. From this analysis Barlow et al. reported significant main effects of contact quantity ( $\gamma = 0.12, SE = .17, p < .001$ ) and valence ( $\gamma = -0.43, SE = .02, p < .001$ ), as well as a significant interaction between quantity and valence ( $\gamma = -0.03, SE = .01, p < .001$ ).

The coefficients for the main effects reported by Barlow et al. indicate, as expected, that contact valence is inversely related to prejudice (i.e., more positive perceptions of contact are associated with reduced prejudicial attitudes). However, somewhat unexpectedly, the results of Study 1 also suggest a positive correlation between contact quantity and prejudice (i.e., as the frequency of one's contact increases, so too do levels of prejudice). In order to unpack this finding, Barlow et al. presented the simple slopes for the interaction effect. They reported that at low levels of contact valence (1 SD below the mean, interpreted as indicating negative contact), there was a significant positive slope for the effects of contact quantity on prejudice. At high levels of contact valence (1 SD above the mean, interpreted as positive contact), there was a smaller, but still significant positive effect of contact quantity on prejudice. Thus, Barlow et al. concluded that, in line with their predictions, negative contact with black Australians was a stronger predictor of prejudice than was positive contact. However, their results also produced the unexpected finding that increased quantity of positive contact also significantly increased prejudice.

In their discussion, Barlow et al. themselves highlighted the principal methodological issue with Study 1, namely the use of a single bipolar scale to measure contact valence. However, this concern was only discussed with reference to theoretical considerations (i.e., the idea that participants from a multicultural area may have high levels of both positive and negative contact, whilst those from an ethnically homogenous area may experience little of either). This is undoubtedly a valid concern, and one which Barlow et al. resolved in their second study by employing separate unipolar measures of positive and negative contact. However, their discussion ignored the fundamental statistical fallacy engendered by the use of a bipolar contact valence measure.

Barlow et al. argued that a significant positive slope for the effects of contact quantity under conditions of low contact valence represented the effect of negative contact quantity.

This is not the case. The slope in fact represented the effect of *both* positive contact quantity and negative contact quantity (and perhaps even neutral contact quantity), that is the sum total of *all* contact experiences on prejudice, when perceived overall contact valence is negative. This aggregation of all contact experiences helps explain the unexpected finding, as stated by Barlow et al. that, “increased quantity of positive contact also predicted a slight increase in racism” (Barlow et al., 2012, p. 7). This seems highly counterintuitive (at least from a contact standpoint), since it implies that positive contact experiences could increase prejudice. However, the significant positive slope of contact quantity on prejudice under conditions of low (i.e., negative) contact valence actually suggests that even when one's overall contact experiences are perceived positively, the sum total of those experiences may be associated with an increase in prejudice. This is far less surprising given that the sum of one's outgroup experiences is likely to include some negative contact. If those negative experiences are indeed more impactful (as predicted, although at this stage untested by Barlow et al.), it is conceivable that they might outweigh the benefits provided by one's positive contact, leading to a net increase in prejudice.

In Study 2, Barlow et al. introduced independent unipolar measures of positive and negative contact, as well as multiple dependent measures of prejudice. They recruited 441 white American participants from an online survey pool, who completed a survey with questions on contact with, and prejudice towards black Americans. As a result of their criticism of Pettigrew et al.'s experiential measures of valenced contact, Barlow et al. used non-specific single-item questions to measure positive and negative contact: “On average, how frequently do you have POSITIVE/GOOD contact with Black people?” and “On average, how frequently do you have NEGATIVE/BAD contact with Black people?” (1 = 'never' – 7 = 'extremely frequently'). The survey also included an eight-item measure of modern racism (McConahay, 1986), a five-item measure of old-fashioned racism (I. Walker,

1994), a three-item measure of issue-avoidance (Barlow, Louis, & Hewstone, 2009), a 3-item measure of active-avoidance (Barlow et al., 2009), and a three-item measure of scepticism over President Obama's birthplace (all prejudice measures 1 = '*strongly disagree*' – 7 = '*strongly agree*').

The correlation between positive and negative contact in Barlow et al.'s data was similar to that reported by Pettigrew (2008), suggesting a small but significant association between the two variables ( $r = -.26, p < .001$ ). Positive contact was also once again reported as more frequent ( $M = 4.97, SE = 1.36$ ) than negative contact ( $M = 3.01, SE = 1.40, p < .001$ ), and Barlow et al. also noted that both positive and negative contact differed significantly from the scale midpoint (4). Barlow et al. then presented data from five separate hierarchical linear regression analyses testing the effects of positive and negative contact on each of their five prejudice indices. These analyses revealed main effects of positive and negative contact on all five dependent variables, with the exception of scepticism regarding Obama's birthplace, for which only negative contact was a significant predictor. In all cases, prejudice was positively associated with negative contact and negatively associated with positive contact (for regression coefficients, see

Table 1). In order to test the relative effect sizes of positive and negative contact, Barlow et al. reported the results of two path analyses in which all five prejudice indices were regressed onto positive and negative contact. In the first model, all five positive contact paths were constrained to equality, as were all five negative contact paths. In the second model, all ten paths (positive and negative) were constrained to equality. Barlow et al. then reported that the second model showed significantly reduced model fit compared to the first model,  $\chi^2_{\text{change}}(1, 441) = 4.64, p = .03$ , suggesting that, “on average, the magnitude or strength of the association between negative contact and indices of prejudice differed from the magnitude or

strength of the association between positive contact and prejudice” (Barlow et al., 2012, p. 11).

*Table 1. Results from Barlow et al. (2012), Study 2 with Post-hoc Regression*

*Coefficient Comparisons\*<sup>1</sup>*

|   | Modern racism |         |           | Old-fashioned racism |         |           | Issue avoidance |         |           | Active avoidance |         |           | Obama's birthplace |         |           |
|---|---------------|---------|-----------|----------------------|---------|-----------|-----------------|---------|-----------|------------------|---------|-----------|--------------------|---------|-----------|
|   | <i>b</i>      | $\beta$ | <i>SE</i> | <i>b</i>             | $\beta$ | <i>SE</i> | <i>b</i>        | $\beta$ | <i>SE</i> | <i>b</i>         | $\beta$ | <i>SE</i> | <i>b</i>           | $\beta$ | <i>SE</i> |
| Positive contact  | -0.12         | -.15**  | .04       | -0.06                | -.13**  | .02       | -0.15           | -.16**  | .05       | -0.16            | -.16**  | .05       | -0.09              | -.09    | .05       |
| Negative contact  | 0.21          | .27***  | .04       | 0.11                 | .24***  | .02       | 0.23            | .25***  | .04       | 0.20             | .20***  | .05       | 0.11               | .11*    | .05       |
| <i>R</i> <sup>2</sup>                                       | .12           |         |           | .09                  |         |           | .11             |         |           | .08              |         |           | .03                |         |           |
| <i>Regression coefficients comparison</i><br>* <sup>2</sup> | <i>z</i>      | -1.59   |           | -1.77                |         | -1.13     |                 | -0.57   |           | -0.28            |         |           |                    |         |           |
|   | <i>p</i>      | .11     |           | .08                  |         | .26       |                 | .57     |           | .78              |         |           |                    |         |           |

\* $p \leq .05$  \*\* $p < .01$  \*\*\* $p < .001$

\*<sup>1</sup> Results reproduced from Barlow et al. (2012), p. 10

\*<sup>2</sup> Post-hoc comparison of unstandardised regression coefficients using method recommended by Goodman and Blum (1996)

From their analyses in Study 2, Barlow et al. concluded that, in accordance with their predictions, negative contact was a "stronger and more robust predictor" (Barlow et al., 2012, p. 11) of prejudice. However, as in Study 1, this conclusion is undermined by several methodological shortcomings. Firstly, in their five hierarchical regression models, the authors did not control for inflated family-wise error resulting from multiple comparisons. Adjusting their significance criteria using the Bonferroni correction (Dunn, 1959) reduces the appropriate *p* value for establishing significance to .01, which excludes the ostensibly significant relationship between negative contact and scepticism regarding Obama's birthplace.

Secondly, the hierarchical regression approach fails to take into account measurement error which could have been controlled for using structural equation modelling (SEM) with

latent variables. Given the structure of Barlow et al.'s data (in particular the presence of multiple latent outcome variables approximated from several observed variables), SEM would allow the authors to control for multiple sources of measurement error – error which has the potential to bias linear regression models that use aggregated unidimensional scales (for an example of the consequences of failing to control for measurement error, see Bollen, 1989). Even if SEM were not used, path analysis would have permitted the authors to simultaneously test the effects of positive and negative contact on multiple outcome variables, overcoming the issue of inflated family-wise error discussed above.

Thirdly, when presenting the results of their path analysis comparison, Barlow et al. do not report information on model fit, the type of estimation method used, or whether the chi-squared comparison of model fit was adjusted for sample size. Since no fit statistics are included, it is entirely possible that neither of the two models provided an acceptable fit for the data, thus limiting the validity of any subsequent comparison.

Finally, having observed a drop in model fit between the more- and less-constrained models, the authors do not identify the source of model misfit. This could be achieved by running a series of model fit comparisons in which each pair of regression paths is constrained in turn, comparing each single path-constrained model to the less-constrained model from the original analysis. This would allow the authors to identify whether the difference in effect size for positive and negative contact was consistent across all prejudice indices, or instead was driven by a subset of large coefficient differences.

Though Barlow et al. do not provide sufficient data in their report to allow post-hoc testing of these omitted analyses, I have conducted separate comparison of regression weights for the five reported hierarchical regression models using the method recommended by Goodman and Blum (1996). The results of these comparisons can be found in

Table 1, and show no significant magnitude differences between the effects of positive contact and negative contact on any index of prejudice. This, coupled with the omission of key analyses described above, and the theoretical and methodological issues with the operationalisation of valenced contact in Study 1, suggests that Barlow et al.'s conclusions should be treated with caution. From the title onwards, the 2012 paper makes the strong claim that, "Negative contact predicts increased prejudice more than positive contact predicts reduced prejudice". Though theoretically plausible, this assertion is not strictly supported by Barlow et al.'s analyses in their current form.

### **Further evidence for the positive-negative asymmetry of contact**

I have identified six further studies which have provided the data necessary to make a direct comparison of the effects of positive and negative contact on prejudice. The first, reported by Christ et al. (2008) involved a second national probability telephone survey of German nationals' attitudes towards, and contact with foreigners, carried out in 2006 ( $n = 994$ ). Positive and negative contact were measured using similar questions to Barlow et al.'s (2012) items: "How often have you had positive experiences with foreigners living in Germany", "How often have you had negative experiences with foreigners living in Germany" (1 = 'never' to 4 = 'very often'). Prejudice was measured with two items: "When jobs get scarce, foreigners living Germany should be sent back to their home countries" and "There are too many foreigners living in Germany" (1 = 'strongly disagree' to 4 = 'strongly agree'). Christ et al. reported significant main effects of positive contact ( $b = -.314, p < .001$ ) and negative contact ( $b = .084, p = .001$ ) on prejudice (they also tested for the interaction between positive and negative contact, but found no significant effect).

Unfortunately, the conference presentation from which these data are drawn did not include the standard errors necessary to explicitly compare the regression weights following the Goodman and Blum (1996) method. The presentation did, however, include data

describing the relative effects of positive and negative contact on three indices of attitude strength: attitude certainty - "How certain are you in your opinion about foreigners?", attitude knowledge - "How much do you personally know about foreigners?", and attitude importance - "How important is the topic of foreigners for you?". On all three measures of attitude strength, there was a larger effect of positive contact than negative contact. For attitude certainty, the coefficient for positive contact was larger ( $b = .16, SE = .02, p < .001$ ) than for negative contact ( $b = .05, p = .021$ ), but the standard error for the effect of negative contact was missing, precluding further statistical comparison. For attitude knowledge, the effect of positive contact ( $b = .21, SE = .02, p < .001$ ) was significantly larger than the effect of negative contact ( $b = .09, SE = .02, p < .001, \text{Siegler's } z = 3.95, p < .001$ ). The same pattern emerged for attitude importance, with the effect of positive contact ( $b = .16, SE = .02, p < .001$ ) being significantly larger than that of negative contact ( $b = .03, SE = .02, p = .134, \text{Siegler's } z = 4.00, p < .001$ ). These results stand in contrast to those of Barlow et al., suggesting a larger effect of positive contact on prejudice in terms of both attitude content and attitude strength.

This pattern was once again supported by a second source of valenced contact data, provided by Dhont and Van Hiel (2009). In their Study 2, Dhont and Van Hiel reported the results of a survey of Belgian nationals' contact with, and attitudes towards immigrants ( $n = 90$ ). Positive and negative contact were each measured with four items, for example: positive contact – "How often do you have friendly contact with immigrants?", "How often did you have positive experiences with immigrants up till now?"; negative contact – "How often do you have a conflict with immigrants?", "How often do you have unpleasant contact with immigrants?" (1 = 'very little' to 7 = 'very much'), presented along with a 9-item scale of

blatant racism (Billiet & De Witte, 1991). Respondents reported more<sup>2</sup> positive contact ( $M = 3.84$ ,  $SD = 1.59$ ) than negative contact ( $M = 2.34$ ,  $SD = 1.30$ ). Post-hoc comparison of the correlation coefficients for negative contact on racism ( $r = .46$ ,  $p < .001$ ) and positive contact on racism ( $r = -.28$ ,  $p < .01$ ) revealed that there was no significant difference in coefficient sizes ( $z = 1.38$ ,  $p = .17$ ).

Dhont, Cornelis and Van Hiel (2010) also provided positive and negative contact data in a Belgian sample, this time consisting of 172 Flemish police officers who completed a questionnaire of contact with, and attitudes towards immigrants. The positive and negative contact items used in the study were adapted from Dhont and Van Hiel (2009). These data were submitted to a structural equation model which showed positive and negative contact as significant predictors of prejudiced attitudes, which in turn predicted prejudiced behaviour towards immigrants (with both positive and negative contact producing significant indirect effects on prejudiced behaviour, via outgroup attitudes). In terms of the comparison between positive and negative contact in this sample, negative contact was more frequent ( $M = 4.12$ ,  $SD = 1.72$ ) than positive contact ( $M = 3.38$ ,  $SD = 1.25$ )<sup>2</sup>. Positive and negative contact were not significantly correlated ( $r = .01$ ,  $p = .89$ ), and the correlation between negative contact and prejudiced attitudes ( $r = .36$ ,  $p < .001$ ) was not significantly larger than the correlation between positive contact and prejudiced attitudes ( $r = -.33$ ,  $p < .001$ ),  $z = 0.31$ ,  $p = .76$ . The regression coefficient for negative contact on prejudiced attitudes was larger ( $b = .44$ ,  $p < .001$ ) than that of positive contact ( $b = -.36$ ,  $p < .001$ ), but the lack of reported standard errors precludes statistical comparison of the regression weights.

Aberson and Gaffney (2009) provide a fourth source of valenced contact data from a survey of 402 White American undergraduates' contact with, and attitudes towards African

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<sup>2</sup> Post-hoc statistical comparison is unfortunately not possible due to the lack of paired difference scores.

Americans. In this study, positive contact was measured using six items describing the qualities of the respondents' contact with their closest African-American acquaintances (Islam & Hewstone, 1993). Negative contact was measured with 14 items describing specific negative outgroup experiences, which appear to be drawn from a previous study by C. W. Stephan et al. (2000). These measures were then submitted to a structural equation model as direct predictors of intergroup anxiety, realistic threat, and symbolic threat, and indirect predictors of explicit and implicit outgroup attitudes. All direct and indirect paths for both positive and negative contact were found to be significant, but once again a lack of reported standard errors precludes post-hoc comparison of the regression weights. There was, however, no significant difference in the correlations between positive contact and explicit attitudes ( $r = .38, p < .001$ ), and negative contact and explicit attitudes ( $r = -.35, p < .001$ ),  $z = 0.49, p = 0.63$ . There was also no significant difference in the correlations between positive contact and implicit attitudes ( $r = .10, p = .02$ ), and negative contact and implicit attitudes ( $r = -.01, p = 0.84$ ),  $z = 1.31, p = .19$ .

A fifth study of positive and negative contact was carried out in 2013 by Bekhuis, Ruiter, and Coenders. In this study, the authors investigated the effects of positive and negative contact on xenophobia (ethnic distance) amongst 1,444 school children from 77 different classes in 10 Dutch schools. Bekhuis et al. utilised a measure of valenced contact in which participants rated their experiences of contact in their class, their school, their sports clubs, and their neighbourhood as either very positive, positive, neutral, negative, very negative, or not applicable (e.g., if they were not members of a sports club). In the key comparison model, Bekhuis et al. reported significant effects of classroom positive contact ( $\beta = -.164, SE = .051, p < .01$ ), negative contact ( $\beta = .190, SE = .083, p < .01$ ), and lack of contact ( $\beta = -.131, SE = .057, p < .01$ ) on ethnic distance, as well as school positive contact ( $\beta = -.134, SE = .065, p < .05$ ), and neighbourhood negative contact ( $\beta = .158, SE = .072, p <$

.01). All these coefficients were calculated for the effects relative to neutral contact. Bekhuis et al. reported that although negative contact showed a larger beta coefficient than positive contact, the difference was not significant.

Note that from the description of the valenced contact measure provided by Bekhuis et al., it appears that positive and negative contact were treated as mutually exclusive (i.e., participants were required to rate all their contact in each context as either positive, negative or neutral). Although this approach contradicts a key premise of the valenced contact literature (namely the independence of positive and negative contact experiences), Bekhuis et al. applied a between-subjects design in their multilevel analyses, allowing for the comparison of effect sizes between positive and negative contact. The method used to generate the valenced contact scores is therefore somewhat questionable, but I have included this study in the interest of completeness.

Finally, the sixth source of valenced contact survey data was provided by Graf, Paolini, and Rubin (2014), in a study which aimed to remedy several of the methodological shortcomings of previous (negative) contact research. In particular, Graf et al. were concerned with the potential for bias in self-report measures of valenced contact. The authors pointed to work by Schwarz (2007) detailing the inaccessibility or inaccuracy of retrospective self-report data. Graf et al. also noted that the explicitly evaluative nature of valenced contact questions requires participants to make judgements about experiences which they may not otherwise have ascribed valence to. This latter concern is of particular importance, since it may artificially inflate retrospective perceptions of positive or negative contact through the appropriation of experiences that were previously regarded as unvalenced or neutral.

In order to resolve these methodological issues with closed self-report measures, Graf et al. constructed an open-ended measure of contact experiences. This free-response

technique invited participants to provide descriptions of specific contact experiences in a large text-box, cued with the following text: “Can you recall any experiences you had with a [national outgroup member] during your visit abroad or here in [home country]? How did the [national outgroup member] behave in that particular situation? How did you behave? Please describe the situation, below”. In their 2014 paper, Graf et al. provided this question, along with demographic measures, a national identity measure, and two measures of outgroup attitudes, to 1276 student participants from Austria, the Czech Republic, Germany, Poland and Slovakia. Following data collection, participants’ responses were coded by 5 independent raters of different nationalities. Each contact situation described in the free response section was coded by valence (e.g., positive: “the family I stayed with was doing its best to prepare an entertaining program for me”; negative: “this whole experience has knocked them off the pedestal of our paragons”), and situational framing (e.g., person framing: “my classmate worked on the project with great effort”; situation framing: “the atmosphere in the restaurant was quite tense”). Thus, each instance of contact could be ascribed any of four possible coding labels (i.e., positive situational, positive personal, negative situational, negative personal), which were not mutually exclusive, meaning that each instance of contact could include positive and negative elements, as well as both personal and situational framings.

In order to test the relative effects of positive and negative contact in a manner consistent with previous self-report research, Graf et al. first employed a unidimensional coding method in which only exclusively positive or exclusively negative situations were considered. This approach was consistent with self-report valenced contact questions, which treat each intergroup interaction as either positive, or negative. Following this coding method, Graf et al. found that reports of positive contact were around four times more prevalent than reports of negative contact for both person framing,  $\chi^2(673) = 215.69, p < .001$ , and situation framing  $\chi^2(576) = 100.00, p < .001$ . However, Graf et al. pointed out that the unidimensional

coding method falls foul of the same criticisms that they levelled at self-report measures of valenced contact, in that it ignores the possibility for neutral or unvalenced contact. They therefore also analysed their data using a bidirectional coding method, in which each reported contact situation is coded for the presence or absence of the four possible coding labels. Based on the bidirectional coding method, Graf et al. reported that a large number of reported situations were not described in an evaluative fashion, leaving the remainder coded for each of the four evaluative labels: person positivity = 53%, person negativity = 23%, situation positivity = 39%, situation negativity = 21%. Post-hoc chi-squared tests revealed that a significantly greater proportion of positive contact was reported compared to negative contact in both person,  $\chi^2(2252) = 240.37, p < .001$ , and situation,  $\chi^2(2252) = 107.45, p < .001$ , conditions.

In addition to frequency comparisons, the bidirectional coding method allowed Graf et al. to consider each of the four contact attributes as independent predictors of outgroup attitudes. To achieve this, the authors submitted the binary variables for the presence (1) vs. absence (0) of person positivity, person negativity, situation positivity and situation negativity to two multiple linear regression analyses, with their two different attitude measures (a 30-increment feeling thermometer, and a five-point Likert rating for outgroup perceptions of 'good' vs. 'bad'). In the feeling thermometer model (in which contact accounted for 6% of the variance in outgroup attitudes), person negativity ( $b = -2.75, SE = .42, p < .001$ ) was a stronger predictor of outgroup attitudes than person positivity ( $b = 0.17, SE = .34, p > .05$ ), and post-hoc testing using the Goodman and Blum (1996) method revealed this difference to be significant ( $z = 4.34, p < .001$ ). Situation negativity ( $b = -1.19, SE = .44, p < .01$ ) was also a slightly stronger predictor of attitudes than situation positivity ( $b = .93, SE = .35, p < .01$ ), but this difference was non-significant. In the bad-good item model (in which contact accounted for 4% of the variance in attitudes), there was no significant

difference between the regression coefficients for person positivity ( $b = 0.11$ ,  $SE = .04$ ,  $p < .01$ ) and person negativity ( $b = -0.21$ ,  $SE = .05$ ,  $p < .001$ ), or between those of situation positivity ( $b = 0.12$ ,  $SE = .04$ ,  $p < .01$ ) and situation negativity ( $b = -0.09$ ,  $SE = .05$ ,  $p > .05$ ).

Graf et al.'s results add to the growing consensus that positive contact is experienced more frequently than negative contact, and demonstrate this using a less biased reporting method (although it is notable that both contact models accounted for a very small percentage of the variance in participants' attitudes, potentially limiting the value of the approach). Their data also provide some support for the idea that negative contact may have a stronger effect on attitudes than positive contact, but this effect only reached significance using a feeling thermometer measure of outgroup attitudes, when the contact experiences in question were framed in terms of the person, rather than the situation. Furthermore, as with Barlow et al. (2012), the data in Graf et al.'s two multiple linear regression analyses would have benefitted from instead being submitted to a single path analysis (with multiple dependent variables) or structural equation model (with a single latent dependent variable composed of the two observed outgroup attitude variables, which were significantly correlated,  $r(1276) = .41$ ,  $p < .001$ ). Therefore, although Graf et al.'s methodology can potentially overcome many of the biases associated with post-hoc self-report ratings of contact valence, their results do not provide concrete resolution to the question of positive-negative valence asymmetry.

Although not designed to directly test the effect of valenced contact on attitudes, one further study by Paolini et al. (2010) is highly relevant to the discussion of positive negative valence asymmetry. In their paper, Paolini et al. built on Brown and Hewstone's (2005) integrated contact theory, as well as self-categorisation theory (J. C. Turner, Hogg, Oakes, Reicher, & Wetherell, 1987) to investigate the relative effects of positive and negative contact on group category salience. Specifically, Paolini et al. hypothesised that negative contact experiences would make individuals' group categories more salient than positive

contact experiences. Given the evidence for the critical role of category salience in generalising the effects of contact from the contacted individual to the outgroup as a whole (reviewed in detail by Brown & Hewstone, 2005), Paolini et al. argue that such a valence asymmetry effect could explain the apparently greater strength of negative contact as a predictor of prejudice. If the person-directed attitudes formed during negative intergroup interactions generalise more readily to the outgroup as a whole than those formed during positive interactions, this would produce exactly the pattern of negative contact dominance proposed by Barlow and others.

In order to test the effects of valenced contact on category salience, Paolini et al. conducted two studies. The first, an experiment carried out with 52 white undergraduate students from an Australian university, sought to vary the valence of a contact interaction in order to measure subsequent spontaneous references to group membership. Participants completed a 45 minute interaction task, in which their non-white confederate partner's behaviour was either warm and relaxed (positive contact condition), distant and tense (negative contact condition), or somewhere between the two (neutral contact condition). Following the interaction and two filler tasks, participants completed an open measure of group category salience (in which they provided 12 descriptive statements about their partner, which were subsequently coded by condition-blind judges), as well as subjective ratings of the contact experience (partner nonverbal immediacy, positive emotions, and intergroup anxiety). Paolini et al. report main effects of condition on category salience for both primacy (i.e., position of the first reference to ethnicity in the participants' response set, with higher scores indicating earlier ethnicity references and thus higher category salience) and frequency (how frequently ethnicity was mentioned). Specifically, ethnicity-related terms were reported significantly more frequently in the negative condition ( $M = 25.74$ ,  $SD = 9.34$ ) vs. the positive ( $M = 17.50$ ,  $SD = 10.35$ ) and neutral conditions ( $M = 17.65$ ,  $SD = 9.94$ , no

significant difference between neutral and positive conditions). Ethnicity related terms also emerged significantly later in reports of positive contact ( $M = 6.33$ ,  $SD = 4.10$ ) compared with negative ( $M = 9.65$ ,  $SD = 2.74$ ) and neutral contact ( $M = 7.76$ ,  $SD = 4.70$ ). There were no significant difference between negative and neutral conditions. This effect appeared to be related to a lack of nonverbal immediacy and positive emotions in the negative contact condition, since these were the two subjective rating variables significantly related to both ethnicity frequency (immediacy:  $b = -1.97$ ,  $p < .01$ ; positive emotions:  $b = -3.03$ ,  $p < .01$ ) and ethnicity primacy (immediacy:  $b = -0.65$ ,  $p < .05$ ; positive emotions:  $b = -0.86$ ,  $p < .05$ ).

In Study 2, Paolini et al. attempted to replicate the contact valence asymmetry effect on category salience using a two-wave longitudinal survey with 240 undergraduate students from a US university. In the T1 questionnaire, participants completed an open-ended description of a “positive and enjoyable” or “negative and unenjoyable” interaction they recalled having in the past year with someone 65 years or older. They then completed self-report ratings of positive and negative communication behaviours during the interaction, episodic category salience (awareness of group categories during the interaction) and after several filler tasks, chronic category salience (general awareness of the over-65s as a social group category). Eight to twelve weeks later the participants completed the T2 questionnaire, in which they described a second interaction (valence not specified) with the same outgroup member, and completed the same set of self-report questions included in the T1 questionnaire. Analysing these data, Paolini et al. found significant cross-sectional relationships between all contact valence indicators (deficit-oriented interactions, positive and negative disclosure, and disdainful emotions) and episodic category salience (these effects were markedly less extreme for chronic category salience, although significant effects for negative disclosure and deficit orientation did emerge). Finally, Paolini et al. tested for a longitudinal valence-salience mediation path, and found a significant indirect effect such that

more positive disclosure and less disdainful emotions at T1 led to less episodic category salience at T1, which in turn was significantly associated with less chronic category salience at T2 10 weeks later. The authors also found a single salience-valence mediation effect from episodic (T1) category salience to chronic (T2) category salience via T1 negative contact (positive disclosure).

The results of Paolini et al.'s studies provide a fairly reliable demonstration that negative contact has a stronger effect on category salience than does positive contact (results corroborated by a follow-up study by Paolini et al., 2014, see below). This in turn provides a possible mechanism to explain the positive-negative asymmetry effect of contact on attitudes. However, Paolini et al. (2010) did not directly test the effects of category salience on intergroup attitudes, and even if they had, the evidence for the existence of that valence asymmetry effect is inconsistent at best. From the pre-existing survey data relating to the effects of positive and negative contact on prejudice, four studies have found positive contact to be the stronger predictor of prejudice (Aberson & Gaffney, 2009; Christ et al., 2008; Pettigrew et al., 2011; Pettigrew, 2008). However, there are limits to the interpretations which can be drawn from these data, given the inconsistency in effects across multiple dependent variables (Graf et al., 2014), an inability to compare contact coefficients (Christ et al., 2008), and suboptimal contact measures (Pettigrew, 2008). Four further studies have found negative contact to be the stronger predictor of prejudice (Barlow et al., 2012; Bekhuis et al., 2013; Dhont et al., 2010; Dhont & Van Hiel, 2009). However, none of these papers produced a statistically significant difference between the coefficients for positive and negative contact when subjected to post-hoc testing. Thus, the possibility of a contact-specific negativity effect remains unresolved.

## **The Interaction of Positive and Negative Contact**

### **Advantages of the interaction approach**

In addition to the various methodological and theoretical challenges that have emerged from research on the positive-negative asymmetry of contact, there is a broader issue which calls into question the basic assumption of a negativity (or positivity) bias. The move towards treating positive and negative contact as independent unipolar scales is of great value to the intergroup contact literature, but thus far the full potential of this reformulation has yet to be exploited. With very few exceptions, the majority of research into the relative effects of positive and negative contact has treated their effects as additive. The implication of papers such as Barlow et al. (2012, see also Pettigrew & Tropp, 2013) is that the sum total of our negative contact experiences is subtracted from the sum total of our positive experiences, with the resulting difference determining our outgroup attitudes. However, an additional statistical property of modelling positive and negative as independent variables is that we can consider their interaction. That is, how does one's experience of positive contact alter the effects of negative contact (and vice versa)?

As a avenue for contact research, the potential interaction between positive and negative contact is both theoretically interesting, and methodologically advantageous. At the theoretical level, many of the explanations for the general negativity effect (see above) involve interaction processes. For example, Kellermann's (1984) salience account argues that the preferential psychological value applied to negative stimuli results from their relative uniqueness against the common 'background' of positive stimuli. Baumeister et al. (2001) extrapolates from this explanation, suggesting that the frequency of positive stimuli in one's environment should determine the strength of the negativity effect. Baumeister's hypothesis fundamentally describes an interaction between positive and negative stimuli, whereby the

frequency of positive stimuli alters the impact of negative stimuli. Since the negativity effect literature provides the nearest available proxy for explanations of valence asymmetry effects in intergroup contact, one might reasonably predict such an interaction between the positive and negative contact.

Furthermore, within the context of intergroup contact, each contact ‘experience’ (insofar as it is possible to impose boundaries on discrete contact experiences) does not occur in a vacuum. Social norms, prior experiences, conscious and unconscious associations, attitudes, stereotypes, and affective responses can all influence how an interaction with an outgroup member is interpreted, and how this interpretation will be absorbed into the contact recipient’s outgroup schema and social identity. This plethora of intergroup processes provides a list of variables that could potentially bias any attempt to measure valence asymmetry. The study of valenced contact interactions may suffer less from the issue of measurement bias than work focussed on valence asymmetry effects, because interactions do not require a common unit of measurement between positive and negative contact.

In order to accurately measure the additive effects of positive and negative contact, the two constructs need to be measured using a common scale. By dint of their research question, valence asymmetry researchers need to calculate the effects of positive and negative contact using the same unit of measurement, such that the amount of attitude change per unit of positive contact can be directly compared to the amount of attitude change per unit of negative contact. Barlow et al. (2012) approach this problem using generalised, content-matched self-report questions. However, achieving a valence-independent standardised unit of contact through self-report is extremely difficult, if not impossible, because the very processes that the valence asymmetry researchers seek to investigate (e.g., salience effects, recall bias, and all the intergroup processes listed previously) will necessarily introduce asymmetric bias into participants’ self-reports. It is extremely difficult to disentangle the

question of 'which is stronger', from confounds such as, 'which is easier to elicit', 'which is easier to recall', and 'which is perceived as normative'.

When considering valenced contact interactions however, the question is no longer 'which is stronger, positive or negative contact?' but instead, 'how do the effects of positive contact change at different levels of negative contact (and vice versa)?'. In this instance, it is clearly useful to have some standardised unit of contact (for example, if the experiences referenced by the negative contact measure are more extreme or intense than those described in the positive contact measure, this may skew the negative contact data and thus limit the potential for identifying interactions), but it does not fundamentally undermine the question if measurement biases are allowed to play out. Whether or not our measures of positive and negative contact describe identical, valence-invariant units of contact matters less, since we are interested in the *processes* of change, rather than the magnitude of that change.

Unhampered by the need for a common unit of measurement, the interaction approach allows us to investigate extremely important and socially relevant questions about the effects of contact beyond the issue of valence asymmetry, for example: how will a positive contact experience be interpreted if the individual in question has a history of negative contact? Can positive contact protect against the detrimental effects of negative contact? How does the level of positive or negative contact in someone's social context influence their individual intergroup experiences? It is my goal in this thesis to provide answers to (or at least, to begin to ask) questions of this kind.

### **Preliminary evidence for interaction effects**

The empirical research on the effects of negative contact is limited, and the research on its interaction with positive contact more limited still. However, a small number of studies have investigated certain aspects of valenced contact interactions, from various theoretical starting points. For example, the aforementioned conference presentation by Christ et al.

(2008) not only included main effects for positive and negative contact on outgroup attitudes and attitude strength, but also tested for interaction effects. In particular, Christ et al. were interested in the questions of whether having some mixed contact experiences was better or worse than having little contact of any kind. To investigate this question, the authors presented data on the ‘diagonal contrast’ derived from the simple slopes of the positive contact\*negative contact interaction term, with which they compared hypothetical individuals with high levels of positive and negative contact against those with low levels of positive and negative contact.

In their first study, Christ et al. reported the interaction effect from the same German probability survey data described by Pettigrew (2008), and Pettigrew et al. (2011). In these data, Christ et al. found no significant interaction between positive and negative contact, but did report a significant diagonal contrast between the hypothetical high positive/high negative and low positive/low negative conditions,  $b = -.18$ ,  $t(1316) = 5.85$ ,  $p < .001$ . This result indicated that those with frequent but mixed contact experiences (i.e., high levels of positive *and* negative contact) still reported less prejudice than those with little contact of either valence. The same pattern was observed in Christ et al.’s second study (using data from a second German telephone survey, described previously), where once again there was no interaction effect, but a significant diagonal contrast between the high positive/high negative and low positive/low negative conditions derived from the simple slopes,  $b = -.23$ ,  $t(988) = 7.03$ ,  $p < .001$ . Although Christ et al. did not find significant interaction effects in either of their studies, their results undermine the additive valence asymmetry account, because the change in positive contact (from low/low to high/high conditions) was associated with a relative reduction in prejudice, despite also involving an increase in negative contact.

Providing further evidence for the non-additive interaction of positive and negative contact, Birtel and Crisp (2012) report experimental evidence for the effects of contact order

on attitude outcomes. In their paper, Birtel and Crisp sought to apply psychotherapeutic methods to the 'treatment' of prejudice, in particular through an intergroup analogue of exposure therapy. In clinical settings, exposure therapy refers to a technique for treating anxiety disorders, particularly those involving specific anxiety-inducing objects or situations (e.g., phobias). The principle of exposure therapy is to modify the fearful associations with anxiety-eliciting stimuli through gradual exposure under safe, controlled conditions. Based upon this emotion-processing account, Birtel and Crisp hypothesised that an effective way to reduce prejudice might be to initially expose participants to a negative interaction with a stigmatised (anxiety-inducing) outgroup, followed by a positive interaction to modify the fearful association. In order to test this hypothesis, the researchers conducted a series of experiments in which the order of positive and negative imagined contact interactions was manipulated.

In Birtel and Crisp's first experiment, twenty-nine participants completed an online task in which they were asked to imagine interacting with a person with schizophrenia. The first group was asked to imagine a negative interaction (cue text, "Please take a moment to imagine yourself meeting an adult with schizophrenia for the first time. Imagine that the interaction is negative, tense, and uncomfortable."), followed by a positive interaction (cue text, "Please take a moment to imagine yourself meeting an adult with schizophrenia for the first time. Imagine that the interaction is positive, relaxed, and comfortable."), whilst the second group imagined two positive interactions. Following each imagined interaction, participants were asked to write down what they had imagined, in order to reinforce the strength of the imaginary manipulation. Participants also completed a measure of intergroup anxiety towards people with schizophrenia after each imagined interaction.

Birtel and Crisp found a significant main effect of time, with participants reporting less intergroup anxiety after the second imagined interaction ( $M = 2.93$ ,  $SD = 0.70$ ) than after

the first ( $M = 3.91, SD = 0.78$ ),  $F(1, 27) = 48.17, p < .001$ . Crucially, they also identified a significant interaction between time and condition,  $F(1, 27) = 23.61, p < .001$ . Unpacking the interaction using simple main effects, Birtel and Crisp found that after the first interaction, participants who experienced negative contact reported significantly higher anxiety ( $M = 4.31, SD = 0.80$ ) than those who experienced positive contact ( $M = 3.48, SD = 0.48$ ),  $F(1, 27) = 11.26, p = .002$ . However, following the second interaction, participants in the negative-positive condition reported lower levels of anxiety ( $M = 2.69, SD = 0.81$ ) than those in the positive-positive condition ( $M = 3.19, SD = 0.45$ ),  $F(1, 27) = 4.21, p = .046$ . This pattern of results was replicated with future contact intentions as the dependent variable in Experiment 2a (this time with gay men as the target outgroup, for a sample of 32 heterosexual male students). Future contact intentions were significantly higher for participants in the negative-positive condition ( $M = 5.25$ ) compared to those in the positive-positive condition ( $M = 3.99$ ),  $t(30) = -2.48, p = .019$ . A Sobel test of mediation also revealed that the effect of condition on future contact intentions was significantly mediated by intergroup anxiety ( $Z = 2.06, p = .039$ ).

Birtel and Crisp's results cannot be directly compared to those of Christ et al. (2008), because they involve discrete (imagined) outgroup interactions, rather than retrospective measures of overall valenced contact. However, both studies strongly contradict the idea that the effects of positive and negative contact are simply additive. In the case of Birtel and Crisp's experiments, the change in intergroup anxiety between a negative and a positive interaction was much larger than between two positive interactions. There are two possible interpretations for this effect. First, it might be concluded that the increase in anxiety following the initial negative interaction simply meant that there was more 'room for improvement' in the negative-positive condition. However, this interpretation does not explain why the final levels of anxiety were significantly lower (and future contact intentions

significantly higher) in the negative-positive, compared to the positive-positive conditions. The straightforward 'room for improvement' account predicts that levels of anxiety would, at best, return to an equal level in both conditions, with the subsequent positive contact working to 'reset' participant to their default resting state. Instead, Birtel and Crisp's results show an augmentation of the effects of positive contact following a negative interaction, so much so that anxiety and contact intentions are improved beyond the capacity of positive contact alone. This key finding suggests a second interpretation, more in line with salience accounts of the negativity effect, wherein the initial negative experience renders the participant more susceptible to the effects of subsequent positive contact than they would otherwise be. In this way, Birtel and Crisp's study demonstrates a true positive augmentation effect. It should be noted, however, that Birtel and Crisp's study was conducted on a very small sample (only twenty-nine participants), and that the inclusion of intergroup anxiety questions directly following the imagined interactions may have produced demand characteristics.

Assuming that their results are genuine, an interesting question resulting from Birtel and Crisp's design concerns what the result of a positive-negative, or indeed a negative-negative condition would be. In principle, the salience account would predict a worsening of outgroup attitudes in both cases, with the effect exacerbated in the positive-negative condition (i.e., a direct inversion of the results from Birtel and Crisp's study). However, data from a 2014 study by Paolini et al. (2014) suggest that this 'negative augmentation' effect may not, in fact, occur. Providing a follow-up to the work of Paolini et al. (2010), Paolini et al. (2014) investigated the extent to which the effect of negative contact on category salience was moderated by prior positive contact. The authors point out that the level of category salience in a given contact scenario is determined by, "a dynamic interaction between the qualities of the specific and discrete contact experience and the qualities of pre-existing and chronic individual expectations about the specific groups involved" (Paolini et al., 2014, p.

549; see Blanz, 1999; Oakes, Turner, & Haslam, 1991). In particular, the category salience literature emphasises the importance of stereotypic fit, with outgroup behaviours that appear consistent with pre-existing stereotypes or attitudes leading to increased category salience (Coats, Latu, & Haydel, 2006; Reynolds, Turner, & Haslam, 2000). These pre-existing expectations about outgroup behaviour in part reflect the individual's past contact experiences (Oakes, Haslam, & Turner, 1994; Bruner, 1957). Thus, Paolini et al. hypothesised that past experiences of positive contact should diminish the effect of negative contact on category salience, by reducing its perceived fit.

In order to test this buffering effect of positive contact, Paolini et al. carried out one correlational study, and three experiments. In the correlational study, 405 Northern Irish students completed a questionnaire that included measures of past contact quantity (before coming to university), current contact quality (frequency of cross-group friendship visits), perceived change in contact quantity before and after coming to university, and category salience. Paolini et al. report a significant main effect of contact quality on category salience ( $b = 0.16, p < .001$ ), and a significant moderation effect of past contact quantity ( $b = -0.07, p = .040$ ) such that participants with high levels of past contact showed a smaller effect of current contact quality on category salience. The authors point out that these data are purely correlational, and that the measure of contact valence is only a proxy. I would like to emphasise this latter point, given that the frequency of cross-group friendship visits only represents quality of positive contact. This study therefore suffers from the implied non-independence of positive and negative contact that has undermined previous valenced contact research.

In their first experiment, Paolini et al. assigned 83 American students to either positive or negative television-mediated contact conditions in an online study. The contact manipulation took the form of a 10-minute excerpt from a documentary which included

interactions between a US border patrolman and a family of Latino illegal immigrants. The content of the excerpt was manipulated to be either mostly positive (approximately 80% positive interactions, 20% negative, positive final outcome), or mostly negative (80% negative, 20% positive, negative final outcome). Following the clip viewing, participants completed manipulation checks of interaction quality, category salience, quantity of past contact (“how often do you believe you have interacted with illegal immigrants?”) and quality of past contact (“how positive or negative have your interactions with illegal immigrants been?”; 1 = ‘*Very negative*’ - 7 = ‘*Very positive*’). After confirming the perceived valence of the positive and negative conditions through manipulation checks, Paolini et al. report a significant main effect of condition on category salience ( $b = 0.54, p < .001$ ) such that category salience was higher in the negative contact condition ( $M = 4.87, SD = 1.28$ ) than in the positive contact condition ( $M = 3.80, SD = 1.10$ ). The authors also report a trend interaction effect between condition and past contact quality ( $b = -1.91, p = .060$ ), with simple slopes producing a similar pattern to Study 1. Paolini et al. suggest that the lack of a significant p-value may result from the low power produced by their sample size. Note also that this experiment once again used a bipolar measure of valenced contact, limiting the interpretability of its results.

In Experiment 2, Paolini et al. replaced the television-mediated contact with an imagined contact manipulation similar to that used by Birtel and Crisp (2012). Seventy-six American students were prompted to engage in either positive or negative interactions with an illegal immigrant, followed by open-ended interaction content questions to strengthen the manipulation. Following their imagined interactions, participants completed a measure of perceived contact valence (interaction pleasantness), as well as measures of category salience, past contact quantity, and past contact quality (a combined score based on two items: “have you ever had any friends/family members who are illegal immigrants?”, and,

“how close was your closest friendship/family relationship with an illegal immigrant?”). Paolini et al. report a successful manipulation check, with participants in the positive condition reporting the interaction as significantly more pleasant ( $M = 4.74$ ,  $SD = 1.27$ ) than those in the negative condition ( $M = 2.21$ ,  $SD = 1.34$ ). Significant main effects of contact valence on category salience were found for all three interaction models, but the only significant interaction effect was between contact valence and past contact quantity ( $b = -0.32$ ,  $p = .020$ ), though the authors also report an interaction between contact valence and the presence of past close contact as approaching significance ( $b = -0.54$ ,  $p = .077$ ).

Finally, in their third experiment, Paolini et al. replicated the imagined contact manipulation from Experiment 2 with a sample of 91 Turkish Cypriots, who were asked to imagine positive and negative interactions with Greek Cypriots. The follow-up questionnaire included measures of contact valence (interaction pleasantness), past positive and negative contact (adapted from the items used by Barlow et al., 2012), overall past contact quality, exposure to positive and negative storytelling about the outgroup, direct and extended cross-group friendships, and category salience. As in previous studies, significant main effects of contact valence on category salience were found for all moderation models. However, the only significant interaction effects were found between contact valence and direct cross-group friendships ( $b = -0.58$ ,  $p = .024$ ), and exposure to positive outgroup stories ( $b = -0.77$ ,  $p = .042$ ), with a trend for the interaction between contact valence and exposure to negative outgroup stories ( $b = 0.29$ ,  $p = .057$ ). As in their previous studies, the simple slopes for all moderation models followed the predicted pattern. For example, participants with more cross-group friendships showed less effect of contact valence on category salience – a buffering effect.

Paolini et al.’s results are consistent in pattern, but limited in usefulness. The general lack of significant interaction effect limits the conclusions that can be drawn from their

findings, as does the tendency in all but the final experiment to operationalise contact valence as a single bipolar scale. However, the consistency of the buffering pattern (I would hesitate at this stage to call it a full effect) suggests the need for further investigation, particularly given that it contradicts the predictions one would draw from a purely salience-based account. Put simply, the salience and emotion-processing hypotheses predict that unexpected information (e.g., positive contact following previous negative contact) should be most impactful, because it deviates from the norm. This is exemplified by Birtel and Crisp's (2012) finding that negative contact augments the beneficial effect of positive contact on anxiety and contact intentions. Conversely, the perceived fit hypothesis predicts that unexpected information should be less impactful, because it does not conform to pre-existing stereotypes. This in turn is demonstrated by Paolini et al.'s (2014) studies, which indicate that prior positive contact buffers against the detrimental effects of negative contact on category salience. Although the fundamental contradiction between the results of Paolini et al. (2014) and Birtel and Crisp (2012) certainly demands attention, at present the empirical evidence for either side is extremely limited. The focus of my thesis is therefore, in the first instance, to expand the body of empirical data in which the buffering and augmentation effects (or indeed alternative interaction effects) may be tested.

## **Research Summary and Thesis Aims**

### **Summary and conclusions from literature review**

In reviewing the evidence for the effects of negative intergroup contact, I have arrived at several broad conclusions. Firstly, there is a general lack of empirical research which includes negative contact as a discrete variable. Although the imbalance caused by this positivity bias is beginning to be redressed, there remains little in the way of replication or methodological standardisation. Secondly, much of the literature that does deal with negative

contact exhibits flaws in its operationalisation. In particular, the majority of the existing negative contact literature deals with the question of valence asymmetry, that is, whether negative contact exerts a stronger influence on outgroup attitudes than positive contact. Several authors (Barlow et al., 2012; Bekhuis et al., 2013; Dhont & Van Hiel, 2009; Paolini et al., 2014, 2010) make the claim that whilst positive contact may be more frequent, negative contact is more impactful on levels of prejudice. However, not only is the statistical evidence for this mixed, but the methods used to compare the effect sizes of positive and negative contact are often flawed. With few exceptions (Graf et al., 2014; Paolini et al., 2010), studies of the positive/negative asymmetry of contact have employed self-report measures of valenced contact. Whether relating to specific contact experiences, or general perceptions of contact valence, these self-report measures are likely to be biased by the very processes that may drive the valence asymmetry effect (e.g., valence biases in salience, memory, attention etc). This being said, it is difficult to conceptually (let alone methodologically) disentangle these biases from what might be considered a 'true' valence asymmetry effect, and even questionable whether such a distinction makes theoretical sense.

Thirdly, the methodological inconsistencies in negative contact research are exacerbated by a lack of theoretical foundation. When predicting or interpreting apparent valence asymmetry in contact, some authors refer to the general psychological negativity effect (Barlow et al., 2012; Graf et al., 2014). The negativity effect literature certainly provides a rich array of theoretical mechanisms by which to explain the psychological superiority of negative stimuli (for a review, see Baumeister et al., 2001), and it is entirely plausible that one or more of these mechanisms may contribute to the effects of negative contact. However, certain key assumptions of several general negativity theories may not be applicable to contact scenarios. In particular, the salience (Kellermann, 1984), informativeness (Skowronski & Carlston, 1989), and extremity (Fiske, 1980) accounts of the

negativity effect rely on the idea that positive stimuli are generally more frequent in our day to day existence, and thus provide a normative ‘background’ or ‘neutral point’, against which negative stimuli will stand out. Whilst there is fairly consistent evidence to suggest that positive contact is more frequent than negative contact (Barlow et al., 2012; Bekhuis et al., 2013; Dhont & Van Hiel, 2009; Paolini et al., 2014, 2010; Graf et al., 2014), in the context of intergroup attitudes frequency may not necessarily lead to positive outgroup behaviours being perceived as the norm.

Unlike negativity effects in other domains, normative expectations about outgroup members are not set by pure statistical comparison of positive and negative experiences. For example, Paolini et al. (2010) argue that the self-categorisation processes involved in group formation lead outgroup behaviour to be perceived less objectively, and more in terms of stereotypic ‘fit’. The existence of a bias towards stereotype-confirming stimuli suggests that if attitudes towards a particular outgroup are negative, negative outgroup information will receive preferential processing. In terms of simple effect size comparisons, the category salience hypothesis is largely compatible with other theoretical positions drawn from the general negativity effect literature in terms of direction of predicted effects (i.e., it predicts stronger effects of negative contact). However, as an alternative theoretical framework for interpreting negative contact effects, Paolini et al.’s category salience account is thus far the only position to have received empirical support (Paolini et al., 2014, 2010). Furthermore, its predictive compatibility with general negativity-based explanations is lost when considering valenced contact in terms of the interaction of positive and negative contact, and this leads me to the final conclusion from my literature review of valenced contact.

The interaction between positive and negative contact has, to my knowledge, only been empirically tested in four studies so far (Birtel & Crisp, 2012; Christ et al., 2008; Dhont

& Van Hiel, 2009<sup>3</sup>; Paolini et al., 2014), with inconsistent results. Despite this lack of attention and evidential consistency, I consider the interaction approach to be a valuable direction for valenced contact research, for three reasons. Firstly, the additive approach to the effects of positive and negative contact is overly simplistic, theoretically questionable, and at least partially contradicted by the available evidence. Secondly, the interaction approach avoids some of the issues of measurement bias associated with questions of valence asymmetry, because testing non-additive interaction effects does not strictly require a standardised, valence-independent unit of contact. This methodological flexibility contributes to the third and final reason for studying valenced contact interactions. To wit, the interaction approach provides greater potential for developing a theoretical framework to explain the effects of negative contact. In terms of valence asymmetry, the informativeness, extremity, general salience and group category salience accounts all predict the same thing – the dominance of negative contact. However, when considering interaction effects, these various theories produce discriminable (and thus eminently testable) predictions.

For example, the general salience hypothesis would predict that an individual who has had frequent negative outgroup experiences should view positive contact as more surprising, less normative, and thus more informative and impactful than an individual who has had few negative outgroup interactions (the pattern of results observed by Birtel & Crisp, 2012). By contrast, the group category salience account might predict that someone who experiences frequent negative contact would form stronger negative associations with the outgroup category. This would reduce their perceived fit for positive contact, and thus its effect on

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<sup>3</sup> Dhont and Van Hiel (2009, study 2) report a non-significant interaction between positive and negative contact in their aforementioned study of Belgian nationals' attitudes towards immigrants.

outgroup attitudes compared to someone with fewer negative contact experiences. This ‘poisoning’ effect of negative contact provides a diametrically opposed prediction to the positive augmentation effect described above. Considering the alternative moderation, someone with frequent positive contact might be expected to view subsequent negative contact as more salient, and thus more harmful to their outgroup attitudes than someone with positive outgroup experiences. Alternatively, a high level of positive contact might reduce the stereotypic fit of negative contact, reducing its effect on prejudice (as demonstrated by Paolini et al., 2010). The apparent incompatibility of these theoretical approaches provides an intriguing theoretical and empirical challenge, one which is only intensified by the existence of (albeit limited) supporting evidence for both positions. Based on this evidence, and the theoretical explanations discussed above, I have generated two pairs of competitive, mutually exclusive hypotheses regarding the interaction of positive and negative contact.

### **Competitive Hypotheses 1:**

*1a. The buffering hypothesis:* positive contact moderates the relationship between negative contact and attitudes such that more positive contact reduces the effects of negative contact.

*1b. The positive augmentation hypothesis:* negative contact moderates the relationship between positive contact and attitudes such that more negative contact increases the prejudice-reducing effects of positive contact.

### **Competitive Hypotheses 2:**

*2a. The poisoning hypothesis:* negative contact moderates the relationship between positive contact and attitudes such that more negative contact reduces the effects of positive contact.

*2b. The negative augmentation hypothesis:* positive contact moderates the relationship between negative contact and attitudes such that more positive contact increases the effects of negative contact.

It should be noted that the mutual exclusivity in this pairing refers to simple statistical contradiction – positive contact cannot simultaneously increase (augment) and decrease (buffer) the effects of negative contact, for example. The fact that buffering/poisoning and positive/negative augmentation appear to require distinct explanatory mechanisms is a question that requires attention. However, of more pressing importance is the fact that, at present, I have found only four studies which have directly assessed the interaction of positive and negative contact (Birtel & Crisp, 2012; Christ et al., 2008; Dhont & Van Hiel, 2009; Paolini et al., 2014), amongst which only Christ, Ullrich, and Wagner (2008) and Dhont and Van Hiel (2009) tested attitudinal dependent variables (Birtel and Crisp tested interaction effects on intergroup anxiety and future contact intentions, whilst Paolini et al. investigated valenced contact in relation to category salience), and only Birtel and Crisp (2012) and Paolini et al. (2014) found significant interaction effects. A key priority for my thesis is therefore to build a consensus on the existence and nature of valenced contact interaction effects.

### **Aims of this thesis**

Based upon my review of the existing negative contact literature, my aims for this thesis are twofold:

- 1- To increase the volume of empirical data on the effects of negative contact, in a variety of social contexts, and using a variety of experimental and non-experimental methods.

- 2- To test, wherever possible, the interaction between positive and negative contact, with a view to resolving the apparent contradiction in the existing literature (through empirical disconfirmation or theoretical synthesis).

In pursuing these aims, I have employed a multimodal approach involving multiple analytic techniques: experimental data, cross-sectional survey data, longitudinal survey data, and multilevel survey data. The experimental studies focus on testing the effects of discrete valenced contact experiences using different contact formats: imagined contact (Experiment 1), extended contact (Experiment 2), and direct contact (Experiment 3). The survey studies focus on testing interaction effects using a variety of valenced contact measurement methods including both single-item general measures (as used by Barlow et al., 2012) and multi-item experience-based measures. Finally, the studies are carried out in a variety of social contexts: former Yugoslavia (Study 1), Northern Ireland (Studies 2 and 3), the UK (Study 4, Experiments 1 and 3) and Spain (Experiment 2).

## CHAPTER TWO: CROSS-SECTIONAL TESTS OF VALENCED CONTACT INTERACTIONS

### Study 1

My first study consisted of secondary analysis of data collected by Biro et al. (2004), who had measured valenced contact, but had not subjected those measures to detailed analysis. Biro et al. used this data set to investigate the attitudes of Bosniaks, Croats and Serbs (in Croatia and Bosnia and Herzegovina) towards other national groups, the possibility of reconciliation, and war crime tribunals following the conflict in former Yugoslavia.<sup>4</sup>

The conflict in former Yugoslavia (centred around the Yugoslav Wars between 1991 and 1995) primarily involved Serbs fighting against Bosniaks and Croats, although in Bosnia there was also conflict between Bosniaks and Croats, and intragroup violence between rival Bosniak factions. The wars marked the bloodiest conflict in Europe since the Second World War, and involved numerous incidents of war crimes and ethnic cleansing. The death-toll from the wars has been estimated at around 140,000 (International Center for Transitional Justice, 2009), and, in the years since, inter-ethnic tensions have remained high amongst the groups involved. Prior to the wars, the cities from which respondents in this study were recruited had been marked by peaceful coexistence for many years with relatively high levels of social integration. For example, Biro et al. (2004) reported data from the 1991 national census suggesting that 36% of all marriages in Bosnia and Herzegovina involved mixed nationalities. However, following the ethnic cleansing of the Muslim Bosniak population in

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<sup>4</sup> Biro et al.'s (2004) use of the questionnaire measures in Study 1 was primarily descriptive. They provided a detailed interpretation of the percentage distribution of responses to all survey items, giving historical and political context to the data while considering national and geographical differences. They also tested for personality-type predictors of several sub-components of a multi-item measure of ethnocentrism, authoritarianism and nationalism. Although Biro et al. (2004) did discuss all five variables used in the current study, they did not undertake analysis of the structural relationships between the variables (only descriptive patterns and group differences), hence the present analysis is novel.

Prijedor, as well as widespread violence in Mostar and Vukovar, the communities in these cities have become dramatically socially (and in the case of Mostar, physically) segregated.

My secondary analysis included only a subset of the questions posed in Biro et al.'s original survey. I focused on positive and negative contact, and readiness for reconciliation. I was not involved in the design of these valenced contact items, but made use of them here to establish initial evidence for the possible existence of interaction effects in a compelling intergroup context. I modelled the interaction between positive and negative contact on readiness for reconciliation, and, based on the simple slopes derived from that interaction term, tested my two pairs of moderation hypotheses (buffering vs. poisoning, positive vs. negative augmentation).

## **Method**

***Respondents and procedure.*** Respondents were drawn from communities of Serbs, Croats and Bosniaks in three cities in former Yugoslavia: Vukovar and Mostar in June 2001, and Prijedor in September 2001 (see Biro et al., 2004). Of the 1624 respondents from the original sample, 239 failed to complete the sections of the questionnaire relevant to the current study and were excluded from the analysis, leaving a final sample of 1385 (modal age-group = 30-39), which included 412 Croats (187 male, 225 female), 588 Bosniaks (319 male, 269 female) and 382 Serbs (208 male, 174 female, 3 missing). The respondents were recruited through a three-stage cluster procedure, in which the first stage units were city areas predominated by each nationality. The second stage units were households selected using a 'random walk' technique. Third stage units were single household members (randomly selected as the individual with a birthday closest to the interview date). The survey itself followed a standardised interview procedure, and was conducted by trained interviewers of the same nationality as the respondents.

## **Measures**

*Valenced contact.* Valenced contact was measured using two single items, assessing pre-war positive and negative contact. The wording of the items was as follows: "Have you, as a [Croat / Bosnian / Serb], had . . . (1) any positive experience with other nations before the war? . . . (2) any negative experience with other nations before the war?" (contrast coded as 1 = *Yes*, -1 = *No*).

*Readiness for reconciliation.* Readiness for reconciliation was measured using a single item, "In what way, in your opinion, could Croats, Bosnians and Serbs reconcile?" (1 = *One should never reconcile* to 5 = *We should make the first move*).

## **Results and discussion**

*Preliminary analyses.* Pre-war positive contact (77.3% 'yes') was significantly negatively correlated with both pre-war negative contact (13.6% 'yes'),  $r = -.07, p = .008$ , and readiness for reconciliation ( $M = 2.67, SD = 1.02$ ),  $r = .27, p < .001$ . Pre-war negative contact was also significantly negatively correlated with readiness for reconciliation,  $r = -.07, p = .013$ .

I tested for group differences in positive and negative contact based on nationality (Bosniak vs. Croat vs. Serb) and gender (Female vs. Male) using a Chi-squared test. This revealed significant associations between nationality and both positive contact,  $\chi^2(2) = 26.59, p < .001$ , and negative contact,  $\chi^2(2) = 16.85, p < .001$ . Bosniaks showed higher levels of positive contact (81.6%) than Serbs (79.4%) or Croats (69.2%), whilst Croats reported higher levels of negative contact (16.6%) than Serbs (15.3%) or Bosniaks (8.8%). There was no significant effect of gender on positive contact,  $\chi^2(1) = 3.06, p = .08$ , or negative contact,  $\chi^2(1) = 0.37, p = .54$ .

In addition, I carried out a 3 (Nationality: Serb vs. Bosniak vs. Croat) X 2 (Gender: Female vs. Male) between-subjects analysis of variance (ANOVA) with readiness for reconciliation as the dependent variable. This revealed a significant main effect of nationality,  $F(2, 1570) = 30.19, p < .001, \text{partial } \eta^2 = .037$ , which was qualified by a significant interaction between gender and nationality,  $F(1, 1570) = 5.38, p = .005, \text{partial } \eta^2 = .007$ . The difference in readiness for reconciliation between Serb males ( $M_{\text{male}} = 2.82, SD = 1.06$ ) and females ( $M_{\text{female}} = 3.11, SD = 0.97, p = .004, d = 0.29$ ) was greater than the difference between Bosniak males ( $M_{\text{male}} = 2.66, SD = 1.01$ ) and females ( $M_{\text{female}} = 2.54, SD = 0.97, p = .135, d = 0.12$ ), or Croat males ( $M_{\text{male}} = 2.41, SD = 1.00$ ) and females ( $M_{\text{female}} = 2.53, SD = 0.97, p = .154, d = 0.12$ ).

***Structural relationships and moderation results.*** In this and all subsequent studies, I used MPlus (version 6.1; Muthén & Muthén, 1998-2010) to carry out path analysis in order to investigate the hypothesised structural relationships between my variables. In all structural equation models, goodness of model fit was judged according to the criteria recommended by Hu and Bentler (1999). All model comparisons were made using the Satorra-Bentler chi-squared difference test (Satorra & Bentler, 2001). Given the group differences identified above, I first tested for structural relationship invariance across national groups. I ran a multigroup model in which positive contact, negative contact, age, gender, educational level and employment status were regressed on to readiness for reconciliation, grouping respondents by nationality. I allowed each structural path to be freely estimated. Using the corrected chi-square difference test (Satorra & Bentler, 2001), I compared the fit of this unconstrained model (which was just identified) to a second model in which the structural paths for each group were constrained to equality,  $\chi^2(12) = 47.82, p < .001, \chi^2/df = 3.99, CFI = .75, RMSEA = .08 [.06, .10], SRMR = .03$ . This test revealed a significant drop in model fit between the unconstrained and constrained models,  $\Delta\chi^2(12) = 47.82, p < .001$ , suggesting a

lack of structural invariance across groups. Since the data did not demonstrate structural invariance across groups, I analysed each national group separately in the subsequent multi-group models.

In order to determine the main effects of valenced contact and demographic covariates on readiness for reconciliation, I used the coefficients from the unconstrained multigroup model. I then tested the interaction effects between positive and negative contact in a separate model. These models revealed significant main effects of positive contact on readiness for reconciliation for all three national groups. Negative contact was significantly negatively related to readiness for reconciliation amongst Croats only (see Table 2). The interaction term for positive\*negative contact was significantly positively related to readiness for reconciliation for Serbs only. Decomposing the simple slopes revealed that negative contact was only significantly negatively related to readiness for reconciliation for participants reporting no positive contact,  $b = -.44$ ,  $SE = .12$ ,  $p = .001$ , and not for respondents who reported having positive contact,  $b = .12$ ,  $SE = .07$ ,  $p = .11$  (for a graph of these simple slopes, see Figure 1), suggesting a buffering effect. When I treated negative contact as the moderator, the effect of positive contact on readiness for reconciliation was significant for respondents who reported having and not having negative contact, but the relationship was significantly stronger when negative contact was present,  $b = .82$ ,  $SE = .13$ ,  $p < .001$ , when compared to absent,  $b = .26$ ,  $SE = .08$ ,  $p = .001$ . This pattern of results supports the positive augmentation hypothesis (although see discussion below regarding the lack of statistical discriminability between the buffering and positive augmentation hypotheses in cross-sectional data).

Table 2. Path Coefficients for Multigroup Comparison of Valenced Contact and Demographic Covariates on Readiness for Reconciliation (Study 1)

| Independent Variable | Croats   |           | Bosniaks |           | Serbs    |           |
|----------------------|----------|-----------|----------|-----------|----------|-----------|
|                      | <i>b</i> | <i>SE</i> | <i>b</i> | <i>SE</i> | <i>b</i> | <i>SE</i> |
| Positive contact     | .20***   | .05       | .35***   | .04       | .35***   | .07       |
| Negative contact     | -.28***  | .05       | .10      | .07       | -.01     | .07       |
| Interaction          | -.05     | .05       | .08      | .08       | .28***   | .07       |
| Age                  | -.03     | .03       | -.01     | .03       | .07      | .04       |
| Gender               | .04      | .09       | -.15     | .08       | .14      | .10       |
| Education            | .09*     | .05       | .02      | .04       | .02      | .05       |
| Employment           | -.06     | .10       | -.23**   | .08       | -.28*    | .11       |

\*\*\*  $p < .001$ , \*\*  $p < .01$ , \*  $p < .05$ , regression coefficients are unstandardised. *SE* = standard error.

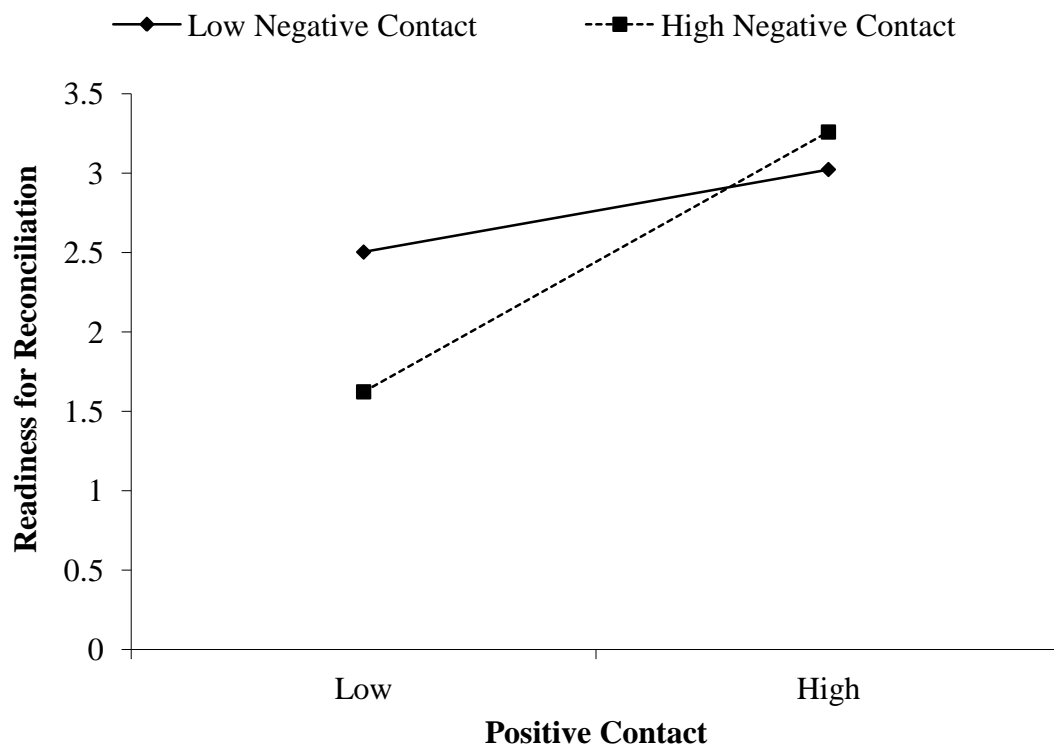


Figure 1. Simple slopes of positive and negative contact on readiness for reconciliation (Study 1)

The results of Study 1 demonstrated that the most consistent predictor of readiness for reconciliation was positive contact, which emerged as a positive and significant predictor across all three national groups. Negative contact only significantly (and negatively) affected readiness for reconciliation amongst Croats. Croats were also the group with the highest

mean levels of negative contact, which may indicate floor effects amongst the other national groups (i.e., such low levels of negative contact as to have little or no effect on their readiness for reconciliation). This issue may have been exacerbated by the binary coding of responses on the valenced contact questions, limiting the variability of responses open to the respondents. Although this data set provided a valuable first pass at the question of valenced contact interactions in a rarely-studied social context, in subsequent investigations I have prioritised the use of more granular, Likert-scale measures of positive and negative contact.

The only significant interaction effect was found for Serbs (who interestingly also showed the highest overall levels of readiness for reconciliation). Solving the simple slopes for this interaction revealed that negative contact had a weaker negative effect on readiness for reconciliation when positive contact was present (in line with the buffering hypothesis), and/or that positive contact was more effective at increasing readiness for reconciliation when negative contact was present (the positive augmentation hypothesis). Unfortunately, the nature of cross-sectional moderation analysis means that these two processes - buffering and positive augmentation - are statistically indistinguishable in the current data. I addressed this issue of statistical equivalence in Study 3 using a longitudinal study design to separately test the buffering and positive augmentation hypotheses. For now Study 1 serves solely to provide initial evidence for the existence of valenced contact interaction effects. The results are limited by their group-specific nature (both in terms of interactions and main effects of negative contact), the low resolution of the contact measures, and the cross-sectional design. Based upon these limitations, the main aim of Study 2 was to replicate the findings from Study 1 using more granular measures of valenced contact in a different social context, namely Northern Ireland.

## Study 2

In my second study, I sought to replicate the findings from Study 1, once again using a pre-existing data set in which valenced contact had been measured, but not analysed. In order to improve the generalisability of my findings, I used contact and attitude data from a different social setting - Northern Ireland. The ethno-religious conflict in Northern Ireland has a long history (see McLernon & Cairns, 2003), marked by intergroup tensions and violence. The conflict can be broadly traced to disagreement over the reunification of the Republic of Ireland with Northern Ireland, with those in favour of reunification predominantly identifying as Catholic, and those who want Northern Ireland to remain under British rule primarily identifying as Protestant (see Moxon-Browne, 1991). The conflict escalated during the 1960s with a sustained period of violence and British military intervention known as 'the Troubles'. Violence during the Troubles has been estimated to account for more than 3600 deaths, 16 000 injuries, 16 000 charges of terrorist offences, 34 000 shootings and 14 000 bombings (Fay, Morrissey, & Smyth, 1999). Despite continued efforts towards peace and reconciliation (Jarman, 2004), Northern Ireland today is still characterised by intergroup tension and distrust (Kenworthy et al., 2015).

The respondents in this study were recruited from six towns in Northern Ireland with varying levels of residential segregation. This approach was intended to provide a range of contact experiences throughout the sample, although it is likely that the majority of these respondents will have had at least some experience of intergroup violence, either direct or indirect (see Hayes & McAllister, 2005). As in Study 1, the goal was to investigate main effects and interactions for positive and negative contact, this time with outgroup attitudes as the dependent variable. Although the source survey only included dedicated measures of general negative contact, I was able to use cross-group friendships as a reasonable proxy

measure of positive contact. Cross-group friendships are clearly a more specific (and potentially more intense) measure of positive intergroup contact than are more general instances of positive outgroup interactions (e.g., greeting an outgroup member), so any comparison of effect sizes between positive and negative contact will be limited in its validity. However, the unmatched valenced contact measures do not preclude my testing for interaction effects.

## **Method**

***Respondents and procedure.*** This study used data drawn from a cross-sectional survey of cross-community perceptions in Northern Ireland (Schmid, Hewstone, Tausch, Cairns, & Hughes, 2009, Study 1)<sup>5</sup>. A sample of 2000 respondents was drawn randomly from six towns in Northern Ireland (Ballymena, Coleraine, Limavady, Newry, Newtownards, and Strabane) in order to achieve a spread of ethno-religious mixing. Of the original sample, 1940 were retained for analysis after removing respondents who failed to complete the relevant sections of the survey ( $n = 8$ ), and those who were not born in Northern Ireland ( $n = 52$ ). The final sample ( $M_{\text{age}} = 45.4$ ,  $SD_{\text{age}} = 17.4$ , age range from 18 to 92 years) consisted of 970 Catholics (353 male) and 970 Protestants (390 male). Data collection was carried out by a professional survey organization between March and October 2007, during which time respondents were interviewed in their own homes by trained survey interviewers, using computer-aided personal interviewing (CAPI) software. Prior to the interviews, respondents were contacted first by letter, then by telephone to confirm their participation.

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<sup>5</sup> Schmid, Hewstone, Tausch, Cairns, and Hughes (2009, Study 1) analysed the following variables from the questionnaire: (unvalenced) intergroup contact, distinctiveness threat, similarity complexity, overlap complexity, ingroup bias, ethnoreligious identification, education level and income. None of these variables were used in the current study, and none of the variables in the current study were used by Schmid et al. (2009). Schmid et al. (2009) sought to test the role of social identity complexity as a mediating variable between the independent variables of intergroup contact and distinctiveness threat, and the dependent variable of ingroup bias. There is therefore no overlap in the constructs or hypotheses between Schmid et al. (2009) and the current study.

**Measures.** The study used measures of positive and negative contact with, and attitudes towards ethno-religious outgroup members (Catholics or Protestants). These measures were drawn from a larger selection of items included in the survey.

*Positive contact (cross-group friendships).* Positive contact was measured using a single question on cross-group friendships (R. N. Turner, Hewstone, et al., 2007), "How many of your close friends are OUTGROUPERS?" (1 = *none*, to 7 = *all*).

*Negative contact.* Negative contact was measured using a three-item scale derived from C. W. Stephan et al. (2000), each within the stem: "In general, when you had contact with OUTGROUPERS in the past, how often were you...", "...made to feel unwanted", "verbally abused", and "intimidated" (1 = *never*, to 5 = *very often*). These three items formed a reliable scale of negative contact ( $\alpha = .91$ ) and were combined to produce a single negative contact score.

*Outgroup attitudes.* Outgroup attitudes were measured using a 'feeling thermometer' (Converse, Dotson, Hoag, & McGee, 1980), which was described to respondents as follows, "How do you feel about OUTGROUPERS in general? Please rate OUTGROUPERS on a thermometer that runs from zero (0) to a hundred (100) degrees. The higher the number, the warmer or more favourable you feel towards OUTGROUPERS. The lower the number, the colder or less favourable you feel. If you feel neither warm nor cold towards OUTGROUPERS, rate them at 50. You can choose any number between 0 and 100."

## **Results and discussion**

*Preliminary analyses.* Positive contact (cross-group friendships) was significantly negatively correlated with negative contact,  $r = -.13$ ,  $p < .001$ , and positively correlated with outgroup attitudes,  $r = .30$ ,  $p < .001$ . Negative contact was significantly negatively correlated with outgroup attitudes,  $r = -.32$ ,  $p < .001$ .

To test for possible group effects, I computed a 2 (Religious group: Catholic vs. Protestant) X 2 (Gender: Female vs. Male) between-subjects Multivariate Analysis of Variance (MANOVA) on the three key variables (positive contact, negative contact, and outgroup attitudes). The effect of religious group emerged for positive contact,  $F(1, 1936) = 14.03, p < .001, \text{partial } \eta^2 = .007$ , and outgroup attitudes  $F(1, 1936) = 85.72, p < .001, \text{partial } \eta^2 = .042$ , whilst the effect of gender only occurred for negative contact  $F(1, 1939) = 20.71, p < .001, \text{partial } \eta^2 = .011$ . There were no interaction effects. Exploring the direction of these group effects showed that Catholics reported more positive contact ( $M_{\text{Catholics}} = 2.92, SD = 1.38$  vs.  $M_{\text{Protestant}} = 2.69, SD = 1.28, p < .001, d = 0.17$ ) and had more positive outgroup attitudes ( $M_{\text{Catholics}} = 73.75, SD = 19.48$  vs.  $M_{\text{Protestants}} = 65.51, SD = 19.97, p < .001, d = 0.42$ ) than Protestants. Males reported higher levels of negative contact ( $M = 1.64, SD = 0.90$ ) than females ( $M = 1.47, SD = 0.78, p < .001, d = 0.20$ ). However, given the large sample size, it must be noted that only the group difference in outgroup attitudes produced anything larger than a small effect size.

***Structural relationships and moderation results.*** Because the measurement model only contained a single latent variable derived from three highly uniform items, the initial confirmatory factor analysis produced perfect fit. I tested for metric invariance across religious groups by comparing the chi-square statistic for a model wherein the factor loadings of the three negative contact manifest variables were allowed to be freely estimated across the two sample groups (model 1a) to one in which the factor loadings were constrained to equality (model 1b, all fit statistics and model comparisons shown in Table 3). This produced no significant change in model fit, indicating that the factor loadings were statistically equivalent for both Catholic and Protestant participants. Next, I tested for invariance of structural paths across religious groups by comparing the freely estimated structural model (model 2a) to one in which the structural paths from positive contact and negative contact to

outgroup attitudes were constrained to equality across groups (model 2b). This produced a significant drop in model fit (see Table 3), suggesting that the relationship between valenced contact and attitudes differed for Catholics and Protestants.

Due to the lack of structural invariance between religious groups, I generated my final structural models separately for Catholics and Protestants. These structural models for both Catholics (model 3) and Protestants (model 4) produced acceptable model fit (see Table 3), and were used to test the main effects of valenced contact (and demographic covariates) on outgroup attitudes. In order to test the interaction between positive and negative contact, a final model was generated for each participant group, which included a latent positive-negative contact interaction term as an additional predictor of outgroup attitudes. The latent variable interaction terms in these models (and in all subsequently reported models) were calculated using Klein and Moosbrugger's (2000) maximum likelihood estimation approach.

*Table 3. Model Fit and Chi-squared Difference Statistics Including Measurement and Structural Models (Study 2)*

| Model     | Model Fit   | Model Comparison | $\Delta \chi^2$ (df) | <i>p</i> |
|-----------|---|------------------|----------------------|----------|
| <b>1a</b> | Just identified   |                  |                      |          |
| <b>1b</b> | $\chi^2(2) = 2.51, p = 0.286, \chi^2/df = 1.25, CFI = 1, RMSEA = 0.02, SRMR = 0.015$      | 1a vs. 1b        | 2.51 (2)             | 0.286    |
| <b>2a</b> | $\chi^2(22) = 45.53, p = 0.002, \chi^2/df = 2.07, CFI = 0.99, RMSEA = 0.03, SRMR = 0.013$ |                  |                      |          |
| <b>2b</b> | $\chi^2(24) = 77.24, p < .001, \chi^2/df = 3.22, CFI = 0.98, RMSEA = 0.05, SRMR = 0.029$  | 2a vs. 2b        | 28.44 (2)            | < .001   |
| <b>3</b>  | $\chi^2(10) = 11.15, p = 0.346, \chi^2/df = 1.11, CFI = 1, RMSEA = 0.01, SRMR = 0.007$    |                  |                      |          |
| <b>4</b>  | $\chi^2(10) = 33.29, p < .001, \chi^2/df = 3.33, CFI = 0.99, RMSEA = 0.05, SRMR = 0.015$  |                  |                      |          |

Amongst Catholics, positive contact was significantly positively associated with outgroup attitudes,  $b = 1.88, SE = .48, p < .001$ . Negative contact was significantly negatively

associated with outgroup attitudes,  $b = -6.30$ ,  $SE = 0.87$ ,  $p < .001$ ). Age,  $b = 0.14$ ,  $SE = 0.04$ ,  $p < .001$ , and educational level,  $b = 3.38$ ,  $SE = 0.83$ ,  $p < .001$ , were associated with more favourable outgroup attitudes. The moderation model yielded a significant interaction between positive and negative contact,  $b = 2.02$ ,  $SE = 0.63$ ,  $p = .001$ . Simple slopes analysis based on the latent variable interaction coefficient revealed that at high levels of positive contact, the effect of negative contact on outgroup attitudes was smaller,  $b = -9.44$ ,  $SE = 1.07$ ,  $p < .001$ , than at low levels of positive contact,  $b = -15.00$ ,  $SE = 2.55$ ,  $p < .001$ . This pattern of relationships is consistent with a buffering effect. Conversely, at high levels of negative contact, the effect of positive contact on outgroup attitudes was larger,  $b = 3.60$ ,  $SE = 0.75$ ,  $p < .001$ , than at low levels of negative contact,  $b = .021$ ,  $SE = 0.69$ ,  $p = .759$ , indicating a positive augmentation effect of negative contact (for a graph of the simple slopes for Catholic respondents, see Figure 2).

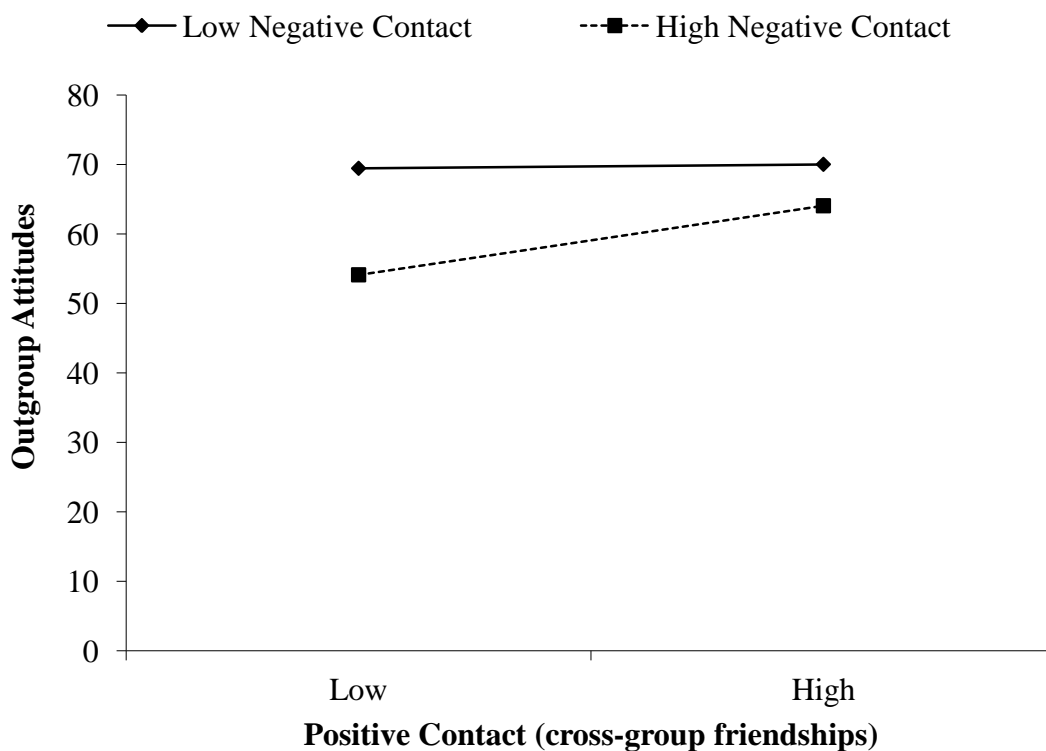


Figure 2. Simple slopes of positive and negative contact on outgroup attitudes amongst Catholics (Study 2)

Amongst Protestants, the observed relationships were similar. Positive contact was significantly positively associated with outgroup attitudes,  $b = 5.59$ ,  $SE = 0.51$ ,  $p < .001$ , whereas negative contact was significantly negatively associated with outgroup attitudes,  $b = -6.40$ ,  $SE = 0.83$ ,  $p < .001$ . Age,  $b = .09$ ,  $SE = 0.03$ ,  $p = .010$ , and educational level,  $b = 3.85$ ,  $SE = 0.82$ ,  $p < .001$ , were both positively associated with more favourable outgroup attitudes. The moderation model yielded a significant interaction between positive and negative contact,  $b = 1.78$ ,  $SE = 0.58$ ,  $p = .002$ . Solving the simple slopes of the interaction showed that at high levels of positive contact, the effect of negative contact on outgroup attitudes,  $b = -8.09$ ,  $SE = 0.98$ ,  $p < .001$ , was smaller than at low levels of positive contact,  $b = -12.65$ ,  $SE = 2.24$ ,  $p < .001$ , indicating a buffering effect of positive contact. Conversely, at high levels of negative contact, the effect of positive contact on outgroup attitudes was larger,  $b = 7.15$ ,  $SE = 0.74$ ,  $p < .001$ , than at low levels of negative contact,  $b = 4.15$ ,  $SE = 0.66$ ,  $p < .001$ , indicating an augmentation effect of negative contact (for a graph of the simple slopes for Protestant participants see Figure 3).

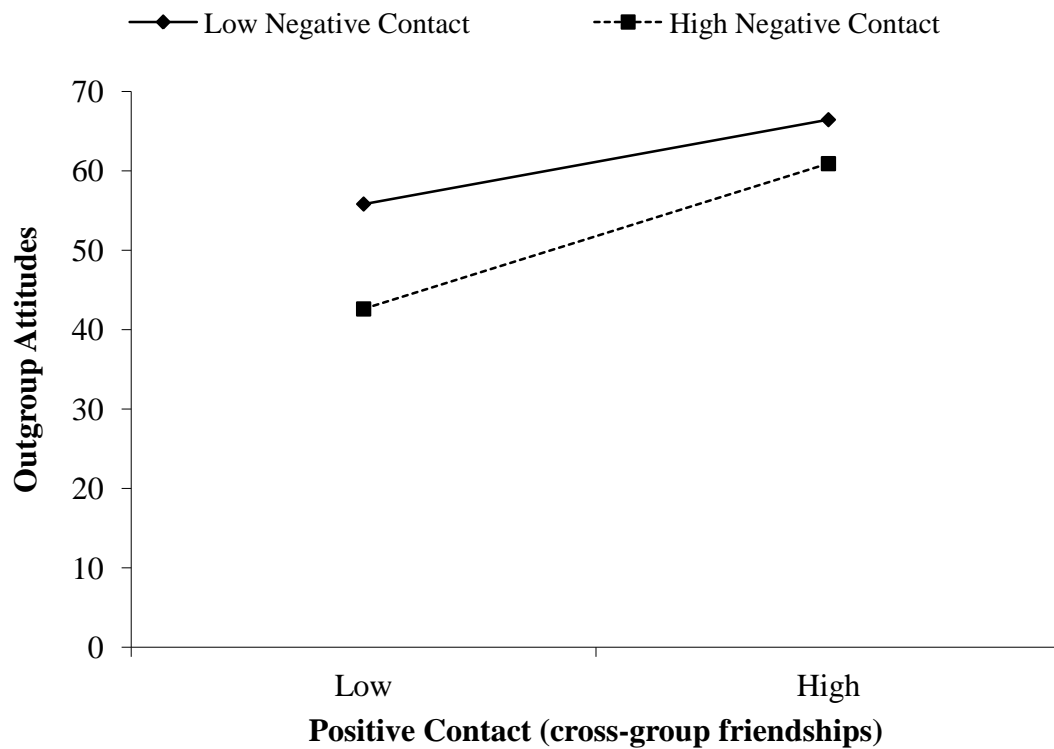


Figure 3. Simple slopes of positive and negative contact on outgroup attitudes amongst Protestants (Study 2)

The results of Study 2 replicated the interaction effect found amongst Serbian respondents in Study 1. Furthermore, despite significant differences in the structural relationships between Catholic and Protestant participants, both groups demonstrated the same pattern of valenced contact interactions. Specifically, at high levels of positive contact, the effect of negative contact on outgroup attitudes was smaller than at low levels of positive contact (i.e., a buffering effect), and/or at high levels of negative contact the effect of positive contact on outgroup attitudes was larger than at low levels of negative contact (i.e., a positive augmentation effect). Just as in Study 1, Study 2 was limited by its cross-sectional design, meaning that two theoretically discriminable processes (buffering and positive augmentation) are statistically indistinguishable. This once again limits the interpretability of the results, but at the very least the findings do support the existence of some kind of valenced contact interaction. In particular, it is notable that in Study 2 I found significant interaction effects for

respondents of both ethnoreligious sample groups. Given the lack of such cross-group consistency in Study 1, an ongoing question for this line of research is what factors affect the occurrence of valenced contact interactions (or a lack thereof) in different social groups or contexts.

At present, my only explanation for why Catholics, Protestants and Serbs have shown interaction effects whilst Croats and Bosniaks have not is some factor relating to the groups themselves. It is possible that differences in physical segregation, outgroup attitudes, social norms, or any number of other potentially influential variables could facilitate interaction effects in the Northern Irish context, or inhibit them in the former Yugoslavian samples. However, given that at this stage of the research project I have yet to establish evidence for the possible underlying mechanisms of valenced contact interactions, it is difficult to make any concrete hypothesis as to which specific group-level variables might be influential. Although I will periodically return to this question throughout the course of the thesis, identifying such contextual moderators is a task that is likely beyond the scope of the current project, given that my focus is primarily on determining the existence and profile of valenced contact interactions. With this in mind, Study 3 will remain in the Northern Irish setting, but will employ a longitudinal design to overcome the issues of discriminability that have hampered the cross-sectional studies discussed thus far.

Finally, it should be noted that the differences in measurement method between Studies 1 and 2 limit the validity of any direct comparison of their results. Specifically, the binary contact questions used in Study 1 remove any distinction between participants with frequent vs. infrequent (but still present) contact experiences. This fundamentally alters the interpretation of the results of Study 1 compared to Study 2. Whilst Study 2 demonstrates that the effects of negative contact are different at high vs. low levels of positive contact, Study 1 shows that (for Serbian respondents) the effects of negative contact are different for those

who have had positive contact vs. those who have not had positive contact. From the results of the two studies, it would seem that the interaction effect based on the difference between 'some positive/negative contact' and 'no positive/negative contact' is less prevalent than an interaction based on the difference between 'little positive/negative contact' and 'extensive positive/negative contact'.

## **CHAPTER THREE: LONGITUDINAL TESTS OF VALENCED CONTACT INTERACTIONS**

In Study 3, my goal was to move beyond the cross-sectional approach that had limited the interpretability of the interaction effects found in Studies 1 and 2. With this in mind, the data for Study 3 were drawn from a 2-wave longitudinal survey of contact and attitudes between Catholics and Protestants in Belfast, Northern Ireland. Using these data, I tested the buffering/negative augmentation hypotheses by modelling time 1 positive contact as a moderator of the relationship between time 2 negative contact and time 2 attitudes (i.e., determining whether past positive contact altered the effect of subsequent negative contact). In a separate model, I tested the poisoning/positive augmentation hypotheses by modelling time 1 negative contact as a moderator of the relationship between time 2 positive contact and time 2 attitudes (i.e., determining whether past negative contact altered the effect of subsequent positive contact). This presented an advantage over the cross-sectional moderation tests used in Studies 1 and 2, because it allowed me to more reliably infer the existence of separate interaction processes (e.g., buffering and positive augmentation), as the critical tests of these processes were drawn from separate models.

With a two-wave longitudinal data set such as this, it would also be possible to test an alternative moderation, for example the effect of time 1 positive contact on the relationship between time 1 negative contact and time 2 attitudes. However, this approach would suffer from the same limitation as my previous studies, because the interaction term would be derived from cross-sectional data (i.e., the interaction between time 1 positive and time 1 negative contact). The statistical separation of the tests for buffering and positive augmentation is made possible through the interaction of chronologically distinct measures of valenced contact.

The survey used in Study 3 also improves upon the measures of valenced contact used in previous studies by including a multi-item battery of questions referencing specific positive and negative contact experiences. This allowed me to use full latent variable modelling for both positive and negative contact, providing an advantage over previous single-item measures by presenting a richer description of contact experiences in the questionnaire, and by allowing estimation of error terms in the resulting structural equation model.

### Study 3

#### Method

***Respondents and procedure.*** The data in this study were drawn from a longitudinal survey of cross-community perceptions in Northern Ireland (Schmid, Tausch, & Hewstone, 2008)<sup>6</sup>. The original survey was carried out in four areas of Belfast matched as closely as possible on indices of social class, unemployment, and sectarian violence according to the 2001 Northern Ireland Census (Northern Ireland Statistics and Research Branch, 2002). The survey was conducted by a professional survey organisation, using face-to-face interviews supplemented by cards with question and response options. Two waves of data were collected, the first between March and July 2006, and the second between May and August 2007. The first wave consisted of 984 respondents ( $M_{\text{age}} = 51.7$ ,  $SD_{\text{age}} = 17.4$ ) including 437

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<sup>6</sup>Schmid et al. (2008) analysed the following variables from the questionnaire used in Study 3: positive intergroup contact, level of ethnoreligious segregation, exposure to violence, threats to physical safety, symbolic threat, ingroup bias and offensive action tendencies. Of these measures, only the three positive contact items were also used in the current study. Schmid et al. (2008) did not include the negative contact items in their analysis, and their study investigated the effects of level of ethnoreligious segregation on positive contact, exposure to violence, ingroup bias and perceptions of threat. Tausch et al. (2010) used the following variables from the same questionnaire: (unvalenced) intergroup contact with the ethnoreligious outgroup and racial minorities, and attitudes towards the ethnoreligious outgroup, the ethnoreligious ingroup, and racial minorities. The only one of these measures used in the current study was the feeling thermometer measure of ethnoreligious outgroup attitudes. The goal of Tausch et al.'s (2010) study was to investigate the effect of contact with a primary outgroup on attitudes towards secondary outgroups (the Secondary Transfer Effect). Given the content and hypotheses of the above studies, there is minimal construct overlap, and negligible conceptual overlap between the current study and prior use of the same data set.

Catholics (156 male) and 547 Protestants (225 male). The second wave consisted of 811 respondents ( $M_{\text{age}} = 50.0$ ,  $SD_{\text{age}} = 17.4$ ) including 427 Catholics (155 male) and 384 Protestants (151 male). The total number of matched respondents was 411 ( $M_{\text{age}} = 51.2$ ,  $SD_{\text{age}} = 16.7$ ), including 183 Catholics (60 male) and 228 Protestants (85 male).

**Measures.** In this study, I made use of measures of positive and negative intergroup contact with the ethno-religious outgroup, as well as intergroup attitudes. These items formed a subset of the questions included in the original survey.

*Valenced contact.* Valenced contact was measured using a seven-item scale derived from C. W. Stephan et al. (2000). Four items measured negative contact and three measured positive contact. Respondents were instructed as follows: "Please indicate how often you have experienced the following types of treatment from OUTGROUPERS:" followed by four negative contact items: "Been made to feel unwanted", "Been verbally abused", "Been intimidated", "Been threatened with harm"; and three positive contact items: "Been supported", "Been helped out", "Been made to feel welcome" (1 = *Never* to 5 = *Very Often*). These items were split into independent scales for positive and negative contact for the purposes of the analysis (see below for details of the exploratory factor analyses, and by-wave factor-structure analysis). Both the positive and negative contact scales showed good reliability across both time points ( $\alpha_{T1 \text{ positive contact}} = .84$ ,  $\alpha_{T2 \text{ positive contact}} = .88$ ,  $\alpha_{T1 \text{ negative contact}} = .87$ ,  $\alpha_{T2 \text{ negative contact}} = .89$ ).

*Outgroup attitudes.* Outgroup attitudes were measured using a 'feeling thermometer', identical to the scale used in Study 2.

## Results and discussion

*Preliminary analyses.* Descriptive statistics and intercorrelations between key variable are shown in Table 4<sup>7</sup>. For details of group differences, see footnote<sup>8</sup>.

*Selective attrition.* I tested for attrition following the steps outlined by Goodman and Blum (1996). As such, none of the variables predicted dropout; there were no mean differences between those who dropped out and those who stayed (Wilk's lambda  $\Lambda = .99$ ,  $F(6, 975) = 1.20$ ,  $p > .05$ , partial  $\eta^2 = .007$ ); there were no changes in variance (all variance comparisons  $p > .05$ ); and no changes in structural paths (all regression coefficient comparisons  $p > .05$ ). These analyses suggested that my data were missing at random (see Goodman & Blum, 1996). Therefore, I used the full information maximum likelihood (FIML) estimator in my subsequent structural equation models in order to preserve the complete T1 sample size of 984. FIML is a technique for obtaining accurate parameter

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<sup>7</sup> A single item, T1 negative contact item 1 ("Been threatened with harm") showed skewness outside the bounds of  $\pm 2$  as recommended by West, Finch, and Curran (1995) (skewness = 2.28, kurtosis = 5.10). Since this value was only marginally outside the acceptable range, and since the criteria violation only occurred at T1, the item was retained for analysis. All other items and time points fell within the bounds of  $\pm 2$  for skewness and  $\pm 7$  for kurtosis (skewness range = -0.88 - 1.84, kurtosis range = -0.61 - 3.65).

<sup>8</sup> To test for possible group differences in Study 3, I computed a 2 (Religious Group: Catholic vs. Protestant) X 2 (Gender: Female vs. Male) between-subjects MANOVA with outgroup attitudes, positive contact and negative contact as dependent variables. This analysis revealed significant main effects of religious group,  $F(3, 978) = 36.86$ ,  $p < .001$ , partial  $\eta^2 = .102$ , and gender,  $F(3, 978) = 23.71$ ,  $p < .001$ , partial  $\eta^2 = .068$ , as well as a significant interaction effect,  $F(3, 978) = 2.74$ ,  $p = .042$ , partial  $\eta^2 = .008$ . The main effect of religious group was found for positive contact,  $F(1, 980) = 7.85$ ,  $p = .005$ , partial  $\eta^2 = .008$ , negative contact,  $F(1, 980) = 34.32$ ,  $p < .001$ , partial  $\eta^2 = .034$ , and outgroup attitudes,  $F(1, 980) = 47.53$ ,  $p < .001$ , partial  $\eta^2 = .046$ . Catholics showed more favourable outgroup attitudes ( $M_{\text{Catholics}} = 72.75$ ,  $SD = 18.17$  vs.  $M_{\text{Protestant}} = 63.63$ ,  $SD = 18.87$ ,  $p < .001$ ,  $d = .49$ ), more positive contact ( $M_{\text{Catholics}} = 3.42$ ,  $SD = 0.98$  vs.  $M_{\text{Protestant}} = 3.20$ ,  $SD = 0.98$ ,  $p = .005$ ,  $d = .23$ ), and more negative contact ( $M_{\text{Catholics}} = 1.69$ ,  $SD = 0.83$  vs.  $M_{\text{Protestant}} = 1.46$ ,  $SD = 0.65$ ,  $p < .001$ ,  $d = .31$ ) than Protestants. The main effect of gender was also found for positive contact,  $F(1, 980) = 14.28$ ,  $p < .001$ , partial  $\eta^2 = .014$ , negative contact,  $F(1, 980) = 63.05$ ,  $p < .001$ , partial  $\eta^2 = .060$ , and outgroup attitudes  $F(1, 980) = 19.89$ ,  $p < .001$ , partial  $\eta^2 = .020$ . Females showed more favourable outgroup attitudes ( $M_{\text{female}} = 69.88$ ,  $SD = 18.36$  vs.  $M_{\text{male}} = 64.20$ ,  $SD = 19.74$ ,  $p < .001$ ,  $d = .30$ ), and positive contact ( $M_{\text{female}} = 3.38$ ,  $SD = 1.00$  vs.  $M_{\text{male}} = 3.15$ ,  $SD = 0.94$ ,  $p < .001$ ,  $d = .24$ ), whilst males reported having more negative contact higher than females ( $M_{\text{male}} = 1.77$ ,  $SD = 0.87$  vs.  $M_{\text{female}} = 1.43$ ,  $SD = 0.63$ ,  $p < .001$ ,  $d = .46$ ). All main effects produced small to medium effect sizes. The interaction between gender and religious group was found for positive contact,  $F(1, 980) = 4.48$ ,  $p = .035$ , partial  $\eta^2 = .005$ , and negative contact,  $F(1, 980) = 5.24$ ,  $p = .022$ , partial  $\eta^2 = .005$ . Female Catholics reported significantly higher levels of positive contact ( $M_{\text{female}} = 3.56$ ,  $SD = 0.98$ ) than male Catholics ( $M_{\text{male}} = 3.18$ ,  $SD = 0.93$ ,  $p < .001$ ,  $d = 0.40$ ), but there were no differences between male ( $M_{\text{male}} = 3.13$ ,  $SD = 0.94$ ) and female ( $M_{\text{female}} = 3.24$ ,  $SD = 1.00$ , Cohen's  $d = 0.11$ ) Protestants. Male Catholics also showed higher levels of negative contact ( $M_{\text{male}} = 2.00$ ,  $SD = 0.95$ ) than female Catholics ( $M_{\text{female}} = 1.52$ ,  $SD = 0.70$ ,  $p < .001$ ,  $d = 0.57$ ), as did male Protestants ( $M_{\text{male}} = 1.62$ ,  $SD = 0.76$ ) compared with female Protestants ( $M_{\text{female}} = 1.35$ ,  $SD = 0.54$ ,  $p < .001$ ,  $d = .41$ ). All interaction effects were of medium effect sizes.

estimates with partially missing data that is considered to be missing at random (Finkbeiner, 1979).

*Table 4. Inter-item Correlations, Means and Standard Deviations of Study Variables (Study 3)*

| Time Point | Variable                                  | Mean (SD)        | T1 (N = 984) |        |        | T2 (N = 411) |        |        |
|------------|---|------------------|--------------|--------|--------|--------------|--------|--------|
|            |   |                  | 1.           | 2.     | 3.     | 4.           | 5.     | 6.     |
| T1         | 1. Positive Contact<br>( $\alpha = .84$ ) | 3.30<br>(0.99)   | -            | -.20** | .42**  | .55**        | -.15** | .33*   |
|            | 2. Negative Contact<br>( $\alpha = .87$ ) | 1.56<br>(0.75)   |              | -      | -.23** | -.04         | .52**  | -.11*  |
|            | 3. Outgroup Attitudes                     | 67.68<br>(19.10) |              |        | -      | .30**        | -.07   | .30**  |
| T2         | 4. Positive Contact<br>( $\alpha = .88$ ) | 3.19<br>(0.95)   |              |        |        | -            | -.35** | .32**  |
|            | 5. Negative Contact<br>( $\alpha = .89$ ) | 1.65<br>(0.70)   |              |        |        |              | -      | -.31** |
|            | 6. Outgroup Attitudes                     | 67.81<br>(17.22) |              |        |        |              |        | -      |

*Note.* \* $p < .05$ . \*\* $p < .01$ . *SD* = Standard Deviation, shown in parentheses.

***Metric and structural invariance.*** Before adding the structural paths, I tested the model for measurement invariance across time points (see Cole & Maxwell, 2003), by comparing two factor models. The first factor model (1a, see Table 5) allowed the individual item parameters to be freely estimated across time points and participant group. The errors for individual manifest variables at time 1 were allowed to correlate with their corresponding errors at time 2 (see Little, Preacher, Selig, & Card, 2007). The second model (1b) constrained the factor loadings of the manifest variables to equivalence across time. I then compared the chi-squared statistics for the two models, which revealed a significant drop in model fit indicating that the factor structure at time 2 differed from time 1 (see Table 5 for fit

comparison statistics). In order to determine which factor loading might be causing the change in model fit, I carried out a series of single degree of freedom comparisons, each comparing the unconstrained model to a model in which only a single factor loading was constrained to equality across waves. This produced seven separate model comparisons, so the alpha level was adjusted accordingly ( $p = .007$ ). It emerged that only a single item ("Been made to feel unwanted"), was producing a significant drop in model fit at the adjusted alpha level. This item was therefore removed from the model, and the test for measurement invariance was re-run. This time, there was no significant drop in model fit between the 6-item unconstrained model (1c), and the 6-item constrained model (1d).. With this new 6-item model, I was therefore able to satisfactorily demonstrate measurement invariance across time.

*Table 5. Model Fit and Chi-squared Difference Statistics for Tests of Measurement and Structural Invariance (Study 3)*

| Model     | Model Fit  | Model Comparison | $\Delta \chi^2$ (df) | $p$    |
|-----------|--|------------------|----------------------|--------|
| <b>1a</b> | $\chi^2(105) = 191.02, p < .001, \chi^2/df = 1.82, CFI = 0.98, RMSEA = 0.04, SRMR = 0.037$ |                  |                      |        |
| <b>1b</b> | $\chi^2(115) = 242.66, p < .001, \chi^2/df = 2.11, CFI = 0.97, RMSEA = 0.04, SRMR = 0.052$ | 1a vs. 1b        | 41.94 (10)           | < .001 |
| <b>1c</b> | $\chi^2(84) = 122.22, p = 0.004, \chi^2/df = 1.45, CFI = 0.99, RMSEA = 0.03, SRMR = 0.035$ |                  |                      |        |
| <b>1d</b> | $\chi^2(92) = 136.65, p = 0.002, \chi^2/df = 1.49, CFI = 0.99, RMSEA = 0.03, SRMR = 0.042$ | 1c vs. 1d        | 13.64 (8)            | .092   |
| <b>1e</b> | $\chi^2(96) = 156.45, p < .001, \chi^2/df = 1.63, CFI = 0.98, RMSEA = 0.03, SRMR = 0.047$  | 1e vs. 1d        | 14.47 (4)            | .006   |
| <b>1f</b> | $\chi^2(95) = 141.62, p = 0.001, \chi^2/df = 1.49, CFI = 0.99, RMSEA = 0.03, SRMR = 0.043$ | 1f vs. 1d        | 4.83 (3)             | .184   |
| <b>1g</b> | $\chi^2(176) = 311.05, p < .001, \chi^2/df = 1.77, CFI = 0.97, RMSEA = 0.04, SRMR = 0.062$ |                  |                      |        |
| <b>1h</b> | $\chi^2(183) = 369.16, p < .001, \chi^2/df = 2.02, CFI = 0.96, RMSEA = 0.04, SRMR = 0.093$ | 1g vs. 1h        | 60.35                | < .001 |

I also tested for measurement invariance across participant groups by comparing the time-constrained metric invariance model (1d) to a model in which the positive and negative contact factor loadings were constrained to equality across group (1e). This produced a significant drop in model fit, so I carried out a series of four single degree of freedom comparisons, each comparing the time-constrained model (1d) to a model in which each of the factor loadings were in turn constrained to equality across groups. From this I identified a single item (“Been threatened with harm”) that was causing model misfit. I therefore created a new model (1f) in which all factor loadings were constrained to equality across time and group, except for the “Been threatened with harm” item, which was freely estimated across groups. Comparing this model to the time-constrained model (1d) produced no significant drop in model fit, allowing me to demonstrate partial metric invariance across groups, a minimum requirement when wanting to make meaningful group comparisons (Vandenberg & Lance, 2000).

Finally, I tested for structural invariance across participant groups. To test the effects of positive and negative contact on outgroup attitudes, T2 attitudes were regressed onto T1 positive and negative contact, T2 positive and negative contact, and the demographic covariates T1 age and gender. These structural paths were added to the partially constrained measurement model (along with autoregressive paths and within-wave correlations) and allowed to vary freely across groups to produce an unconstrained structural model (1g). This was compared to a model in which the structural paths were constrained across participant groups (1h). The model comparison produced a significant drop in model fit, suggesting a lack of structural invariance across groups. Because of this, all subsequent models and comparisons were calculated separately for Catholics and Protestants.

***Cross-lagged models.*** The contact-attitudes relationship has been shown to be bidirectional. Not only does contact predict attitudes, but attitudes also predict contact (see

Binder et al., 2009; Pettigrew, 1997; Swart, Hewstone, Christ, & Voci, 2011). For the purposes of this investigation, I was only interested in the forward path from contact to attitudes, but I needed to establish that this relationship did indeed exist in my data. For each participant group I therefore ran a full cross-lagged model which included both the forward (T1 positive and negative contact predicting T2 outgroup attitudes), and reverse (T1 outgroup attitudes predicting T2 positive and negative contact) structural paths, as well as autoregressive paths and demographic effects (T1 age and gender on all T2 variables).

The cross-lagged models for both Protestants (model 2a) and Catholics (model 3a) produced acceptable model fit according to the criteria proposed by Hu and Bentler (1999, see Table 6). Amongst Protestant respondents, I found significant autoregressive paths for positive contact,  $b = 0.63$ ,  $SE = 0.07$ ,  $p < .001$ , and negative contact,  $b = 0.79$ ,  $SE = 0.10$ ,  $p < .001$ , but not for outgroup attitudes. There were significant forward relationships between T1 positive contact and T2 outgroup attitudes,  $b = 5.51$ ,  $SE = 1.63$ ,  $p = .001$ , and between T1 negative contact and T2 outgroup attitudes,  $b = -6.96$ ,  $SE = 2.06$ ,  $p = .001$ . In line with my predictions, positive contact was thus predictive of more positive, and negative contact of more negative, attitudes over time.

*Table 6. Model Fit Statistics for Cross-Lagged and Forward Models (Study 3)*

| Model | Model Fit  |
|-------|--|
| 2a    | $\chi^2(78) = 121.41$ , $p = 0.001$ , $\chi^2/df = 1.56$ , CFI = 0.98, RMSEA = 0.03 [0.019, 0.040], SRMR = 0.035 |
| 2b    | $\chi^2(86) = 145.33$ , $p < .001$ , $\chi^2/df = 1.69$ , CFI = 0.98, RMSEA = 0.03 [0.024, 0.042], SRMR = 0.059  |
| 3a    | $\chi^2(78) = 133.29$ , $p < .001$ , $\chi^2/df = 1.71$ , CFI = 0.98, RMSEA = 0.04 [0.025, 0.046], SRMR = 0.040  |
| 3b    | $\chi^2(86) = 151.99$ , $p < .001$ , $\chi^2/df = 1.77$ , CFI = 0.97, RMSEA = 0.04 [0.027, 0.047], SRMR = 0.062  |

There was also a significant reverse relationship between T1 outgroup attitudes and T2 positive contact,  $b = 0.007$ ,  $SE = 0.003$ ,  $p = .029$ , but no significant reverse effect of T1 outgroup attitudes on T2 negative contact. Finally, there were significant demographic effects of T1 gender on T2 positive contact,  $b = 0.24$ ,  $SE = 0.11$ ,  $p = .04$ , T1 age on T2 negative contact,  $b = -0.005$ ,  $SE = 0.002$ ,  $p = .038$ , and T1 age on T2 outgroup attitudes,  $b = 0.15$ ,  $SE = 0.06$ ,  $p = .01$ .

Amongst Catholic respondents, I once again found significant autoregressive paths for positive contact,  $b = 0.59$ ,  $SE = 0.08$ ,  $p < .001$ , and negative contact,  $b = 0.25$ ,  $SE = 0.06$ ,  $p < .001$ , but not outgroup attitudes. T2 negative contact was significantly associated with T1 positive contact,  $b = -0.18$ ,  $SE = 1.34$ ,  $p < .001$ .

There was a significant forward relationship between T1 positive contact and T2 outgroup attitudes,  $b = 4.12$ ,  $SE = 1.34$ ,  $p = .002$ , but not between T1 negative contact and T2 outgroup attitudes. There were also no significant reverse relationships between T1 outgroup attitudes and T2 positive or negative contact, nor were there any significant demographic effects.

***Structural model and moderation results.*** Having confirmed a longitudinal effect of positive and negative contact on outgroup attitudes (except for a lack of longitudinal negative contact effect on attitudes for Catholics), I proceeded to probe for my hypothesised moderation effects using models which included the forward paths only. These forward models 2b (Protestants) and 3b (Catholics) therefore regressed T2 outgroup attitudes onto T1 and T2 positive and negative contact, and T1 age and gender (see Figure 4).

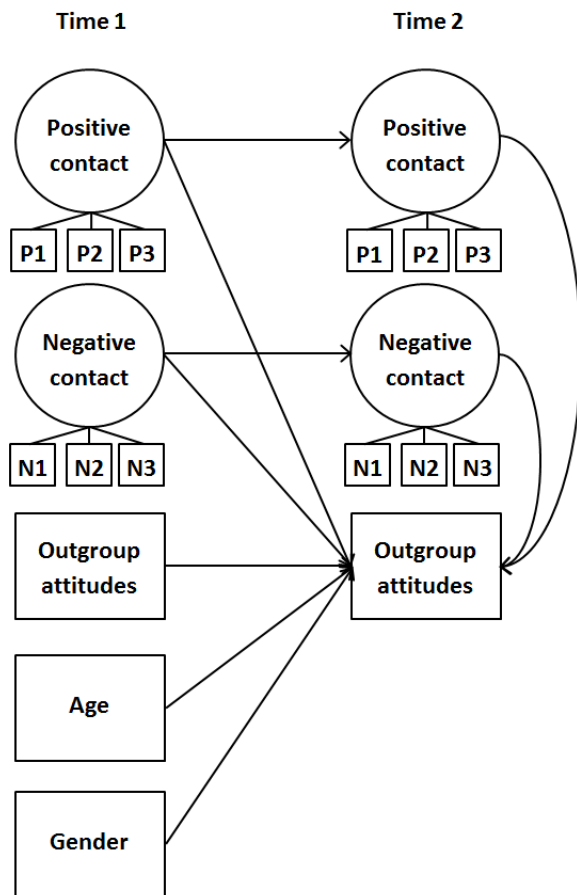


Figure 4. Path diagram for forward models (Study 3, models 2a and 3a)

Amongst Protestant respondents, T2 positive contact was significantly positively associated with T2 outgroup attitudes,  $b = 3.86$ ,  $SE = 1.39$ ,  $p = .005$ . T2 negative contact was negatively associated with outgroup attitudes,  $b = -6.85$ ,  $SE = 2.40$ ,  $p = .004$ , and T1 age,  $b = 0.14$ ,  $SE = 0.06$ ,  $p = .015$ . The associations between T1 positive and negative contact and T2 outgroup attitudes were non-significant. Amongst Catholic respondents, T2 positive contact was also significantly positively associated with T2 outgroup attitudes,  $b = 4.46$ ,  $SE = 1.41$ ,  $p = .002$ , whilst T2 negative contact was negatively associated with T2 outgroup attitudes,  $b = -4.09$ ,  $SE = 2.12$ ,  $p = .05$ . Once again, there were no significant associations between T1 positive or negative contact and T2 outgroup attitudes.

In order to test the interaction of positive and negative contact according to my four hypotheses, two separate moderation models were run for each respondent group. The moderation terms for each model were generated using latent variable interactions, in which the interaction term was defined by a random slope produced from the independent variable and the moderator variable. In the first moderation model, T1 positive contact was treated as the moderator of T2 negative contact on T2 attitudes, in order to test the buffering/negative augmentation hypotheses. Amongst Protestant respondents, the interaction term was significant,  $b = 5.75$ ,  $SE = 1.95$ ,  $p = .003$ , with simple slopes analysis indicating that the effect of T2 negative contact on T2 attitudes was significant when T1 positive contact was low,  $b = -10.23$ ,  $SE = 2.64$ ,  $p < .05$ , but non-significant when T1 positive contact was high,  $b = 0.88$ ,  $SE = 3.32$ ,  $p = .71$ . This result is consistent with the buffering hypothesis. Amongst Catholic respondents, the interaction term was non-significant,  $b = 2.83$ ,  $SE = 3.28$ ,  $p = 0.39$ .

In the second moderation model, T1 negative contact was entered as a moderator of the effects of T2 positive contact on T2 attitudes, in order to test the poisoning/positive augmentation hypotheses. For Protestant respondents, the interaction term was significant,  $b = 5.21$ ,  $SE = 1.74$ ,  $p = .003$ . Simple slopes revealed that T2 positive contact was significantly positively associated with T2 attitudes when T1 negative contact was high,  $b = 7.50$ ,  $SE = 1.61$ ,  $p < .05$ , but not when it was low,  $b = 0.68$ ,  $SE = 1.89$ ,  $p = .72$ . This result is consistent with the positive augmentation hypothesis. Once again, the interaction term was non-significant amongst Catholic respondents,  $b = -1.00$ ,  $SE = 2.29$ ,  $p = 0.66$ . Simple slopes from the Protestant moderation models are shown in Figure 5 and Figure 6.

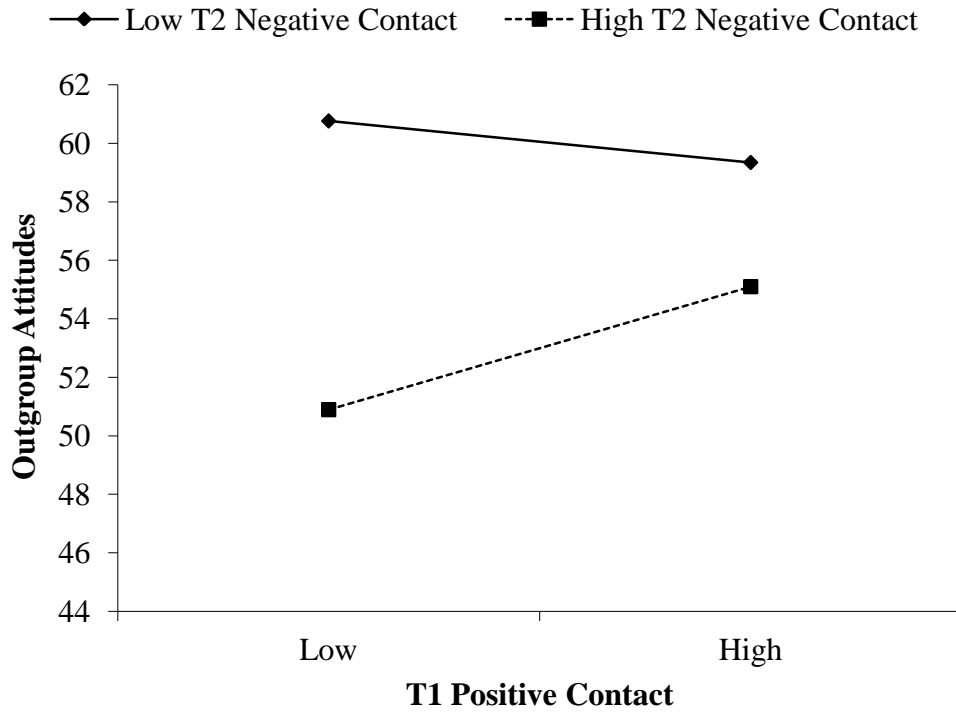


Figure 5. Simple slopes for T1 positive contact moderating the effects of T2 negative contact on T2 outgroup attitudes (Study 3)

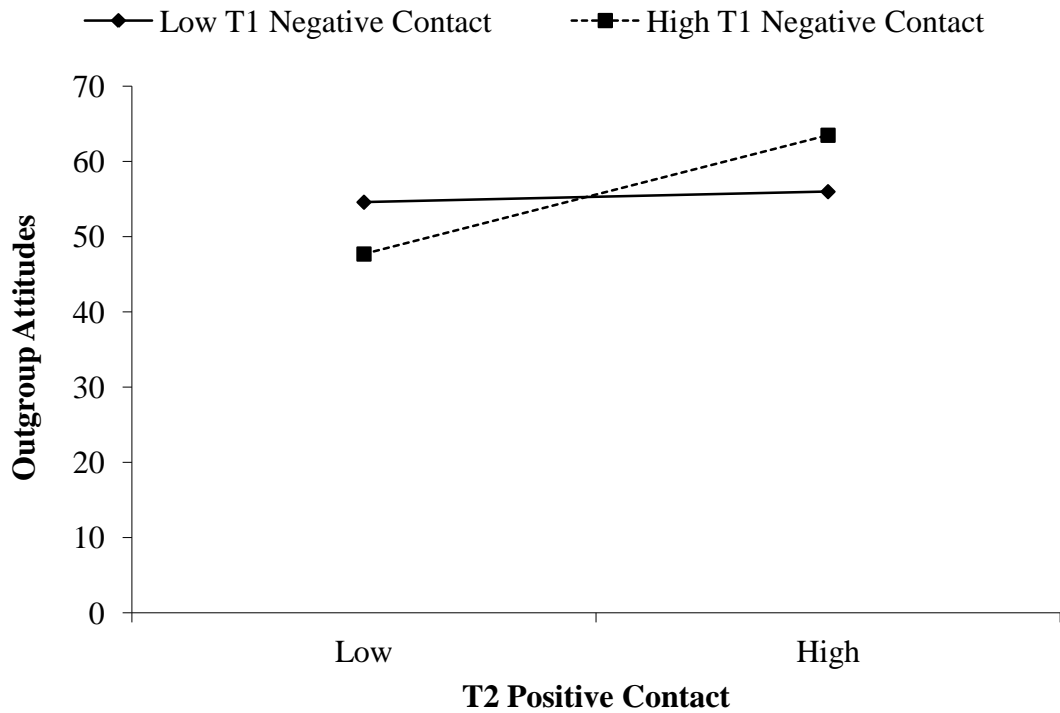


Figure 6. Simple slopes for T1 negative contact moderating the effects of T2 positive contact on T2 outgroup attitudes (Study 3)

The results from Study 3 provide further support for the existence of valenced contact interactions, and in particular supply the first direct evidence (in this thesis) for the buffering and positive augmentation hypotheses. The augmentation effect of time 1 negative contact on the relationship between time 2 positive contact and time 2 attitudes matches the findings of Birtel and Crisp (2012), who demonstrated a similar effect, albeit using sequential *imagined* contact interactions to affect participants' levels of outgroup anxiety. The buffering effect found in my data also resembles that of Paolini et al. (2014), who showed that past positive contact had the potential to reduce the detrimental effects of subsequent negative contact (in their data, specifically the effects of negative contact on outgroup category salience). By testing these ideas using measures of readiness for reconciliation (Study 1) and outgroup attitudes (Studies 2 and 3) the data I have presented thus far add to a growing body of literature demonstrating the potential for valenced contact interactions to affect multiple contact outcomes. Although limited to only two studies (2 and 3), my results also provide some degree of replication using the same dependent variable (outgroup attitudes).

However, the replication of interaction effects (including the use of common dependent variables) is once again qualified by group differences. It is interesting that the interaction effects in Study 3 were limited to the Protestant respondents, in contrast to the group-invariant effects found in the same Northern Irish context in Study 2. In all three studies reported thus far, I found evidence for a lack of structural invariance across groups, that is, respondent groups differed in the structural relationships between the contact and outcome variables. This once again reinforces the point discussed in Study 2 that although valenced contact interactions appear to be generalisable (i.e., occurring in multiple different contexts), they are not universal.

In terms of an explanation for these group differences (see also Study 2 discussion), there may be some effect of the measurement method used in Studies 2 and 3. The only major difference between the two studies was the use of cross-group friendships as a proxy for positive contact in Study 2, as opposed to the dedicated positive contact experience questions used in Study 3. The conceptual difference between these measures, though by no means trivial, is certainly less extreme than the difference between these Likert scale measures, and the binary contact items used in Study 1. It is possible that the more 'intense' form of contact represented by cross-group friendships (as opposed to more general positive contact experience measures) could perhaps explain the loss of interaction effects for Catholic respondents between Studies 2 and 3. However, this hypothesis would still require elements of my proposed explanation from Study 2 - that of group- or context-specific moderating variables - to account for why the difference was only observed for Catholics and not Protestants. Perhaps the specific social context in Belfast is such that Catholic residents require more intense positive exposure to the outgroup in order to successfully buffer against the effects of negative contact. If this were the case, it may be that the general positive contact scale used in Study 3 does not capture such an intense form of outgroup contact.

It is also possible that the lack of interaction effects for Catholics in Study 3 may result from the lack of a significant forward effect of time 1 negative contact on time 2 outgroup attitudes. Although it is entirely possible to find significant interaction effects without one or both associated main effects, the lack of a negative contact effect amongst Catholics provides a key point of differentiation between the two respondent groups. From the group effects analysis, I found that Catholic respondents actually reported greater overall levels of negative contact than Protestants, so it is unlikely that the group differences resulted from floor effects. The final model used to test interaction effects in Study 3 showed metric invariance across groups, suggesting that Catholics and Protestants understood the valenced

contact questions in a similar fashion (though this did require the exclusion of one item, "Been threatened with harm", indicating some difference in broad social experience between the two groups). Without more contextual or process variables to include in the model, I remain unable to provide a more specific response to the question of group differences, so once again must reiterate the need for a dedicated research agenda focused on this issue.

As a final comment on the results of Study 3, the longitudinal demonstration of valenced contact interactions provides a stronger inference of causal sequence than the previous cross-sectional studies. These results are, however, far from a comprehensive test of causality for either buffering or positive augmentation effects. With this goal in mind, Chapter Five will detail three experimental tests of valenced contact effects and, where possible, their interactions. Prior to this, however, Chapter Four deals with an alternative form of valenced contact interactions, specifically those occurring in a multilevel framework.

## **CHAPTER FOUR: MULTILEVEL TESTS OF VALENCED CONTACT INTERACTIONS**

In Studies 1-3 I focused on the effects of individuals' positive and negative contact. However, within the broader contact literature there is evidence to suggest that attitudes towards outgroup members are determined by more than just direct personal experience. In particular, Wright, Aron, McLaughlin-Volpe, and Ropp's (1997) concept of extended contact (the knowledge that one's ingroup friends have outgroup friends), along with work on the influence of group norms (e.g., Jetten, Spears, & Manstead, 1997; Pettigrew, Christ, Wagner, & Stellmacher, 2007), and recent research showing the effects of contextual-level contact (e.g., Christ et al., 2014; see also De Tezanos-Pinto, Bratt, & Brown, 2010) all point to the importance of social context in determining outgroup attitudes.

Furthermore, many of the explanations for general negative asymmetry effects emphasise the role of context in driving bias towards negative stimuli. For example, Fiske's (1980) extremity hypothesis states that since the majority of our day-to-day experiences are broadly positive, our psychological default position or neutral point is positively skewed. Thus, any negative stimulus we encounter is likely to deviate further from this neutral point than a similarly intense positive stimulus. This explanation holds true for any system in which the common or normative experience (i.e., the context) is positive. However, when applying the logic of this analysis to behaviour between members of different groups – especially when marked by prejudice, discrimination or open conflict – it is likely that the neutral point (i.e., the perceived normative valence for outgroup behaviour) will be negative, thus augmenting the impact of positive stimuli. In an intergroup scenario, the valence of one's neutral point for the outgroup may be influenced not only by personal experience, but also by

all the contextual factors mentioned previously: extended contact, perceived group norms, and stereotypes.

Based on Fiske's extremity hypothesis (and other negativity effect explanations emphasising the role of context valence; for a review see Baumeister et al., 2001), I predicted that contextual-level contact should interact with individual experience in a similar manner to prior individual contact. For example, if someone lives in a neighbourhood in which there is a high level of negative contact, their outgroup neutral point is likely to be skewed in the negative direction, increasing the effectiveness of any positive outgroup interactions they might experience. Conversely, I predicted that sufficiently high levels of contextual positive contact would increase the effect of individual-level negative contact. However, given the tendency for outgroups to be regarded in a generally negative fashion in relation to the ingroup (Tajfel & Turner, 2001), even for relations between them to be seen in zero-sum terms (Esses, Jackson, & Armstrong, 1998), I expected that this negative contextual augmentation would be less likely to occur than its positive counterpart.

#### **Study 4**

In order to test the interaction between contextual- and individual-level valenced contact, I analysed a large-scale representative sample of UK residents' contact with, and attitudes towards different social groups, collected in collaboration with the Social Integration Commission ([socialintegrationcommission.org.uk](http://socialintegrationcommission.org.uk)). Each participant in the survey was coded according to their residential area, allowing me to apply multilevel modelling techniques to test for the contextual level effects of positive and negative contact on individual-level outgroup trust, as well as the individual-level effects described previously.

The criterion for a positive contextual augmentation effect was a positive interaction term involving neighbourhood-level negative contact and individual-level positive contact, indicating that the effect of individual-level positive contact on trust was increased by neighbourhood-level negative contact. Conversely, the criterion for a negative contextual augmentation effect was a positive interaction term involving neighbourhood-level positive contact and individual-level negative contact.

Finally, the multilevel design of this study allowed me to test one of the key assumptions made by valenced contact researchers such as Barlow et al. (2012), regarding the role of social diversity. Contact theorists have generally argued that by increasing opportunities for positive outgroup interactions, diversity has the potential to reduce prejudice, and improve intergroup relations (see Stein, Post, & Rinden, 2000; Wagner, Christ, Pettigrew, Stellmacher, & Wolf, 2006). However, Barlow et al. (2012) pointed out that living in a diverse neighbourhood should present opportunities for both positive and negative contact. Since Barlow et al. also claim that negative contact exerts a stronger influence over outgroup attitudes than positive contact, they suggest that diversity could therefore have a net negative effect on intergroup relations, despite the positive opportunities it provides. Two studies by Schmid et al. (Schmid, Al Ramiah, & Hewstone, 2014; Schmid et al., 2008) have demonstrated that diversity can have both positive and negative consequences for intergroup bias, action tendencies and trust. In particular, Schmid et al. (2014) showed that the direct negative effect of diversity on outgroup trust was entirely cancelled out by an indirect positive effect via positive contact. However, to my knowledge no study to date has directly tested the relationship between diversity, valenced contact and outgroup trust. In Study 4 I was also able to include neighbourhood diversity as an additional predictor of valenced contact and trust. I hypothesised that neighbourhood diversity (in this case the ethnic

fractionalisation index associated with each neighbourhood) would be positively related to positive and negative contact, both at the neighbourhood and individual level.

## **Method**

***Respondents and procedure.*** The data for this study were collected by a professional survey organisation (Ipsos MORI) between the 17 and 28 January 2014. A representative sample of the UK population (n = 4269) were recruited through online panels (aged 13-80)<sup>9</sup>. The total sample included 500 ethnic minority participants, but I included only White respondents in my analysis (excluding those in the 'Black', 'Asian' 'mixed' and 'other' ethnic categories) due to small cell numbers. This produced a final sample of 3529 (1773 male, 1756 female) respondents, nested within 663 neighbourhoods (average cluster size = 5.3).

***Measures.*** This study used questions on positive and negative contact with, and trust towards different ethnic outgroups. These were drawn from a larger number of items included in the survey, further details of which may be found in the Social Integration Commission report (Social Integration Commission, 2014).

### ***Individual-level variables.***

***Valenced contact.*** The survey included single-item measures of positive and negative contact adapted from Barlow et al. (2012). The measures were prefaced by an additional introductory paragraph: "We would now like to ask you some questions about your social interactions with different groups of people. By social interactions we mean, chatting with or mixing with people or doing something together. Please read the following questions and answer them as honestly as possible." This paragraph was followed by the items for positive contact: "In the last year, how often, if at all, did you have positive/good interactions with [OUTGROUP]? This could be things you have enjoyed, or where someone said or did

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<sup>9</sup> The 13-15 and 60-80 year-old respondents were recruited through oversampling. Excluding the young participants (< 18) did not alter the pattern of results.

something good to you." and negative contact: "In the last year, how often, if at all, did you have negative/bad interactions with [OUTGROUP]...? This could be things that have upset you, or where someone said or did something bad to you." (1 = *never* to 5 = *very often*).

*Outgroup trust.* The valenced contact items were followed by a single-item measure of outgroup trust: "Thinking about people from each of the following ethnic groups, to what extent do you think they can or cannot be trusted?" (1 = *None of them can be trusted* to 5 = *All of them can be trusted*; Schmid, Al Ramiah, & Hewstone, 2014).

### ***Neighbourhood-level variables.***

*Diversity.* Neighbourhood diversity was measured using Taylor and Hudson's (1972) ethnic fractionalisation (EF) index, which describes the probability that two randomly selected individuals from a given population will come from different social groups. EF scores range from 0 (zero diversity) to 1 (high diversity). For the 663 neighbourhoods in my sample, EF scores varied between 0 and 0.86 ( $M = 0.21$ ,  $SD = 0.19$ ).

*Valenced contact.* Individual participants' positive and negative contact scores were aggregated by output area to form measures of neighbourhood-level contact.

## **Results and discussion**

*Preliminary analyses.* Descriptive statistics, inter-item correlations, and intraclass correlations for majority (White) Respondents are shown in Table 7. For group differences, see Notes<sup>10</sup>.

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<sup>10</sup> In order to test target group differences in Study 4, I computed a 2 (Participant Gender: Female vs. Male) X 2 (Contact type: Positive vs. Negative) X 2 (Target Outgroup: Asian vs. Black) mixed-model ANOVA with repeated measures on the last factor and amount of reported contact as the dependent variable. This produced a large main effect of contact type,  $F(1, 3527) = 5359.72$ ,  $p < .001$ , partial  $\eta^2 = .603$ , with respondents showing higher levels of positive than negative contact ( $M_{\text{Positive}} = 2.89$ ,  $SD = 1.13$  vs.  $M_{\text{Negative}} = 1.45$ ,  $SD = 0.71$ ,  $p < .001$ ,  $d = 1.53$ ). There was also a small main effect of target outgroup,  $F(1, 3527) = 50.87$ ,  $p < .001$ , partial  $\eta^2 = .014$ , with respondents reporting more overall contact with Asians than with Blacks ( $M_{\text{Asian}} = 2.21$ ,  $SD = 0.83$  vs.  $M_{\text{Black}} = 2.13$ ,  $SD = 0.77$ ,  $p < .001$ ,  $d = 0.10$ ), plausibly due to the relative size of those populations in the UK (Asian = 6.9%, Black = 3.0% according to 2011 UK census data). Finally, there was a small

Table 7. Majority (White) Respondents' Inter-item Correlations, Intraclass Correlations, Means and Standard Deviations of Study Variables (Study 4)

| Variable                        | Mean (SD)   | Intraclass Correlation | 1. | 2.    | 3.     | 4.    | 5.     | 6.     | 7.    |
|---------------------------------|-------------|------------------------|----|-------|--------|-------|--------|--------|-------|
| 1. Positive Contact with Asians | 2.95 (1.26) | .08 <sup>1</sup>       | -  | .17** | .19**  | .61** | .17**  | .15**  | .22** |
| 2. Negative Contact with Asians | 1.46 (0.81) | .06 <sup>1</sup>       |    | -     | -.11** | .22** | .73**  | -.08** | .19** |
| 3. Trust towards Asians         | 3.48 (0.73) | .00                    |    |       | -      | .16** | -.08** | .87**  | .023  |
| 4. Positive Contact with Blacks | 2.84 (1.22) | .09 <sup>1</sup>       |    |       |        | -     | .22**  | .17**  | .26** |
| 5. Negative Contact with Blacks | 1.43 (0.77) | .06 <sup>1</sup>       |    |       |        |       | -      | -.12** | .23** |
| 6. Trust towards Blacks         | 3.49 (0.72) | .000                   |    |       |        |       |        | -      | .011  |
| 7. Ethnic Fractionalisation     | .220 (1.34) | .66 <sup>1</sup>       |    |       |        |       |        |        | -     |

Note. \* $p < .05$ . \*\*  $p < .01$ . \*\*\*  $p < .001$ . SD = Standard Deviation, shown in parentheses.

<sup>1</sup> Intraclass correlation greater than .05 (see Raudenbush & Bryk, 2002)

**Multilevel relationships and moderations.** In order to test the multi-level relationships between my key variables, two path models were generated, one with Blacks, and one with Asians as the outgroup. In these models, ethnic fractionalisation was modelled as a predictor of both individual-level and neighbourhood-level positive and negative contact. Individual-level positive and negative contact, in turn, were hypothesised to predict individual-level outgroup trust. Finally, neighbourhood-level positive contact was entered as a cross-level moderator of the relationship between individual-level negative contact and

significant interaction between contact type and target outgroup,  $F(1, 3527) = 13.08, p < .001$ , partial  $\eta^2 = .004$ . Respondents reported higher levels of positive than negative contact with Asians ( $M_{\text{Positive}} = 2.95, SD = 1.26$  vs.  $M_{\text{Negative}} = 1.46, SD = 0.81, p < .001, d = 1.41$ ). The same was true of contact with Blacks, ( $M_{\text{Positive}} = 2.84, SD = 1.22$  vs.  $M_{\text{Negative}} = 1.43, SD = 0.77, p < .001, d = 1.38$ ), but the difference between levels of positive and negative contact was smaller than for Asian contact. There were no significant effects of participant gender.

I also tested target group differences in outgroup trust using a 2 (Participant Gender: Female vs. Male) X 2 (Target Outgroup: Asian vs. Black) mixed-model ANOVA with repeated measures on the second factor and outgroup trust as the dependent variable. This produced only a very small main effect of target outgroup,  $F(1, 3290) = 4.60, p = .032$ , partial  $\eta^2 = .001$ , due to the large sample size, with respondents reporting slightly less trust towards Asians than towards Blacks ( $M_{\text{Asian}} = 3.48, SD = 0.73$  vs.  $M_{\text{Black}} = 3.49, SD = 0.72, p = .032, d = 0.01$ ). There was no significant effect of participant gender.

outgroup trust. Similarly, neighbourhood-level negative contact was entered as a cross-level moderator of the relationship between individual-level positive contact and outgroup trust (see Figure 7 for the hypothesised path model). All models used robust maximum likelihood estimation with random intercept random slope designs.

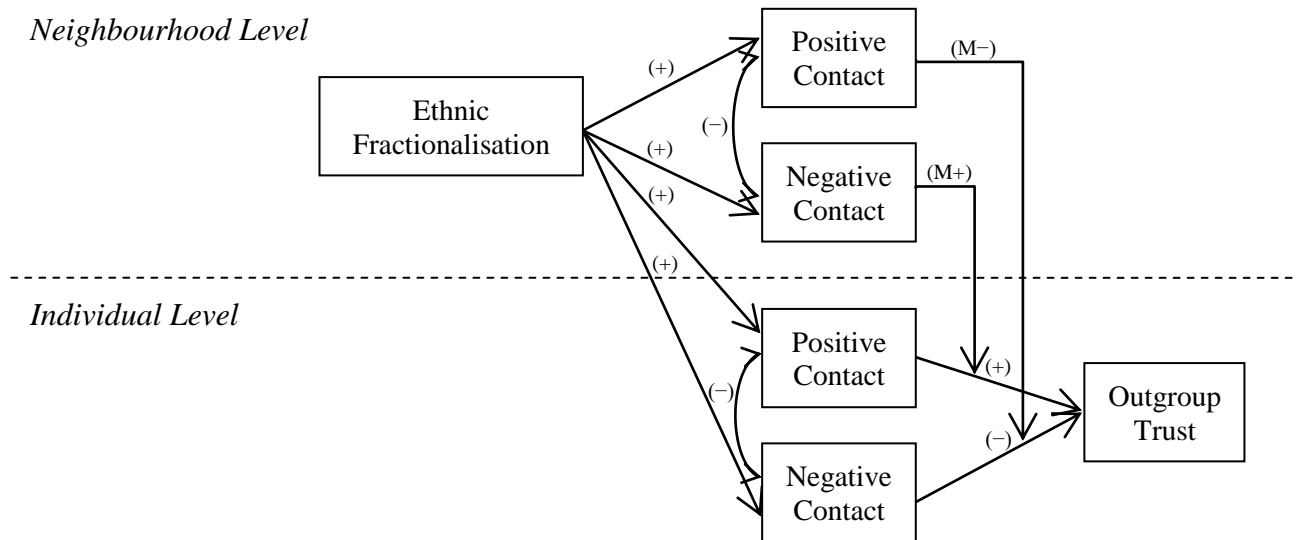


Figure 7. Hypothesised structural relationships between variables (Study 4)

**Basic model.** Positive contact and negative contact produced intraclass correlations greater than .05 justifying their use as multilevel variables (Raudenbush & Bryk, 2002, for ICC values see Table 7); outgroup trust was modelled only at the individual-level for both target outgroups. The structural model showed good fit for both the Black outgroup,  $\chi^2(1) = 4.83$ ,  $p = .028$ ,  $\chi^2/df = 4.83$ , CFI = .99, RMSEA = .033, SRMR<sub>within</sub> = .009, SRMR<sub>between</sub> = .005, and the Asian outgroup,  $\chi^2(1) = 18.91$ ,  $p < .001$ ,  $\chi^2/df = 18.91$ , CFI = .96, RMSEA = .071, SRMR<sub>within</sub> = .010, SRMR<sub>between</sub> = .008. .

Ethnic fractionalisation was significantly related to both individual-level positive contact (Black outgroup  $b = 0.62$ ,  $SE = 0.19$ ,  $p = .001$ ; Asian outgroup  $b = 0.51$ ,  $SE = 0.19$ ,  $p = .009$ ) and individual-level negative contact (Black outgroup  $b = 0.50$ ,  $SE = 0.11$ ,  $p < .001$ ;

Asian outgroup  $b = 0.48$ ,  $SE = 0.14$ ,  $p < .001$ ). In turn, individual-level positive contact was positively associated with outgroup trust (Black outgroup  $b = 0.13$ ,  $SE = 0.01$ ,  $p < .001$ ; Asian outgroup  $b = 0.13$ ,  $SE = 0.01$ ,  $p < .001$ ). Individual-level negative contact was negatively associated with outgroup trust (Black outgroup  $b = -0.15$ ,  $SE = 0.02$ ,  $p < .001$ ; Asian outgroup  $b = -0.13$ ,  $SE = 0.02$ ,  $p < .001$ ). Ethnic fractionalisation at the neighbourhood-level was also significantly related to neighbourhood-level positive contact (Black outgroup  $b = 1.81$ ,  $SE = 0.12$ ,  $p < .001$ ; Asian outgroup  $b = 1.64$ ,  $SE = 0.12$ ,  $p < .001$ ) and neighbourhood-level negative contact (Black outgroup  $b = 0.94$ ,  $SE = 0.09$ ,  $p < .001$ ; Asian outgroup  $b = 0.83$ ,  $SE = 0.10$ ,  $p < .001$ ).

**Moderation model.** To assess whether neighbourhood-level positive and negative contact moderated the relationship between individual-level contact (of the opposite valence) and outgroup trust, I created a model in which the relationships between individual-level contact and trust were used to generate two random slopes (one for positive contact and another for negative contact). These random slopes were regressed onto neighbourhood-level contact of the opposite valence. I identified a significant moderating effect of neighbourhood-level negative contact on the relationship between individual-level positive contact and outgroup trust for both Black,  $b = 0.13$ ,  $SE = 0.06$ ,  $p = .04$ , and Asian,  $b = 0.24$ ,  $SE = 0.07$ ,  $p < .001$ , as the target group. There was also a significant moderating effect of neighbourhood-level positive contact on the relationship between individual-level negative contact and outgroup trust for the Black outgroup,  $b = -0.11$ ,  $SE = 0.06$ ,  $p = .04$ , but not for the Asian outgroup,  $b = -0.06$ ,  $SE = 0.06$ ,  $p = .34$ .

In order to decompose the cross-level interactions, I calculated simple slopes for the individual-level main effects at high (+1SD) and low (-1SD) levels of the neighbourhood-level moderators. In order to generate the neighbourhood-level average of the individual-level main effects coefficients (i.e., the individual-level effect of positive/negative contact on trust,

averaged across clusters), I used the intercepts for the random slopes of the individual-level main effects. The product of the neighbourhood-level interaction term and the standard deviation of the moderator was then added to the individual-level random slope intercepts to produce the high-level simple slope, and subtracted from the individual-level random slope intercept to produce the low-level simple slope. Following this method, I found that the effect of individual-level positive contact on outgroup trust towards Blacks was larger at high levels of neighbourhood-level negative contact,  $b = 0.15$ ,  $SE = 0.02$ ,  $p < .001$ , than at low levels of neighbourhood-level negative contact,  $b = 0.11$ ,  $SE = 0.02$ ,  $p < .001$ . Similarly, the effect of individual-level positive contact on outgroup trust towards Asians was larger at high levels of neighbourhood-level negative contact,  $b = 0.17$ ,  $SE = 0.02$ ,  $p < .001$ , than at low levels of neighbourhood-level negative contact,  $b = 0.09$ ,  $SE = 0.02$ ,  $p < .001$ . Finally, the effect of individual-level negative contact on outgroup trust towards Blacks was larger at high levels of neighbourhood-level positive contact,  $b = -0.14$ ,  $SE = 0.02$ ,  $p < .001$ , than at low levels of neighbourhood-level positive contact,  $b = -0.10$ ,  $SE = 0.03$ ,  $p < .001$ .

The results of Study 4 demonstrate that valenced contact interactions can occur across different levels of contact exposure. Specifically, respondents from neighbourhoods with high levels of negative contact showed larger effects of individual-level positive contact.

Conversely, respondents from neighbourhoods with high levels of positive contact showed larger effects of individual-level negative contact, at least with Black outgroup members.

These findings provide support for both the positive and the negative contextual augmentation hypothesis, and are consistent with an explanation based on Fiske's (1980) extremity hypothesis. They demonstrate, for example, that the presence of a negative social context augments the impact of positive outgroup stimuli. Interestingly, this is thus far the only situation in which I have found evidence (albeit limited to a single outgroup) for the negative augmentation hypothesis. A possible explanation for this might be that contextual

contact is a stronger determinant of individuals' default value of outgroup neutral point (i.e., the default valence of their internal representation of the outgroup), since it more directly relates to the behaviour of the outgroup as a whole entity compared to personal contact experiences. It should be noted, however, that an explanation of contextual augmentation effects based on the extremity hypothesis relies on the assumption that individuals have some level of awareness of the contact interactions occurring in their neighbourhood. Although our data do demonstrate an effect of neighbourhood-level contact on individual-level experiences, we have not tested participants' self-reported ratings of how much contact occurs in their neighbourhood. This is a key variable in the causal sequence proposed by the extremity hypothesis, and should ideally be included in future multilevel studies of valenced contact.

### **Summary of survey research**

From the survey data presented in Chapters One to Four, I have identified significant interactions between positive and negative contact in five of the eight sample groups tested (Serbs in Study 1, Catholics and Protestants in Study 2, Protestants in Study 3, and White UK residents in Study 4). This demonstrates the validity of valenced contact interactions as a genuine phenomenon, although the lack of significant interactions amongst Croats and Bosniaks in Study 1, and Catholics in Study 3 suggests that they are by no means a universal occurrence. As for the specific interaction hypotheses proposed in Chapter One, the majority of findings from these survey studies support the buffering and positive augmentation hypotheses. The cross-sectional data from Studies 1 and 2 do not allow statistical discrimination between possible buffering and/or positive augmentation effects, but certainly do not support the poisoning or negative augmentation hypotheses. In Study 3 however, I found specific evidence for buffering effects across time (i.e., prior positive contact buffering

against the detrimental effect of subsequent negative contact on outgroup attitudes), as well as positive augmentation across time in Study 3 (i.e., prior negative contact augmenting the beneficial effects of subsequent positive contact), and across analytic scale in Study 4 (i.e., neighbourhood-level negative contact augmenting the effect of individual-level positive contact). In Study 4, I have also found limited evidence for the negative augmentation hypothesis, with White respondents' neighbourhood-level positive contact augmenting the effect of their individual-level negative contact with Black (but not Asian) outgroup members.

As for the main effects of valenced contact, the only study in which I have not found both positive and negative contact to be significantly associated with the dependent variable is Study 1 (in which Serbs and Bosniaks show no effect of negative contact on readiness for reconciliation). In terms of the valence asymmetry hypothesis, the presence of significant valenced contact interactions undermines the basic premise of the additive account espoused by authors such as Barlow et al. (2012), because such interactions demonstrate that the 'strength' of positive contact can depend on the presence of negative contact, and vice versa. As far as the contact valence asymmetry hypothesis is concerned, , the only significant differences I have found in the effect sizes of positive and negative contact are for Bosniaks and Serbs in Study 1, whose lack of significant negative contact effects mean that positive contact naturally emerges as the stronger predictor of readiness for reconciliation.

## **CHAPTER FIVE: EXPERIMENTAL TESTS OF VALENCED CONTACT EFFECTS**

In my research on valenced contact effects, I have established a degree of methodological triangulation. That is, I have shown the same effect (valenced contact interactions) using several different research methods. The studies I have presented thus far provide triangulation across different social contexts, survey designs (cross-sectional, longitudinal, multilevel), and outcome measures. However, a key methodological limitation of survey data lies in their inability to allow inferences of causality. In order to overcome this limitation I have carried out a series of experiments designed to manipulate participants' positive and negative contact experiences. Where possible, I have also tested for the interaction between my experimental manipulations of contact, and participants' prior real-world contact experiences. Although the limitations on the sample sizes available to me rendered these studies somewhat underpowered for the purposes of testing interaction effects, I did find some preliminary evidence of valenced contact interactions. These findings, as well as the main effects of several different forms of valenced contact, are detailed in the three experiments described below.

### **Experiment 1**

For my first experiment into the effects of valenced contact, I wanted to find a simple, easily administered contact manipulation that could be applied to both positive and negative intergroup interactions. With this goal in mind, I drew on the existing work of Birtel and Crisp (2012) who used an imagined contact manipulation to test the effects of sequential positive and negative contact on intergroup anxiety. The imagined contact hypothesis, proposed by Turner, Crisp, and Lambert (2007), posits that simply imagining (positive) interactions with outgroup members can have an impact on one's attitudes and behaviour. In the years since its inception, imagined contact has seen a rapid uptake amongst contact

researchers. A recent meta-analysis by Miles and Crisp (2014) reviewed 71 imagined contact studies published since 2009, and found an overall sample-weighted effect of  $d_+ = 0.35$ , 95% CI [0.26, 0.44]. This effect is predictably somewhat smaller than the effect of direct contact ( $d = 0.42-0.44$ , converted from  $r$  values reported by Pettigrew & Tropp, 2006), but still supports the value of imagined contact as a means of influencing outgroup attitudes (and other group-related outcomes, see Miles & Crisp, 2014).

The rapid emergence of imagined contact research may be due in part to the paradigm's potential for providing contact opportunities to groups who do not frequently experience direct contact. However, it is also likely that the profusion of studies on imagined contact is partly due to the ease with which researchers can conduct imagined contact experiments. A basic imagined contact manipulation requires only a short series of written instructions such as, "We would like you to take a minute to imagine yourself meeting [an outgroup] stranger for the first time. Imagine that the interaction is positive, relaxed and comfortable." This can be administered in a lab, but can also be distributed through online survey software, with the potential to dramatically increase the available participant pool (at the expense of a certain degree of control over the experimental setting). This capacity for enabling relatively large-scale, rapid collection of experimental contact data has made imagined contact an attractive option for researchers who might otherwise struggle to operationalise their research questions in a more traditional direct contact experiment. In the case of my research, although I believe it important to study direct negative contact where possible (see Experiment 3), operationalising a negative imagined contact experience is a significantly less demanding proposition than engineering a believably negative direct interaction with an outgroup member in the lab. I therefore elected to use imagined interactions for my *first* experimental manipulation of valenced contact.

My imagined contact paradigm involved participants imagining a positive, negative, or neutral interaction with a homeless person (or a non-contact control scenario). The homeless were chosen because they are an outgroup with whom my participants (Oxford undergraduates) were likely to have had some positive and negative exposure (according to a report published by the Department for Communities and Local Government, 2013, at the time of data collection Oxford had a relatively high street count of 19 rough sleepers).

A key question when designing an imagined contact experiment (or indeed any contact experiment) is the nature of the control condition. Imagined contact researchers have used various controls ranging from neutral (unvalenced) contact (Stathi & Crisp, 2008, Experiment 1; R. N. Turner, Crisp, et al., 2007, Experiment 1), outgroup priming (R. N. Turner, Crisp, et al., 2007, Experiment 2), and non-contact scenarios (Husnu & Crisp, 2010; Stathi & Crisp, 2008, Experiment 3).

Depending on the particulars of their research question, experimenters may wish to test the effects of contact relative to its absence (i.e., contact vs. no-contact control). However, previous work on imagined contact has emphasised the importance of contact valence by instead comparing the effects of positive contact and neutral (unvalenced) contact (e.g., Stathi & Crisp, 2008). The choice of control condition is determined by the specific requirements of the research question, for example, testing the effects of contact quality (neutral contact control) vs. mere imagined outgroup exposure (no-contact control). So far the effects of imagined negative contact have only been compared to those of imagined positive contact (Birtel & Crisp, 2012), so I elected to include both neutral and no-contact controls in my experiment as both comparisons have relevant theoretical implications.

Finally, one of the major criticisms levelled against the imagined contact hypothesis is that the effects of imagined interactions do not persist beyond the immediate experimental

setting. Bigler and Hughes (2010) point out that the effects of simulated social contact have rarely been demonstrated more than a few days after the experimental manipulation, and argue that imagined contact is unlikely to deviate from this trend. In order to test the effects of my imagined valenced contact manipulation beyond the experimental session, I arranged for participants to complete a follow-up questionnaire seven days after participation.

## **Method**

***Design and respondents.*** Fifty-five undergraduate students (40 female) aged between 17 and 21 ( $M = 18.71$ ,  $SD = 0.79$ ) participated in the study. Participants were randomly allocated to one of four conditions: positive imagined contact, negative imagined contact, neutral imagined contact control, or a no-contact control condition. Measures of outgroup attitudes were taken pre-test, immediately post-test, and seven days post-test.

***Procedure.*** All participants initially completed a pre-test questionnaire (T1) to assess their baseline attitudes towards the homeless. Within 10 days of completing the pre-test questionnaire, participants came into the lab to complete an imagined contact task. During the lab session, participants were seated in a cubicle and given an instruction sheet with information on how to proceed with the imagined contact scenario. The instructions given to participants in each of the experimental conditions were adapted from Crisp and Turner (2009), whilst the no-contact control condition was based on Turner, Crisp, and Lambert (2007):

***Positive contact:*** “We would like you to take a minute to close your eyes and imagine walking down Cornmarket on a Monday, and along the way meeting a homeless person. Imagine that you spend a few minutes talking, and that the interaction is positive, relaxed and comfortable.”

*Negative contact:* “We would like you to take a minute to close your eyes and imagine walking down Cornmarket on a Monday, and along the way meeting a homeless person. Imagine that you spend a few minutes talking, and that the interaction is negative, tense and uncomfortable.”

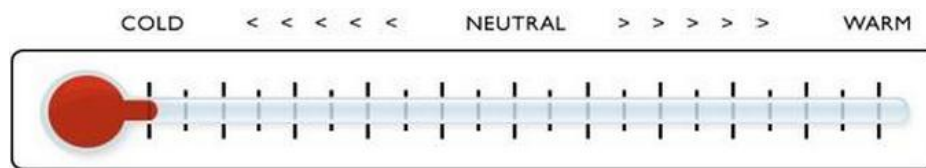
*Neutral contact:* “We would like you to take a minute to close your eyes and imagine walking down Cornmarket on a Monday, and along the way meeting a homeless person. Imagine that you spend a few minutes talking.”

*No-contact control:* “We would like you to take a minute to imagine an outdoor scene. While imagining this, think specifically of when (e.g., Monday afternoon) and where (e.g. is it a beach, a forest, are there trees, hills, what is on the horizon?) the scene is occurring.”

Participants were given a few minutes to read the instructions, after which time they were given the opportunity to ask questions about the task. Once satisfied with the instructions, participants were asked to lie back in their chair, relax, and close their eyes whilst engaging in the imagined scenario. The lights in the cubicle were turned off, and the participants left alone for three minutes to complete the imagined interaction. After the three minutes had elapsed, the lights were turned back on, and the participants were given a short questionnaire (T2) in which they provided details of their imagined experiences, rated the positivity/negativity of the experience (manipulation check), and once again reported their attitudes towards the homeless. Finally, seven days after completing the lab session participants were sent a link to a third online questionnaire (T3) which measured their attitudes towards the homeless one final time. After completing the T3 questionnaire, participants were provided with debriefing information and compensation in the form of course credit.

## Measures

*Outgroup attitudes* were measured in all three questionnaires using a 'feeling thermometer', which was described to respondents as follows, " How do you feel about homeless people in Oxford? Please make your response on a thermometer that runs from zero (0) to one hundred (100) degrees. The higher the number, the warmer or more favourable you feel. The lower the number, the colder or less favourable you feel."



*Imagined scenario positivity* was measured using three items (Cronbach's  $\alpha = .95$ ), "How [positive]/[pleasant]/[comfortable] would you rate the scenario you imagined?" (0: "Not at all" – 100: "Completely"), from which an index of scenario positivity was computed.

*Imagined scenario negativity* was measured using three items (Cronbach's  $\alpha = .88$ ), "How [negative]/[unpleasant]/[uncomfortable] would you rate the scenario you imagined?" (0: "Not at all" – 100: "Completely"), from which an index of scenario negativity was computed.

## Results

**Power analysis.** In order to determine the suitability of my sample for the test I wished to run, I used the power calculation software G\*Power 3 to plot power as a function of total sample size for small ( $f^2 = .05$ ), medium ( $f^2 = .20$ ), and large ( $f^2 = .35$ ) effect sizes (see Figure 8). The results of this analysis indicate that the sample of 55 would provide sufficient power (desired threshold 0.8) for medium and large effects (see Figure 8).

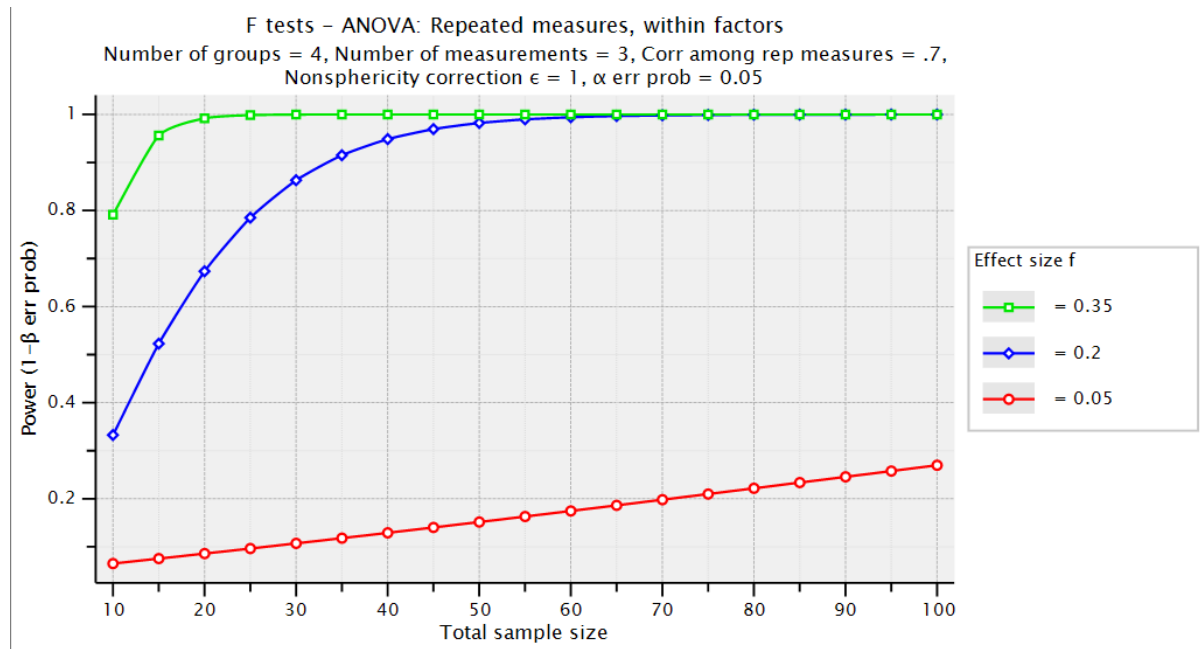


Figure 8. Plot of power against sample size for small, medium and large effects (Experiment 1)

**Preliminary results.** T1 attitudes ( $M = 52.76$ ,  $SD = 14.90$ ) were found to be strongly correlated with T2 attitudes ( $M = 54.87$ ,  $SD = 16.45$ ),  $r = .65$ ,  $p < .001$ , and T3 attitudes ( $M = 57.02$ ,  $SD = 16.12$ ),  $r = .71$ ,  $p < .001$ . T2 attitudes were also strongly correlated with T3 attitudes,  $r = .81$ ,  $p < .001$ . T2 attitudes were significantly positively correlated with imagined scenario positivity ( $M = 59.02$ ,  $SD = 34.00$ )  $r = .36$ ,  $p = .011$ , and negatively correlated with negativity ratings ( $M = 31.98$ ,  $SD = 31.93$ ),  $r = -.43$ ,  $p = .001$ . Finally, imagined scenario positivity and negativity were highly negatively correlated,  $r = -.91$ ,  $p < .001$ .

In order to test the perceived valence of the various imagined scenarios, I ran a one-way (Condition: positive contact vs. negative contact vs. neutral contact vs. no-contact control) MANOVA on the two manipulation check variables (indexes for imagined scenario positivity and negativity). This analysis revealed an overall main effect of condition,  $F(6, 92) = 11.63$ ,  $p < .001$ ,  $\text{partial } \eta^2 = .43$ , which was maintained for both imagined scenario

positivity,  $F(3, 46) = 25.77, p < .001$ , partial  $\eta^2 = .63$ , and negativity,  $F(3, 46) = 43.13, p < .001$ , partial  $\eta^2 = .74$ .

Pairwise comparisons of positivity ratings by condition revealed that participants in the negative contact condition rated their imagined experiences as significantly less positive ( $M = 11.53, SD = 9.04$ ) than those in the neutral contact condition ( $M = 58.55, SD = 28.51, p < .001, d = 2.22$ ), the positive contact condition ( $M = 68.28, SD = 24.77, p < .001, d = 3.04$ ), and the no-contact control condition ( $M = 86.91, SD = 14.04, p < .001, d = 6.38$ ). Positivity ratings of participants in the neutral contact condition were also significantly lower than those of participants in the no-contact control ( $p = .009, d = 1.26$ ). There were no other significant differences in positivity ratings between conditions.

Pairwise comparisons of negativity ratings by condition revealed that participants in the negative contact condition ( $M = 78.23, SD = 15.31$ ) rated their imagined experiences as significantly more negative than those in the neutral contact condition ( $M = 21.74, SD = 20.01, p < .001, d = 3.17$ ), the positive contact condition ( $M = 19.31, SD = 18.24, p < .001, d = 3.50$ ), and the no-contact control condition ( $M = 6.86, SD = 8.00, p < .001, d = 5.84$ ). There were no other significant differences between conditions.

An unexpected finding from these manipulation checks was that the no-contact control condition was rated as the most positive and least negative experience for my participants. Although the only significant pairwise comparison amongst the conditions which did not explicitly involve negative experiences (i.e., positive contact, neutral contact and no-contact) was between neutral contact and no-contact on ratings of positivity, the relatively large mean differences between the no-contact and neutral/positive conditions was surprising. I thought it plausible that negative initial attitudes towards the homeless could make any imagined intergroup interaction (even an explicitly positive interaction) less

pleasant than imagining an outdoor scene. In order to investigate this, I ran a one-way (Condition: positive contact vs. no-contact) MANCOVA on the two manipulation check variables (imagined scenario positivity and negativity) with T1 outgroup attitudes as a covariate. The model included a possible interaction between condition and outgroup attitudes. This analysis produced a non-significant multivariate test of the interaction between condition and attitudes,  $F(2, 20) = 2.97, p = .074, \text{partial } \eta^2 = .23$ , but the tests of individual dependent variables produced a significant interaction between condition and attitudes for imagined scenario negativity ratings,  $F(1, 21) = 5.19, p = .030, \text{partial } \eta^2 = .20$ .

In order to explore the interaction further, I ran a series of pairwise comparisons of imagined scenario negativity ratings at different values (mean, +1 SD, and -1 SD) of outgroup attitudes, applying a Bonferroni adjustment for multiple tests (adjusted  $p$  threshold = .017). At mean levels of outgroup attitudes, participants rated the no-contact condition (estimated  $M = 7.48, SD = 13.53$ ) as significantly less negative than the positive contact condition (estimated  $M = 21.76, SD = 13.68, p = .016, d = 1.05$ ). The no-contact condition (estimated  $M = 5.23, SD = 15.53$ ) was also rated as less negative than the positive contact condition estimated ( $M = 31.34, SD = 22.57, p = .003, d = 1.35$ ) at low levels (-1 SD) of outgroup attitudes. However, at high levels (+1 SD) of outgroup attitudes, there was no significant difference in negativity ratings between the no-contact and positive contact conditions. This finding provided some support for the idea that participants with more positive initial attitudes towards the homeless would be more comfortable imagining an interaction with a homeless person, although it must be noted that the sample size of the interaction analysis ( $n = 25$ ) means that these results should be interpreted with caution.

***Effects of imagined contact manipulation on outgroup attitudes.*** In order to test the effect of the imagined contact manipulation on attitudes towards the homeless, I ran a 4 (Condition: positive contact vs. neutral contact vs. negative contact vs. no contact) X 3

(Time: T1 vs. T2 vs. T3) mixed-model ANOVA with repeated measures on the second factor and outgroup attitudes as the dependent variable. This produced a non-significant between-subjects main effect of condition,  $F(3, 48) = 2.76, p = .052$ , partial  $\eta^2 = .15$ , a non-significant within-subjects main effect of time,  $F(2, 96) = 3.04, p = .053$ , partial  $\eta^2 = .06$ , and a significant interaction between time and condition,  $F(6, 96) = 2.32, p = .039$ , partial  $\eta^2 = .13$ . Pairwise comparisons revealed no significant pairwise differences between conditions, or between time points. However, pairwise comparisons for the interaction effect showed that at T2, outgroup attitudes were significantly lower amongst participants in the negative contact condition ( $M = 42.15, SD = 13.11$ ) than for participants in the neutral contact ( $M = 60.85, SD = 14.81, p = .012, d = 1.34$ ) or positive contact ( $M = 63.23, SD = 12.05, p = .003, d = 1.67$ ) conditions. Furthermore, amongst participants in the negative contact condition there was a significant increase in outgroup attitudes between T2 ( $M = 42.15, SD = 13.11$ ) and T3 ( $M = 51.38, SD = 12.49, p = .002, d = 0.72$ ). There were no other significant between conditions or time points.

At face value, the results of Experiment 1 appear to support the view espoused by Barlow et al. (2012) that negative contact has a stronger effect than positive contact (at least within the medium of imagined contact, with the homeless as the outgroup). Participants in the negative contact condition showed significantly lower attitudes towards the homeless immediately following the imagined interaction compared to participants in the positive contact and neutral contact conditions. However, a number of qualifications apply which limit the impact of this finding.

Firstly, my manipulation checks indicated that participants rated only the negative contact condition as significantly less positive/more negative than the positive or control conditions. This suggests that the positive imagined contact manipulation failed to produce a significant effect. This is an interesting finding in and of itself, given that the stimuli used to

produce the positive and negative manipulations were closely matched, with the only differences being the use of the words positive/negative, relaxed/tense, and comfortable/uncomfortable. The results of the study perhaps suggest the presence of a valence salience effect affecting the stimuli themselves, wherein negative imagined interactions may be easier to elicit than positive imagined interactions. This would certainly match the general valence asymmetry literature, in which negative stimuli have been shown to draw more attention and receive more processing than positive stimuli (see Baumeister et al., 2001). However, it presents a significant challenge for future valenced imagined contact research, which will need to ensure the efficacy of both negative and positive manipulations if any comparison is to be made between their effects. At present, the lack of a successful positive manipulation in Experiment 1 precludes the use of these data for comparing the relative effect sizes of positive and negative imagined contact.

Secondly, in line with Bigler and Hughes' (2010) criticism, the effects of my negative contact manipulation did not persist beyond the experimental session. Specifically, although participants in the negative contact condition showed a significant decrease in attitudes from pre-test to immediately post-test, they also reported a significant increase in attitudes between the lab session and a follow-up seven days later (with no significant difference between the pre-test and seven days post-test attitude scores). The most likely explanation for this is a limitation of imagined contact itself, as hypothesised by Bigler and Hughes (2010). Although it is plausible that this finding represents a more generalisable effect of negative contact effects having a short 'half-life', this would oppose evidence from the valence-asymmetry literature which has shown that negative events tend to persist in memory longer than positive ones (e.g., Brickman et al., 1978). Unfortunately, the lack of positive contact effects in my data once again precludes any kind of comparison of relative valenced contact durations. The durability of positive contact effects over time is an ongoing question for the general contact

literature, and comparative testing of negative contact effect should certainly factor into future valenced contact research.

Finally, an interesting finding from these data which does not directly relate to valenced contact effects is the comparison of positivity ratings in the positive vs. no-contact control conditions. I found that although on average participants rated the imagined positive contact interaction as having been a less positive experience than the no-contact control (an imagined walk on the beach), this effect was not found for participants with highly positive (1 SD above the mean) pre-test outgroup attitudes. Although grounded in initial attitudes, rather than pre-manipulation contact experiences, this finding represents a form of buffering effect (i.e., participants with an initially positive view of the outgroup react more favourably to an imagined interaction with an outgroup member). While this effect does not support the buffering hypothesis as stated in my thesis (relating as it does specifically to the effects of positive contact), it does lend credence to the applicability of buffering processes to intergroup contact in general. This being said, the small sample size for this experiment means that such interaction effects should be treated with caution. In general, the small sample size (and small effect sizes) in this study limit its diagnostic value. Improving the available sample size was therefore a key goal of Experiment 2.

## **Experiment 2**

In Experiment two, I had two key aims: first, to test the effects of valence on a different form of contact from Experiment 1; and second, to collect a large enough sample to support the testing of interactions between participants' prior contact experiences, and the experimental manipulation. In order to achieve this, I collaborated with researchers at the Universidad Nacional de Educación a Distancia to carry out an experiment in which we manipulated Spanish participants' perceptions of extended contact with immigrants.

The extended contact hypothesis (also sometimes referred to as the indirect contact hypothesis<sup>11</sup>) provides another alternative to direct contact. It states that simply being aware that other ingroup members are having positive contact with the outgroup can reduce levels of prejudice. The extended contact hypothesis was introduced by Wright, Aron, McLaughlin-Volpe, and Ropp in 1997 with a series of questionnaire studies and experiments demonstrating improved outgroup attitudes for those whose ingroup friends were engaged in direct intergroup contact. Interestingly, Wright et al.'s fourth study was an experiment which included a negative extended contact condition. In this experiment, artificial social groups were created in the lab using a modified minimal groups paradigm (a standard paradigm used to test intergroup bias with minimal social context, see Tajfel, Billig, Bundy, & Flament, 1971). Following group formation, participants observed an ingroup member engaged in direct contact with an outgroup member (both confederates), with the interaction exhibiting friendly affection, polite neutrality or open hostility in the positive, neutral and negative extended contact conditions respectively. Wright et al. (1997) reported that participants in the negative and neutral conditions showed a significant bias against the outgroup in both positive and negative affection, but no bias for participants in the positive condition. Unfortunately, they did not report direct comparisons of the positive vs. neutral and negative vs. neutral effects except in the form of a bar-graph (without labels for mean values).

One of the major advantages of extended contact is that it can provide a vector for reducing prejudice in social contexts where direct contact is rare. Indeed, several studies have

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<sup>11</sup> There is some conflation of terms in the literature on indirect contact (for a detailed discussion, see review by Vezzali, Hewstone, Capozza, Giovannini, & Wölfer, 2014). Wright et al.'s (1997) paper uses the term 'extended contact', defined as the awareness of other ingroup members having contact with the outgroup. However, some later authors use a more limited operationalisation of extended contact as having ingroup friends who have outgroup friends. Under this strict definition of extended contact, observation of contact amongst (unknown) ingroup members would not constitute extended contact due to the absence of direct friendship ties. Instead, observations of ingroup members' contact would be classed as a form of indirect contact specifically referred to as 'vicarious contact' (Vezzali et al., 2014), a category which could include imagined contact, media contact and online contact. However, for the purposes of this thesis, I will use Wright et al.'s original definition of extended contact.

shown that the effects of extended contact are stronger when the individuals in question have lower levels of direct contact (Christ et al., 2010; Dhont & Van Hiel, 2011; Eller, Abrams, & Gomez, 2012). However, these studies only considered the effects of positive extended contact. If negative extended contact were to exhibit the same pattern, this could have particularly damaging consequences for the very same settings where extended positive contact is most valuable (i.e., contexts where direct contact is difficult). In Experiment 2 I therefore sought not only to expand the extended contact literature by experimentally manipulating positive and negative extended contact, but also to test whether prior direct valenced contact moderated the effects of extended valenced contact.

In order to test interactions between direct and extended valenced contact, the sample size for Experiment 2 needed to be considerably larger than that of Experiment 1. I was able to recruit such a sample from a population of Spanish undergraduate students, using an extended contact manipulation based on the presentation of a newspaper article describing the average levels of contact with immigrants amongst the Spanish general population. Compared to other European countries, Spain has a very large immigrant population (the third highest in Europe according to a report by the United Nations Department of Economic and Social Affairs, 2013), and a correspondingly high level of concern about immigration amongst the native population (Martínez & Duval, 2009). Immigrants therefore represented a salient outgroup, with whom these participants were likely to have had a range of contact experiences: positive and negative, direct and extended.

## **Method**

***Design and respondents.*** I recruited 353 Spanish participants (227 female) aged between 17 and 74 ( $M = 33.60$ ,  $SD = 12.14$ ) using snowball sampling from an initial cohort of undergraduate psychology students. Participants were randomly allocated to one of three conditions: positive extended contact, negative extended contact, or a no-contact control

condition<sup>12</sup>. Dependent variables (outgroup attitudes, intergroup anxiety, behavioural intentions and desire for future contact) were measured immediately following the experimental manipulation. Participants also completed pre-test measures of positive and negative direct contact.

**Procedure.** The study was distributed in the form of an online questionnaire which began with pre-test measures of positive and negative direct contact with immigrants in Spain. After completing the pre-test measures, participants were allocated to conditions, before being presented with the experimental manipulation. On the manipulation screen, participants were informed that the Spanish Ministry of Education was investigating Spanish citizens' attitudes towards, and experiences with immigrants. Participants were then presented with a fictitious 250-word newspaper article presented in the format of an online post from a well-known national newspaper. The article was entitled, "Do Spaniards have contact with immigrants?", and described a research project being carried out by the Ministry of Education in collaboration with a number of Spanish universities involving a representative survey of Spanish citizens' contact experiences with immigrants. Depending on the experimental condition, the results of the survey were reported as follows: positive extended contact - "*Most Spaniards report having frequent pleasant contact with immigrants*"; negative extended contact - "*Most Spaniards report having frequent unpleasant contact with immigrants*"; no-contact control - "*Most Spaniards don't report having frequent contact with immigrants*".

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<sup>12</sup> The original study included a fourth condition for neutral extended contact, "*Most Spaniards report having frequent contact with immigrants*". However, when testing for random allocation using a one-way (Condition: positive extended contact vs. negative extended contact vs. neutral extended contact vs. no-contact) MANOVA with participant age, sex, and pre-test ratings of direct and extended positive and negative contact as the dependent variables, the omnibus test produced a significant overall effect of condition,  $F(18, 1566) = 3.33$ ,  $p < .001$ , partial  $\eta^2 = .04$ . Individual between-subjects tests for each of the dependent variables revealed main effects of condition on pre-test direct positive contact,  $F(3, 525) = 4.55$ ,  $p < .001$ , partial  $\eta^2 = .04$ , such that participants' scores in the neutral contact condition ( $M = 2.96$ ,  $SD = 1.04$ ) were significantly lower than those in the negative ( $M = 3.34$ ,  $SD = 0.71$ ,  $p = .001$ ,  $d = 0.43$ ) or positive ( $M = 3.32$ ,  $SD = 0.69$ ,  $p = .002$ ,  $d = 0.41$ ) extended contact conditions. I therefore removed participants in the neutral contact condition from all subsequent analyses.

Following the manipulation screen, participants were presented with several manipulation check questions to ensure that they had understood the article. Next, the participants completed measures of four dependent variables: outgroup attitudes, intergroup anxiety, desire for future contact, and behavioural intentions towards the outgroup. Finally, participants were shown a debriefing screen explaining the true purpose of the study, and providing them with contact links should they have any questions about the research.

### ***Measures***

*Direct valenced contact* was measured prior to the experimental manipulation using a seven-item scale derived from C. W. Stephan et al. (2000); four items tapped negative and three positive contact. Respondents were instructed as follows: "Please indicate how often you have experienced the following types of treatment from immigrants:" on the following four negative contact items: "Been made to feel unwanted", "Been verbally abused", "Been intimidated", "Been threatened with harm"; and the following three positive contact items: "Been supported", "Been helped out", "Been made to feel welcome" (1 = *Never* to 5 = *Very Often*). These items were split into independent scales for positive and negative contact for the purposes of the analysis (positive contact: Cronbach's  $\alpha = .71$ , negative contact Cronbach's  $\alpha = .73$ ).

*Extended valenced contact* was measured prior to the experimental manipulation using two items adapted from Wright, Aron, McLaughlin-Volpe, and Ropp (1997): positive extended contact, "How many Spaniards you know have had positive experiences with immigrants?", and negative extended contact, "How many Spaniards you know have had negative experiences with immigrants?" (participants provided a numerical response).

*Manipulation checks.* Three items were presented immediately following the experimental manipulation in order to test perceived extended contact valence amongst

Spaniards. Extended contact valence was measured using two items: positive valence, "How often do you think Spaniards have positive experiences with immigrants?", and negative valence, "How often do you think Spaniards have negative experiences with immigrants?" (1 = *Very rarely* to 6 = *Very often*).

*Outgroup attitudes* were measured post-manipulation using a four item version of the General Evaluation Scale (Wright et al., 1997). The items were presented in the form of bipolar scales, with the introductory text, "To what extent would you rate your feelings towards immigrants as", along with the following scale anchors: Cold/Warm, Hostile/Friendly, Contempt/Respect, Disgust/Admiration (1 = *Not at all* to 5 = *Very much*, Cronbach's  $\alpha = .78$ ).

*Intergroup anxiety* was measured using W. G. Stephan and Stephan's (1985) six item inventory as follows, "If you were the only Spanish person interacting with a group of immigrants, to what extent would you feel": nervous, anxious, comfortable (R), awkward, safe (R), at ease (R) (1 = *Not at all* to 5 = *Extremely*, Cronbach's  $\alpha = .78$ , items marked R were reverse coded).

*Desire for contact with immigrants* was measured using three items adapted from Shelton and Richeson (2005) as follows: "I would like to have more immigrant friends at work (or in class)", "I would like to get to know immigrants at work (or in class) better", "I would like to have the chance to learn more about immigrants who come to Spain by talking to them" (0 = *Totally disagree* to 6 = *Totally agree*, Cronbach's  $\alpha = .90$ ).

*Behavioural intentions towards immigrants* were measured using three items as follows, "To what extent they would be you be interested in: ...joining a voluntary organisation for improving the living conditions of immigrants", "...organising activities (e.g., sports, games) with Spanish and immigrant children, to help them to become more

integrated", "...donating money to an organisation that aims to help immigrants" (0 = *Totally disagree* to 6 = *Totally agree*, Cronbach's  $\alpha = .86$ ).

## Results

**Power analysis.** I once again produced a power vs. sample size plot for small ( $f^2 = .05$ ), medium ( $f^2 = .2$ ), and large ( $f^2 = .35$ ) effect sizes (see Figure 9) in order to check the suitability of my sample. The results of the plot indicated that the sample of 353 would be sufficient to reliably identify small, medium and large effects at a power threshold of 0.8.

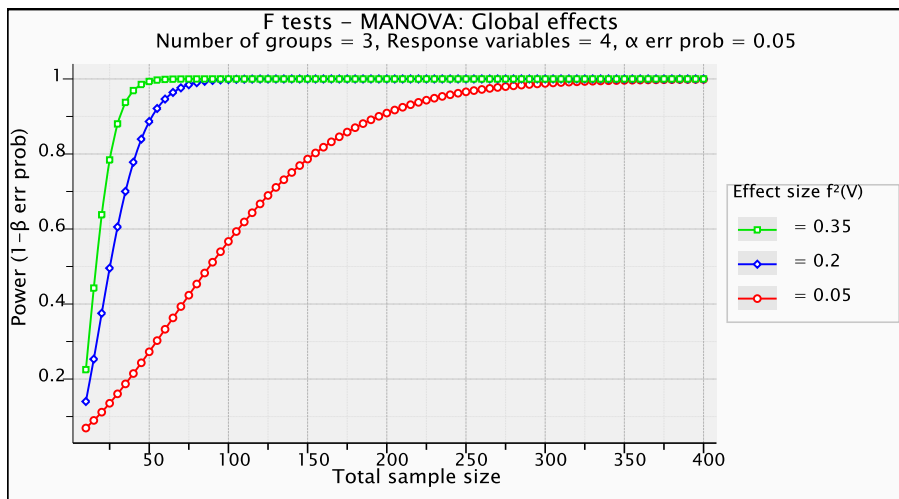


Figure 9. Plot of power against sample size for small, medium and large effects (Experiment 2)

**Preliminary results.** Descriptive statistics and correlations for the independent variables, dependent variables and manipulation check items can be found in

Table 8.

Table 8. Descriptive Statistics and Correlations (Experiment 2)

| Variable                       | Mean (SD)   | 1. | 2.     | 3.   | 4.     | 5.    | 6.     | 7.     | 8.     | 9.     | 10.    |
|--------------------------------|-------------|----|--------|------|--------|-------|--------|--------|--------|--------|--------|
| 1. Positive direct contact     | 3.29 (.71)  | -  | -.20** | .13* | -.16** | .24** | -.12*  | .25**  | -.15** | .20**  | .20**  |
| 2. Negative direct contact     | 1.75 (.67)  |    | -      | .08  | .29**  | -.06  | .29**  | -.20** | .27**  | -.14*  | -.10   |
| 3. Positive extended contact   | 8.20 (9.04) |    |        | -    | .39**  | .19** | -.04   | .20**  | -.17** | .17**  | .20**  |
| 4. Negative extended contact   | 4.39 (7.21) |    |        |      | -      | -.08  | .32**  | -.14*  | .06    | -.08   | .02    |
| 5. Extended contact positivity | 3.34 (1.15) |    |        |      |        | -     | -.32** | .33**  | -.32** | .39**  | .31**  |
| 6. Extended contact negativity | 2.70 (1.24) |    |        |      |        |       | -      | -.22** | .26**  | -.15** | -.09   |
| 7. Outgroup attitudes          | 3.73 (.66)  |    |        |      |        |       |        | -      | -.37** | .44**  | .42**  |
| 8. Intergroup anxiety          | 2.54 (.69)  |    |        |      |        |       |        |        | -      | -.24** | -.18** |
| 9. Contact desire              | 3.58 (1.38) |    |        |      |        |       |        |        |        | -      | .62**  |
| 10. Behavioural intentions     | 3.08 (1.51) |    |        |      |        |       |        |        |        |        | -      |

I tested the effectiveness of the extended contact manipulation using a one-way (Condition: positive extended contact vs. negative extended contact vs. no-contact) MANOVA with dependent variables of the manipulation check items for extended contact positivity and negativity. The omnibus test for the MANOVA produced a significant overall effect of condition,  $F(4, 700) = 14.34, p < .001$ , partial  $\eta^2 = .08$ . Individual between-subjects tests for each of the dependent variables revealed main effects of condition on extended contact positivity ratings,  $F(2, 350) = 23.34, p < .001$ , partial  $\eta^2 = .12$ , and extended contact negativity ratings,  $F(2, 350) = 15.50, p < .001$ , partial  $\eta^2 = .08$ .

Pairwise comparisons revealed that participants in the negative extended contact condition rated Spaniards as having more negative contact with immigrants ( $M = 3.17$ ,  $SD = 1.32$ ) than those in the positive extended contact ( $M = 2.31$ ,  $SD = .95$ ,  $p < .001$ ,  $d = .74$ ) and no-contact control ( $M = 2.58$ ,  $SD = 1.26$ ,  $p = .001$ ,  $d = 0.46$ ) conditions. Participants in the negative extended contact condition also rated Spaniards as having significantly less positive contact with immigrants ( $M = 2.81$ ,  $SD = 1.06$ ) than those in the positive extended contact ( $M = 3.73$ ,  $SD = 1.05$ ,  $p < .001$ ,  $d = .87$ ) and no-contact control condition ( $M = 3.52$ ,  $SD = 1.14$ ,  $p < .001$ ,  $d = .19$ ). No other comparisons were significant.

***Effects of indirect contact manipulation.*** I tested the effects of the extended contact manipulation on the four dependent variables using a one-way MANOVA (Condition: positive extended contact vs. negative extended contact vs. no-contact control), with outgroup attitudes, intergroup anxiety, contact desire and behavioural intentions. The omnibus test revealed an overall main effect of condition,  $F(8, 696) = 15.27$ ,  $p < .001$ , partial  $\eta^2 = .15$ . The between-subjects tests also produced significant main effects of condition for outgroup attitudes,  $F(2, 350) = 43.72$ ,  $p < .001$ , partial  $\eta^2 = .20$ , intergroup anxiety,  $F(2, 350) = 42.50$ ,  $p < .001$ , partial  $\eta^2 = .20$ , contact desire,  $F(2, 350) = 7.97$ ,  $p < .001$ , partial  $\eta^2 = .04$ , and behavioural intentions,  $F(2, 350) = 8.63$ ,  $p < .001$ , partial  $\eta^2 = .05$ . Pairwise comparisons revealed that participants in the negative extended contact condition reported significantly less favourable outgroup attitudes, higher levels of intergroup anxiety, lower levels of contact desire, and less positive behavioural intentions than participants in the other two conditions. Participants in the no-contact control condition also reported lower levels of outgroup attitudes than those in the positive extended contact condition (see Table 9 for all significant comparisons). A  $z$ -comparison of the effect sizes for differences in outgroup attitudes between the positive vs. no-contact and negative vs. no-contact conditions showed that the

negative-neutral difference was significantly larger than the positive-neutral difference,  $z = 4.57, p < .001$ .

*Table 9. Pairwise Comparisons Between Contact Conditions (Experiment 2 - Significant Results Only)*

| DV                     | Contact condition | Mean (SD)      | Comparison condition | Mean (SD)      | <i>p</i> | <i>d</i> |
|------------------------|-------------------|----------------|----------------------|----------------|----------|----------|
| Outgroup attitudes     | <b>Negative</b>   | 3.34<br>(0.51) | vs. Positive         | 4.05<br>(0.60) | < .001   | 1.28     |
|                        |                   |                | vs. No-contact       | 3.84<br>(0.66) | < .001   | 0.85     |
|                        | <b>Positive</b>   | 4.05<br>(0.60) | vs. No-contact       | 3.84<br>(0.66) | .025     | 0.33     |
| Intergroup anxiety     | <b>Negative</b>   | 2.96<br>(0.55) | vs. Positive         | 2.28<br>(0.57) | < .001   | 1.21     |
|                        |                   |                | vs. No-contact       | 2.37<br>(0.71) | < .001   | 0.93     |
| Contact desire         | <b>Negative</b>   | 3.18<br>(1.33) | vs. Positive         | 3.77<br>(1.23) | .003     | 0.46     |
|                        |                   |                | vs. No-contact       | 3.79<br>(1.47) | .001     | 0.44     |
| Behavioural intentions | <b>Negative</b>   | 2.62<br>(1.43) | vs. Positive         | 3.29<br>(1.54) | .002     | 0.45     |
|                        |                   |                | vs. No-contact       | 3.32<br>(1.46) | .001     | 0.48     |

***Interaction between prior valenced contact and extended contact manipulations.*** In order to test the possibility of participants' prior direct positive and negative contact experiences moderating the effect of the extended contact manipulation, I carried out a one-way MANCOVA (Condition: positive extended contact vs. negative extended contact vs. no-contact control) with the dependent variables outgroup attitudes, intergroup anxiety, contact desire and behavioural intentions, as well as two covariates – prior direct positive and negative contact. The model was specified to include covariate interactions for prior positive contact\*condition, and prior negative contact\*condition. The omnibus tests for this model produced significant main effects of positive contact,  $F(4, 341) = 9.51, p < .001$ , partial  $\eta^2 = .10$ , and negative contact,  $F(4, 341) = 7.13, p < .001$ , partial  $\eta^2 = .08$ , as well as a significant

interaction between positive contact and condition,  $F(8, 684) = 3.27, p = .006$ , partial  $\eta^2 = .04$ . The individual between-subjects effects for each dependent variable are shown in Table 10.

*Table 10. Between-subjects Effects for MANCOVA of Extended Contact Manipulation with Prior Valenced Contact.*

| Effect   | Dependent variable     | F     | df     | p      | Partial $\eta^2$ |
|--|------------------------|-------|--------|--------|------------------|
| Main effect of positive contact                          | Outgroup attitudes     | 28.18 | 1, 344 | < .001 | .08              |
|  | Intergroup anxiety     | 5.82  |        | .016   | .02              |
|  | Contact desire         | 14.17 |        | < .001 | .04              |
|  | Behavioural intentions | 15.95 |        | < .001 | .04              |
| Main effect of negative contact                          | Outgroup attitudes     | 9.04  | 1, 344 | .003   | .03              |
|  | Intergroup anxiety     | 22.16 |        | < .001 | .06              |
|  | Contact desire         | 6.19  |        | .056   | .01              |
|  | Behavioural intentions | 1.04  |        | .309   | .003             |
| Interaction between condition and prior positive contact | Outgroup attitudes     | 7.63  | 3, 344 | .001   | .04              |
|  | Intergroup anxiety     | 2.72  |        | .067   | .02              |
|  | Contact desire         | 4.47  |        | .012   | .03              |
|  | Behavioural intentions | 6.46  |        | .002   | .04              |

In order to explore the interaction between positive contact and condition, I ran a series of pairwise comparisons for condition at different levels of prior positive contact (mean, +1SD, and -1SD). At mean and high levels of prior positive contact, the pattern of significant pairwise comparisons was similar to that of the previous non-covariate model, with participants in the negative extended contact condition reporting less positive outgroup attitudes, less desire for contact and less positive behavioural intentions than those in the positive extended contact, neutral extended contact, or no-contact control conditions (see Table 11 and Table 12). However, at low levels of prior positive contact, the pairwise comparisons for contact desire and behavioural intentions became non-significant (see

Table 13).

*Table 11. Pairwise Comparisons for Condition\*Valenced Contact MANCOVA at Mean Levels of Positive Contact (Experiment 2)*

| DV                     | Contact condition | Estimated mean (SD) | Comparison condition | Mean (SD)   | <i>P</i> | <i>d</i> |
|------------------------|-------------------|---------------------|----------------------|-------------|----------|----------|
| Outgroup attitudes     | Negative          | 3.34 (0.55)         | vs. Positive         | 4.02 (0.56) | < .001   | 1.23     |
|                        |                   |                     | vs. No-contact       | 3.87 (0.56) | < .001   | 0.96     |
| Contact desire         | Negative          | 3.18 (1.30)         | vs. Positive         | 3.73 (1.31) | .005     | 0.42     |
|                        |                   |                     | vs. No-contact       | 3.84 (1.30) | < .001   | 0.51     |
| Behavioural intentions | Negative          | 2.63 (1.42)         | vs. Positive         | 3.25 (1.43) | .003     | 0.44     |
|                        |                   |                     | vs. No-contact       | 3.37 (1.43) | < .001   | 0.52     |

*Table 12. Pairwise Comparisons for Condition\*Valenced Contact MANCOVA at High (+1SD) Levels of Positive Contact (Experiment 2)*

| DV                     | Contact condition | Estimated mean (SD) | Comparison condition | Mean (SD)   | <i>P</i> | <i>d</i> |
|------------------------|-------------------|---------------------|----------------------|-------------|----------|----------|
| Outgroup attitudes     | Negative          | 3.34 (0.76)         | vs. Positive         | 4.26 (0.78) | < .001   | 1.20     |
|                        |                   |                     | vs. No-contact       | 4.12 (0.84) | < .001   | 0.98     |
| Contact desire         | Negative          | 3.16 (1.78)         | vs. Positive         | 4.21 (1.83) | < .001   | 0.58     |
|                        |                   |                     | vs. No-contact       | 4.18 (1.97) | < .001   | 0.55     |
| Behavioural intentions | Negative          | 2.57 (1.94)         | vs. Positive         | 3.87 (2.01) | < .001   | 0.66     |
|                        |                   |                     | vs. No-contact       | 3.75 (2.15) | < .001   | 0.58     |

*Table 13. Pairwise Comparisons for Condition\*Valenced Contact MANCOVA at Low (-1SD) Levels of Positive Contact (Experiment 2)*

| DV                     | Contact condition | Estimated mean (SD) | Comparison condition | Mean (SD)   | <i>P</i> | <i>d</i> |
|------------------------|-------------------|---------------------|----------------------|-------------|----------|----------|
| Outgroup attitudes     | Negative          | 3.35 (0.82)         | vs. Positive         | 3.78 (0.82) | < .001   | 0.54     |
|                        |                   |                     | vs. No-contact       | 3.63 (0.76) | .018     | 0.35     |
| Contact desire         | Negative          | 3.20 (1.91)         | vs. Positive         | 3.24 (1.92) | 1.000    | 0.02     |
|                        |                   |                     | vs. No-contact       | 3.51 (1.78) | .58      | 0.17     |
| Behavioural intentions | Negative          | 2.69 (2.09)         | vs. Positive         | 2.64 (2.10) | 1.000    | 0.02     |
|                        |                   |                     | vs. No-contact       | 2.99 (1.95) | .72      | 0.15     |

Just as in Experiment 1, the overall effects of condition in Experiment 2 seem to be driven by the negative contact manipulation. Participants in the negative extended contact condition reported more negative and less positive extended contact on the manipulation check items, as well as less positive outgroup attitudes, less contact desire, less positive behavioural intentions towards the outgroup, and higher levels of intergroup anxiety compared to the two other experimental conditions.

The fact that the results of the manipulation check only showed significant differences involving the negative condition (i.e., no significant differences in manipulation check items between the positive contact and no-contact control conditions) once again demonstrates that a major methodological issue for valenced contact experiments is providing sufficiently strong positive contact manipulations. Given the lack of positive contact effects in the

manipulation checks, I was somewhat surprised to find a significant difference between the positive contact and control conditions for participants outgroup attitudes scores. This was, however, the only positive contact effect to emerge from the data, and was found to be significantly smaller than the equivalent negative contact effect (i.e., the difference in outgroup attitudes between the negative contact and control conditions). No positive contact effects were found for any of the other dependent variables. The lack of significant positive extended contact effects means that the data from Experiment 2 are limited in their usefulness as a response to Barlow et al.'s (2012) claim that negative contact is stronger than positive contact. In order to compare the strengths of valenced contact effects, both positive and negative contact first need to be successfully induced. It should also be noted that the existence of positive extended contact effects is well established, and so their absence here most likely represents a methodological issue with the current study, not a strike against the extended contact hypothesis itself.

What can be claimed from these data, however, is that negative extended contact effects may be easier to elicit than positive extended contact effects. Whether because of the relative importance of information regarding negative outgroup behaviour (see Baumeister et al., 2001; S. E. Taylor, 1991), or due to their increased perceived fit with outgroup negative stereotypes (see Paolini, Harwood, & Rubin, 2010), negative extended contact effects were elicited in this experiment using a relatively weak contact manipulation, whilst positive extended contact effects were not. The same can be said of Experiment 1, where I was able to elicit a significant negative imagined contact effect, but not a positive equivalent using closely matched stimuli (stimuli which were, it should be noted, near identical to those used by other researchers to produce significant positive imagined contact effects). Whether or not the effects of negative contact are 'stronger', as Barlow et al. (2012) put it (that is, producing a larger change in attitudes), the experimental data I have collected thus far suggest that they

can be elicited more easily than the effects of positive contact. It is interesting to note that this finding is similar to a pattern observed in the literature on mood induction, wherein positive mood is typically harder to induce than negative mood (for a meta-analysis, see Westermann, Spies, Stahl, & Hesse, 1996).

Moving on from the question of relative effect sizes, in Experiment 2 I was able to test whether participants' prior direct experiences with immigrants moderated the effects of the extended contact manipulation on the various outcome measures. I found a significant interaction between prior direct positive contact and experimental condition for all dependent variables except intergroup anxiety. However, the profile of these interactions did not precisely match anything I had previously observed in my survey data. Specifically, when levels of prior direct positive contact were low, the differences in contact desire and behavioural intentions between the negative extended contact and the other two experimental conditions were reduced to non-significance. Although the effects of negative contact on outgroup attitudes remained significant, the sizes of the effects were reduced at low levels of prior positive direct contact. To summarise, as levels of prior positive direct contact decreased, the effect of negative extended contact on outgroup attitudes, contact desire and behavioural intentions also decreases. In terms of my four interaction hypotheses, this finding matches the negative augmentation hypothesis (although I have usually stated it in the opposite direction, i.e., the effects of negative contact increasing when levels of positive contact are high).

As for an interpretation of this apparent negative augmentation effect, it is possible that if a person has had few direct positive experiences with the outgroup, their expectations of outgroup behaviour will be less positive. If that person then becomes aware of fellow ingroup members having negative experiences with the outgroup, this may have less of an effect on their attitudes and behavioural inclinations because the new information does not

deviate significantly from their own experience. Although this provides some experimental corroboration for the negative augmentation effect found in Study 4, my concerns about the validity of the extended contact manipulation itself mean that I am reluctant to draw too many conclusions from the results of Experiment 2 before they have been replicated.

### **Experiment 3**

One of the main goals of my experimental tests of valenced contact was to provide a point of methodological triangulation to support the survey-based approach taken in my other studies. Experiments 1 and 2 may have provided some indication of the causal origins of valenced contact effects by showing that negative contact effects are easier to elicit than positive contact effects. However, the direct relevance of these findings to my previous survey studies was limited because Experiments 1 and 2 involved the manipulation of indirect forms of contact (imagined and extended contact respectively). In order to provide true methodological triangulation with my survey data, I needed to conduct an experimental manipulation of direct valenced contact. This was the goal of Experiment 3.

In designing a direct contact paradigm that would allow for realistic conditions of both positive and negative contact, I had several requirements in mind. First and foremost, I needed the negative contact condition to be believable. Given that the contact manipulation would necessarily involve interaction with an outgroup member (or a confederate posing as one), I expected to find that participants would be less inclined to believe that their interaction partner was genuine if they behaved in a negative fashion. There is an expectation that most people recruited to participate in experiments do not go out of their way to be unpleasant to those they are participating with. This meant designing an interaction scenario in which negative behaviour was plausible, but not necessarily excusable based on the conditions of the study.

Secondly, I wanted the content of the interaction to be fairly tightly controlled, in order to ensure parity of experience across participants. Thirdly, I wanted participants to feel confident that they were interacting with a real person (rather than simply being told that they were interacting with an unseen partner via online instant messaging for example). With these

conditions in mind, I designed and piloted a paradigm based on the study by Wilder (1984), in which participants were ostensibly invited to take part in an 'indirect collaboration task' with a member of a rival university. During the collaboration task, participants completed several short persuasive writing tasks and received written feedback from their partner, who was seated in a different cubicle. In order to manipulate the valence of the contact interaction, the feedback could either be positive, negative, or neutral. Using this approach, I was able to produce a contact valence manipulation which, unlike those used in Experiments 1 and 2, elicited equivalent ratings of intensity (i.e., the differences in participants' positivity/negativity ratings of the feedback were equal for the positive vs. neutral and negative vs. neutral conditions - see Results below). This allowed me to more confidently attribute any effects of condition to contact valence effects, as opposed to stimulus confounds.

## **Method**

***Design and respondents.*** Seventy-seven undergraduate students (51 female) aged between 17 and 26 ( $M = 19.81$ ,  $SD = 1.76$ ) participated in the experiment. Participants were randomly allocated to one of three conditions: positive direct contact, negative direct contact, or a neutral contact control condition. Outgroup attitudes were measured in a separate pre-test session, and immediately post-manipulation.

***Procedure.*** The experiment was advertised as a study of indirect cooperation and collaboration. Upon registering to participate, all participants completed a pre-test questionnaire which included (amongst several distraction items) baseline attitudes towards Oxford Brookes students. Following completion of the pre-test questionnaire, participants were invited to a lab session in the Department of Experimental Psychology. Upon arrival in the department, participants were met by a researcher, who also identified a confederate

waiting in reception as a second participant. Both the confederate and the participant were brought to a testing room and seated in separate cubicles, connected by a closed door.

Once inside their cubicles participants were provided with a packet containing the experiment materials, including an instruction sheet which explained that their task in the study was to collaborate with their partner without receiving direct visual or verbal feedback. According to the instructions, the participants had been randomly assigned to the role of the ‘writer’ in the first half of the study. As the writer, they were asked to answer five short written questions, which required them to provide persuasive arguments on the topic of animal testing (for questions, see Figure 11). The instructions stated that after completing each of the first four questions (each under a 90 second time limit), the participants should pass their answers under the door of the cubicle to their partner, who was taking the role of ‘marker’ and would provide brief written feedback. For the fifth and final question, no feedback would be provided. Instead, the instructions explained that after the participant had completed the writing task, they would swap roles with their partner for the second half of the study. Once both ‘participants’ had completed the writing task, their answers to Question 5 would be jointly assessed by an external marker to provide an overall score for their pair. This score would be compared to other participant pairs, with the best five pairs winning a £20 Amazon voucher.

After explaining the task, the instructions asked participants to complete a sheet with some basic information to exchange with their partner. As soon as the participant completed their information sheet and passed it under the door, the researcher positioned outside the cubicle passed a pre-completed version of the sheet back to the participant with information about their supposed partner. The completed version of this sheet is shown in Figure 10.

After giving participants a few minutes to read through their partner's information, the researcher knocked on the door of the cubicle to signal the start of the writing task. Participants were given 90 seconds to complete the first question. After the 90 seconds had elapsed, the researcher once again knocked on the door, and the participants were instructed to place their answer booklet in an opaque folder and pass it under the door to their partner for feedback. At this point, the researcher completed the appropriate section of the feedback form according to the participant's randomly allocated condition (see Figure 12), and posted the answer booklet back under the door inside the folder. Participants were given a few moments to digest their feedback, before the researcher knocked on the cubicle door to signal the start of the next question. Each subsequent question followed the same procedure, with the exception of Question 5 (ostensibly the question that was to be marked by external judges alongside the partner's answer, to potentially win a prize), for which participants were given 120 seconds, and did not receive feedback

Partner Details

First Name: Hannah

Gender: F

University (and college, if applicable): Oxford Brookes

To what extent is your university membership an important aspect of your identity?

1    2    3    4    5    6    7    8    (9)    10

To what extent do you think you are a typical member of your university?

1    2    3    4    5    6    7    (8)    9    10

Degree course: Business & Marketing Management

Age: 21

First language: English

Number of siblings & their ages: 1 sister (14)

To what extent do you consider yourself to be good at working with a partner?

1    2    3    4    5    6    (7)    8    9    10

To what extent do you consider yourself to be academic?

1    2    3    4    5    (6)    7    8    9    10

Figure 10. Partner information sheet (Experiment 3)

**Question 1: List as many arguments as you can for animal testing.**

Please write as clearly as possible so the marker can read your response. You will have 90 seconds. When the experimenter knocks on the door, please place this whole booklet in the green folder and pass it under the door.

**Question 2: Pick one of your arguments from Question 1. In the space below, present counter-arguments to that point.**

You will have 90 seconds. When the experimenter knocks on the door, please place this whole booklet in the green folder and pass it under the door.

**Question 3: List as many arguments as you can against using animals for food.**

You will have 90 seconds. When the experimenter knocks on the door, please place this whole booklet in the green folder and pass it under the door.

**Question 4: Pick one of your arguments from Question 3. In the space below, present counter-arguments to that point.**

You will have 90 seconds. When the experimenter knocks on the door, please place this whole booklet in the green folder and pass it under the door.

**Question 5: Write a short persuasive argument against the use of animals for sports and entertainment.**

You have two minutes for this answer. **REMEMBER:** You will not receive feedback on this answer. This answer will be taken in by the researcher and marked to provide part of your pair's joint score.

*Figure 11. Persuasive arguments task (Experiment 3)*



After completing the writing task, the researcher entered the cubicle and asked the participant to fill out a short questionnaire on their experiences in the first half of the study. This questionnaire included manipulation check ratings for the valence of partner feedback and the overall experience, as well as post-test measures of attitudes towards Oxford Brookes students, and a typicality rating for the contact partner. After completing the questionnaire, the researcher returned to the cubicle to inform the participants that the study was over, and to provide a full debriefing and reimbursement (either financial or course credit).

### ***Measures***

*Outgroup attitudes* were measured at both pre- and post-test using a four-item version of the General Evaluation Scale (Wright et al., 1997). The items were presented in the form of bipolar scales, with the introductory text, “To what extent would you rate your feelings towards Oxford Brookes students as”, along with the following scale anchors: Cold/Warm, Hostile/Friendly, Contempt/Respect, Disgust/Admiration (scale from 0-100). Participants were also provided with a ‘No Response’ option. For the purposes of analysis, the items were collapsed into a single mean score, T1 Cronbach’s  $\alpha = .75$ , T2 Cronbach’s  $\alpha = .90$ .

*Prior direct valenced contact* was measured using an extended 10-item version of the scale used Experiment 2, which included five negative contact items: "How often have you been made to feel unwanted by Oxford Brookes students", "How often have you been verbally abused by Oxford Brookes students", "How often have you been intimidated by Oxford Brookes students", "How often have you been threatened with harm by Oxford Brookes students", “How often have you been ridiculed by Oxford Brookes students”; and the following five positive contact items: "How often have you been supported by Oxford Brookes students", "How often have you been made to feel welcome by Oxford Brookes students", "How often have you been helped by Oxford Brookes students", “How often have you been complemented by Oxford Brookes students”, “How often have you been befriended

by Oxford Brookes students” (1 = *Never* to 5 = *Very Often*). These items were split into independent scales for positive and negative contact for the purposes of the analysis (positive contact: Cronbach's  $\alpha = .90$ , negative contact Cronbach's  $\alpha = .84$ ).

*Partner feedback rating.* The post-test questionnaire included a single item measuring partner feedback valence, “On average, how positive did you feel the feedback your partner gave you was?” (Scale: 0-100).

*Contact experience rating.* The post-test questionnaire also included a single item measuring participants’ overall rating of the experience after completing the writing task, “How would you rate your collaboration experience so far?” (1 = ‘*Very bad*’ – 7 = “*Very good*”).

*Partner typicality.* The final manipulation check item in the post-test questionnaire was a single measure of perceived partner typicality, “To what extent do you think your partner is a typical member of their university?” (1 = “*Not at all typical*” – 7 = “*Highly typical*”).

## **Results**

*Power analysis.* In order to determine the suitability of this sample for the planned analysis, I produced a power vs. sample size plot for small ( $f^2 = .05$ ), medium ( $f^2 = .2$ ), and large ( $f^2 = .35$ ) effect sizes (see Figure 13). The results of the plot suggested that the sample of 77 would be sufficient to reliably (power threshold 0.8) identify medium and large effects.

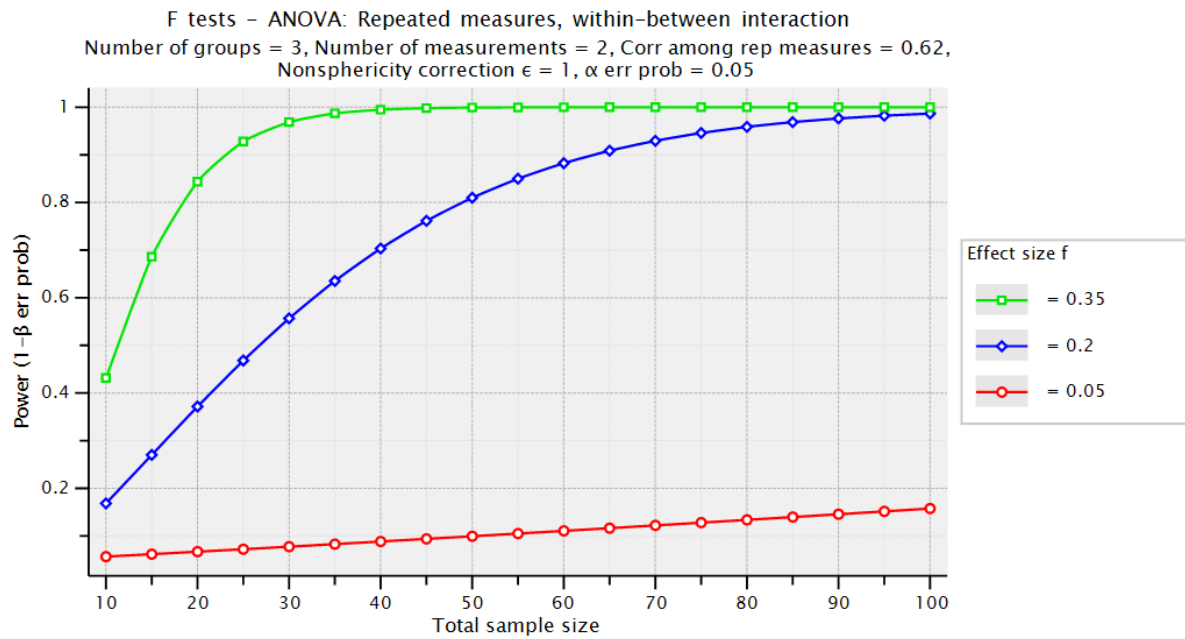


Figure 13. Plot of power against sample size for small, medium and large effects (Experiment 3)

**Preliminary results.** Means, standard deviations and correlations between variables are shown in Table 14. I tested for random allocation of participants to my experimental conditions using a one-way (Condition: positive contact vs. negative contact vs. neutral contact) MANOVA with T1 outgroup attitudes, positive contact, negative contact, participant age and gender as dependent variables. The main effect of condition was non-significant.

Table 14. Correlations, Means and Standard Deviations of T1 Study Variables (Experiment 3)

| Variable              | Mean (SD)     | 1. | 2.   | 3.    | 4.    | 5.    | 6.    |
|-----------------------|---------------|----|------|-------|-------|-------|-------|
| 1. Positive contact   | 2.23 (1.12)   | -  | .23* | .36** | .018  | -.039 | -.033 |
| 2. Negative contact   | 1.33 (0.51)   |    | -    | -.17  | -.005 | -.045 | -.21  |
| 3. Outgroup attitudes | 68.44 (15.57) |    |      | -     | .072  | -.057 | -.003 |
| 4. Partner feedback   | 50.16 (26.77) |    |      |       | -     | .80** | .12   |
| 5. Contact experience | 4.53 (1.41)   |    |      |       |       | -     | .12   |
| 6. Partner typicality | 4.63 (1.15)   |    |      |       |       |       | -     |

I ran manipulation checks using both contact experience and partner feedback ratings to test the effects of the positive and negative conditions vs. the neutral control. These took the form of two separate one-way (Condition: positive contact vs. negative contact vs. neutral contact) ANOVAs with dependent variables of contact experience and partner feedback ratings respectively. The contact experience ANOVA produced a main effect of condition,  $F(2, 74) = 40.37, p < .001, \text{partial } \eta^2 = .52$ . Pairwise comparisons showed that participants rated their experiences in the positive contact condition ( $M = 5.81, SD = .80$ ) significantly more positively than those in the neutral contact condition ( $M = 4.46, SD = 1.10, p < .001, d = 1.41$ ), and those in the negative contact condition ( $M = 3.37, SD = 1.04, p < .001, d = 2.62$ ). Participants in the neutral contact condition also rated their experience significantly more positively than those in the negative contact condition ( $p = .001, d = 1.02$ ). Finally, I compared the magnitude of the positive vs. neutral and negative vs. neutral condition

differences using a z-comparison, finding no significant differences between the two effect sizes,  $z = .97, p = .33$ .

The partner feedback ANOVA also produced a main effect of condition,  $F(2, 70) = 107.49, p < .001$ , partial  $\eta^2 = .75$ . Pairwise comparisons revealed that participants rated their feedback in the positive contact condition significantly more positively ( $M = 76.12, SD = 14.12$ ) than both the neutral contact condition ( $M = 53.38, SD = 10.58, p < .001, d = 1.80$ ), and the negative contact condition ( $M = 21.62, SD = 14.77, p < .001, d = 2.43$ ). Participants also rated feedback in the neutral contact condition significantly more positively than the negative contact condition ( $p < .001, d = 2.43$ ). Comparing the magnitude of the positive vs. neutral and negative vs. neutral condition differences using a z-comparison revealed no significant difference between the effect sizes,  $z = .12, p = .91$ .

***Effect of direct contact manipulation on perceived typicality.*** In order to test Paolini, Harwood, and Rubin's (2010) hypothesis that negative contact produces greater perceived fit with existing outgroup stereotypes (and thus greater category salience during contact interactions), I conducted a one-way (Condition: positive contact vs. neutral contact vs. negative contact) ANOVA with partner typicality ratings as the dependent variable. However, the main effect of condition in this analysis was non-significant.

***Effect of direct contact manipulation on outgroup attitudes.*** In order to test the extent to which the direct contact manipulation changed participants' attitudes towards Oxford Brookes students, I ran a 3 (Condition: positive contact vs. neutral contact vs. negative contact) X 2 (Time: pre-test vs. post-test) mixed-model ANOVA with repeated measures on the second factor and outgroup attitudes as the dependent variable. This analysis produced no significant main effects, but did show a significant interaction between time and condition,  $F(2, 63) = 9.07, p < .001$ , partial  $\eta^2 = .22$ . Simple main effects analysis revealed

that although there were no significant differences in pre-test attitudes between the different conditions, a significant difference did emerge in post-test attitudes between participants in the positive ( $M = 72.11$ ,  $SD = 13.29$ ) and negative ( $M = 62.12$ ,  $SD = 12.87$ ,  $p = .038$ ,  $d = 0.76$ ) contact conditions. There were also significant differences between pre- and post-test attitudes for participants in all three conditions. Attitudes amongst participants in the positive contact condition were significantly higher at post-test ( $M = 72.11$ ,  $SD = 13.29$ ) than at pre-test ( $M = 64.84$ ,  $SD = 16.26$ ,  $p = .003$ ,  $d = 0.49$ ). Conversely, participants in the neutral contact condition showed significantly lower attitudes at post-test ( $M = 69.41$ ,  $SD = 13.43$ ) than at pre-test ( $M = 75.21$ ,  $SD = 12.75$ ,  $p = .029$ ,  $d = 0.44$ ). A similar pattern was observed for those in the negative contact condition, with less positive attitudes at post-test ( $M = 62.12$ ,  $SD = 12.87$ ) than at pre-test ( $M = 67.05$ ,  $SD = 16.13$ ,  $p = .051$ ,  $d = 0.34$ ). Comparing these pre-/post-test differences using z-comparisons revealed no significant differences in effect size.

The results of Experiment 3 demonstrated significant effects of all three contact manipulations on participants' outgroup attitudes. The positive contact condition produced a significant improvement in attitudes from pre- to post-test, whilst the negative and neutral conditions resulted in a significant decrease in positive outgroup attitudes. Critically, there were no significant differences in effect size between the contact effects for each of the three conditions. This provides counter evidence to Barlow et al.'s (2012) claim that negative contact is stronger than positive contact. Furthermore, I was able to demonstrate that there was no difference in effect size between the positive vs. neutral, and negative vs. neutral conditions in terms of participants' ratings of overall experience, and partner feedback. This suggests that the positive and negative manipulations were of equal intensity. Compared to the results of Experiments 1 and 2 where only the negative contact condition elicited any significant effect on both manipulation checks and outgroup attitudes, Experiment 3 indicates

that equivalently intense positive and negative contact experiences produce equivalently sized effects on outgroup attitudes.

There are a number of caveats to this finding however. Firstly, my neutral contact manipulation produced a significant reduction in positive outgroup attitudes from pre- to post-test. A possible explanation for this is that, in the context of a supposed collaboration task, providing neutral feedback could be seen as slightly negative behaviour. It is also possible that my participants, coming as they did from an academically prestigious institution, might consider midpoint scores of 3/5 as low, and therefore treat them as negative feedback. If this were the case, it might indicate that the lack of difference in effect sizes between the positive-neutral and negative-neutral comparisons was due to the neutral condition being negatively skewed, artificially increasing the apparent effectiveness of the positive condition, and reducing that of the negative condition. It should be noted that the mean scores on overall experience and partner feedback ratings for participants in the neutral contact condition were not significantly different from the scale midpoint (contact experience rating midpoint = 4,  $t(20) = 1.92, p = .070$ ; partner feedback rating midpoint = 50,  $t(20) = 1.46, p = .16$ ). It is interesting therefore that this apparently genuinely neutral experience produced a negative effect on outgroup attitudes. When using this paradigm in future, I would aim to include a no-contact control condition in order to compare the effects of some contact (positive, negative or neutral) vs. no contact.

***Effect of prior valenced contact experience on direct contact manipulation.*** In order to test the possible moderation of the contact manipulation by prior positive or negative contact experiences, I repeated the previous ANOVA, adding in pre-test positive and negative contact as covariates. I specified that the general linear model applied by the ANCOVA should include the covariate interactions between time, condition, and prior positive/negative

contact. However, this analysis produced no significant interaction between condition and prior valenced contact, or between condition, time and prior valenced contact.

### **Summary of Experimental Research**

In all three experimental studies, I have found some effect of negative contact on outgroup attitudes. In Experiment 1, participants in the negative imagined contact condition reported significantly less positive attitudes towards the homeless immediately following the imagined interaction than those in the positive or neutral contact conditions. In Experiment 2, participants in the negative extended contact condition reported less positive attitudes towards immigrants (as well as higher levels of intergroup anxiety, less positive behavioural intentions and reduced desire for future contact) than those in the positive or no-contact control conditions. In Experiment 3, participants in the negative direct contact condition reported less positive attitudes towards members of a rival university following the experimental manipulation than those in the positive contact condition. Combined with the fact that in Experiments 1 and 2 I only found significant effects of the negative contact manipulations on my manipulation check variables, these results may indicate that negative contact is easier to elicit than positive contact.

However, several caveats apply to these experimental data. In Experiment 1, there were no significant differences in the pre-/post-test attitude change between the positive and negative contact conditions. Furthermore, at a follow-up test seven days after the experimental session, the difference in attitudes between participants in the positive and negative conditions was no longer significant. In Experiment 2, I found no significant difference between attitude comparisons for the positive/neutral and negative/neutral conditions, indicating that the effect of negative contact was not significantly larger than that of positive contact. The same pattern was found in Experiment 3, in which I also found a

reduction in outgroup attitudes from pre- to post-test for participants in the neutral contact condition (calling into question its validity as a truly neutral contact experience).

Finally, in Experiment 2 I found evidence for a significant interaction between prior direct positive contact and the experimental manipulation of extended contact. Specifically, I found significant effects of the negative extended contact condition on attitudes, contact desire and behavioural intentions for participants with medium (mean) and high (+1 SD) levels of prior direct positive contact, but no effect of the negative extended contact condition for participants with low (-1 SD) levels of prior direct positive contact. This finding provides some support for the negative augmentation hypothesis, although it must be noted that the effect size for the interaction was very small.

## CHAPTER SIX: DISCUSSION AND FUTURE DIRECTIONS

### General Discussion

In the quotation that introduces this thesis, Allport asserted that the concept of mere contact as a means to reduce prejudice was "not so simple" (Allport, 1954, p. 261). As I have conducted my research into negative contact and its effects, I have found that Allport's caveat applies here also. Contrary to the concerns put forward by some authors (e.g., Barlow et al., 2012), I have found that negative contact is not always stronger than positive contact in its effects on intergroup relations. Furthermore, I have found repeated evidence that positive and negative contact interact; that is, the psychological effects of one are moderated by the presence of the other. To reiterate, the interaction hypotheses I sought to test were as follows:

#### **Competitive hypotheses 1:**

*1a. The buffering hypothesis:* positive contact moderates the relationship between negative contact and attitudes such that more positive contact reduces the effects of negative contact.

*1b. The positive augmentation hypothesis:* negative contact moderates the relationship between positive contact and attitudes such that more negative contact increases the prejudice-reducing effects of positive contact.

#### **Competitive hypotheses 2:**

*2a. The poisoning hypothesis:* negative contact moderates the relationship between positive contact and attitudes such that more negative contact reduces the effects of positive contact.

*2b. The negative augmentation hypothesis:* positive contact moderates the relationship between negative contact and attitudes such that more positive contact increases the effects of negative contact.

Within my survey data, the interaction effects I found indicated that, far from being undermined by the 'caveat' imposed by negative contact (the term used in Barlow et al.'s, 2012, paper), positive contact was more important than ever. Not only did positive contact buffer against the detrimental effects of negative contact (see Studies 1-3), but its own positive effects appeared to be augmented when levels of negative contact were high (both at the individual level in Studies 1-3, and at the neighbourhood level in Study 4).

However, the picture emerging from my research does not unequivocally support the buffering and positive augmentation hypotheses. Specifically, I was unable to replicate these interaction effects using an experimental approach (though I did find some evidence for a negative augmentation effect in Experiment 2). Furthermore, although my experimental data did not directly support the claim that the effects of negative contact are larger than those of positive contact, the results I gathered did suggest that negative contact may be easier to elicit than positive contact. Generalising this finding beyond the laboratory may therefore indicate that the threshold for perceiving an interaction with an outgroup member as negative contact may be lower than for positive contact. If this were the case, it would lend weight to the broader concern raised by Barlow et al. (2012) and others that the opportunities for contact brought about by social diversity might lead to a net negative shift in outgroup attitudes. To summarise, once again, the case is not so simple.

### **Summary of results**

In Study 1, I found a significant main effect of positive contact on readiness for reconciliation amongst Serbian, Bosniak and Croat respondents from three cities in former

Yugoslavia. For negative contact, however, only Croats showed a significant effect on readiness for reconciliation. The negative contact effect was not significantly larger than that of positive contact. Only Serbs showed a significant interaction between positive and negative contact. Decomposing the Serbian interaction effect revealed that the effect of negative contact was reduced when positive contact was high (or that the effect of positive contact was increased when negative contact was high), consistent with the buffering/positive augmentation hypothesis.

In Study 2, I found significant main effects of positive and negative contact on outgroup attitudes for both Protestant and Catholic respondents from six towns in Northern Ireland. For Protestant respondents, the negative contact effect was significantly larger than that of positive contact, but for Catholic respondents there was no significant difference in effect size. Both respondent groups showed significant valenced contact interaction effects, which broke down similarly to that of Serbs in Study 1 (i.e., indicating buffering/positive augmentation).

In Study 3, I found significant cross-lagged effects of T1 positive contact on T2 outgroup attitudes for Catholic and Protestant respondents from four areas of Belfast, but only Protestants showed a significant effect of T1 negative contact on T2 attitudes. In the subsequent structural model, T2 (but not T1) positive and negative contact were significantly associated with T2 attitudes for respondents of both ethnoreligious groups. There were no significant differences in effect size between positive and negative contact for any of the models or respondents. Amongst Protestant respondents, there were significant interactions for T1 positive contact \* T2 negative contact (which produced a buffering effect), and for T1 negative contact \* T2 positive contact (which produced a positive augmentation effect).

In Study 4, I found significant main effects of individual-level positive and negative contact on White British respondents' trust towards both Black and Asian outgroup members. There were no significant differences in the sizes of these effects. For both Asian and Black target outgroups, I found significant interactions between neighbourhood-level negative contact and individual-level positive contact. Decomposing these interactions revealed a contextual positive augmentation effect, such that individual-level positive contact had a larger positive effect on outgroup trust when neighbourhood-level negative contact was high. I also found a significant interaction between neighbourhood-level positive contact and individual-level negative contact with Black outgroupers. Decomposing this effect revealed a contextual negative augmentation effect, such that the effect of individual-level negative contact was larger when neighbourhood-level positive contact was high.

In Experiment 1, I found that participants in the negative imagined contact condition reported significantly less positive attitudes towards homeless people immediately following the imagined interaction than those in the neutral or positive contact conditions. Comparing the magnitude of the change in attitudes from pre- to post-manipulation revealed no significant difference between the positive and negative contact conditions. At a follow-up test seven days after the lab session, there were once again no significant difference in outgroup attitudes between any of the experimental groups.

In Experiment 2, I found that Spanish participants in the negative extended contact condition reported significantly less positive attitudes, higher intergroup anxiety, less contact desire and less positive behavioural intentions towards immigrants than those in the positive or no-contact control conditions. Participants in the positive contact condition reported significantly more positive outgroup attitudes than those in the no-contact control condition. The difference in outgroup attitudes between the negative and control condition was significantly larger than the difference between the positive and control condition. I also

found significant interactions between participants' levels of pre-test positive contact and the experimental condition for outgroup attitudes, contact desire and behavioural intentions. Decomposing these interactions, I found that at low levels of prior positive contact there was no difference in outgroup attitudes, contact desire or behavioural intentions between participants in the three experimental conditions. However, at medium and high levels of prior positive contact, participants in the negative contact condition reported significantly less positive outgroup attitudes and behavioural intentions, and less desire for contact. This finding most closely resembles a negative augmentation effect (i.e., higher levels of positive contact augmented the effect of the negative contact manipulation).

In Experiment 3, I found a significant worsening of outgroup attitudes towards Oxford Brookes students between pre- and post-test for participants from Oxford University in the negative and neutral contact conditions, and a significant improvement in outgroup attitudes for participants in the positive condition. There were no significant differences in the size of the changes in outgroup attitudes between the positive and negative contact conditions.

### **Contact valence asymmetry and effect size comparisons**

Although it is not my primary focus, the data I have collected are relevant to the question of whether negative contact is 'stronger' than positive contact. To briefly summarise the negative contact 'caveat', authors such as Barlow et al. (2012) have hypothesised that the general valence asymmetry found in numerous domains across psychology might also apply to contact. Barlow et al., 2012 suggested (and presented some data to support the idea) that negative contact might have a larger effect on outgroup attitudes, with the potential to outweigh the benefits of positive contact in situations where both occur. Paolini et al. (2010) provided a potential explanation for the mechanism behind such a contact valence asymmetry, based on data which showed that negative contact had a greater effect on levels

of group category salience than positive contact. Paolini et al. (2010) argued that that the effects of negative contact with an individual outgroup member might therefore more readily generalise to the outgroup as a whole.

All seven of the studies reported in this thesis provide data which permit comparison of effect sizes between positive and negative contact. In addition, the literature review presented in Chapter One includes a further nine papers which either compared the effects of positive and negative contact, or more frequently, included data which allow for post-hoc comparison of effect sizes. A summary of the effect size comparisons drawn from these papers, as well as my own studies, is shown in Table 15. Of the forty statistical comparisons I was able to report, only ten produced significant differences in the effects of positive and negative contact. Of these ten significant comparisons, four showed negative contact to have the stronger effect. It should be emphasised that the results presented in Table 15 provide only a very rough and ready summary of the available data. In compiling this list of studies, I have not attempted to produce a rigorous and comprehensive meta-analysis, but rather to illustrate the point that the evidence for a contact valence asymmetry effect is inconsistent at best.

Table 15. Comparison of Positive and Negative Contact Effect Sizes in Literature to Date

| Study                             | Participants   | Outgroup   | N                                      | Dependent variable   | Positive contact   | Negative contact   | Comparison  | Notes  |
|-----------------------------------|--|--|--|--|--|--|---|--|
| Wilder (1984)                     | Douglas/Rutgers students   | Rutgers/Douglast students  | 62                                     | Quality of outgroup college  | <i>M</i> (typical) = 5.93, <i>M</i> (atypical) = 4.60  | <i>M</i> (typical) = 4.24, <i>M</i> (atypical) = 4.31  | <i>F</i> (1, 54) = 10.76, <i>p</i> < .001   | Experiment 1 (pleasant vs. unpleasant / typical vs. atypical manipulation)   |
| Pettigrew & Tropp (2013)          | Germans  | Immigrants   | 1383                                   | Anti-Muslim prejudice  | <i>r</i> = -.45  | <i>r</i> = .28   | <i>t</i> = 22.2, <i>p</i> < .001  |  |
| Barlow et al. (2012)              | White Americans  | Black Americans  | 441                                    | Modern racism<br>Old-fashioned racism<br>Issue-avoidance<br>Active-avoidance<br>Scepticism over Obama's birthplace | <i>b</i> = -0.12**, <i>SE</i> = 0.04<br><i>b</i> = -0.06**, <i>SE</i> = 0.02<br><i>b</i> = -0.15**, <i>SE</i> = 0.05<br><i>b</i> = -0.16**, <i>SE</i> = 0.05<br><i>b</i> = -0.09, <i>SE</i> = 0.05                                     | <i>b</i> = 0.21***, <i>SE</i> = 0.04<br><i>b</i> = 0.11***, <i>SE</i> = 0.02<br><i>b</i> = 0.23***, <i>SE</i> = 0.04<br><i>b</i> = 0.20***, <i>SE</i> = 0.05<br><i>b</i> = 0.11*, <i>SE</i> = 0.05   | <i>z</i> = -1.59, <i>p</i> = .11<br><i>z</i> = -1.77, <i>p</i> = .08<br><i>z</i> = -1.13, <i>p</i> = .26<br><i>z</i> = -0.57, <i>p</i> = .57<br><i>z</i> = -0.28, <i>p</i> = .78                                | Study 2  |
| Christ, Ulrich & Wagner (2008)    | Germans  | Foreigners   | 1320<br>994                            | Prejudice<br>Prejudice<br>Attitude certainty<br>Attitude knowledge<br>Attitude importance                          | <i>b</i> = -.35***<br><i>b</i> = -0.31***<br><i>b</i> = 0.16***, <i>SE</i> = 0.02<br><i>b</i> = 0.21***, <i>SE</i> = 0.02<br><i>b</i> = 0.16***, <i>SE</i> = 0.02  | <i>b</i> = 0.17***<br><i>b</i> = 0.084***<br><i>b</i> = -0.05*<br><i>b</i> = -0.09***, <i>SE</i> = 0.02<br><i>b</i> = -0.03, <i>SE</i> = 0.02  | -<br>-<br>-<br><i>z</i> = 3.95, <i>p</i> < .001<br><i>z</i> = 4.00, <i>p</i> < .001   | Study 1<br>Study 2   |
| Dhont & Van Hiel (2009)           | Belgians   | Immigrants   | 90                                     | Racism   | <i>r</i> = -.28**  | <i>r</i> = .46***  | <i>z</i> = 1.375, <i>p</i> = .17  |  |
| Dhont, Cornelis & Van Hiel (2010) | Flemish police officers  | Immigrants   | 172                                    | Prejudiced attitudes   | <i>r</i> = -.33***   | <i>r</i> = .36***  | <i>z</i> = 0.31, <i>p</i> = .76   |  |
| Aberson & Gaffney (2009)          | White American students  | African-Americans  | 402                                    | Explicit attitudes<br>Implicit attitudes   | <i>r</i> = .38***<br><i>r</i> = .10*   | <i>r</i> = -.35***<br><i>r</i> = -.01  | <i>z</i> = 0.49, <i>p</i> = .63<br><i>z</i> = 1.31, <i>p</i> = .19  |  |
| Bekhuis, Ruiter & Coenders (2013) | Dutch schoolchildren   | Different ethnicities  | 1444                                   | Ethnic distance  | Classroom contact: <i>b</i> = -0.17**, <i>SE</i> = 0.05<br>School contact: <i>b</i> = -0.13*, <i>SE</i> = 0.07<br>Sport contact: <i>b</i> = -0.04, <i>SE</i> = 0.08<br>Neighbourhood contact: <i>b</i> = -0.16*, <i>SE</i> = 0.07      | Classroom contact: <i>b</i> = 0.19**, <i>SE</i> = 0.08<br>School contact: <i>b</i> = -0.07, <i>SE</i> = 0.09<br>Sport contact: <i>b</i> = 0.04, <i>SE</i> = 0.13<br>Neighbourhood contact: <i>b</i> = -0.08, <i>SE</i> = 0.05                | <i>z</i> = 0.18, <i>p</i> = .86<br><i>z</i> = 0.61, <i>p</i> = .54<br>-<br><i>z</i> = 0.81, <i>p</i> = .42  | Model 5  |
| Graf, Paolini & Rubin (2014)      | European students  | Other nationalities  | 1276                                   | Outgroup attitudes (feeling thermometer)<br>Outgroup attitudes (good-bad)  | Person positivity: <i>b</i> = 0.17, <i>SE</i> = 0.34<br>Situation positivity: <i>b</i> = .93**, <i>SE</i> = .35<br>Person positivity: <i>b</i> = 0.11**<br>Situation positivity: <i>b</i> = 0.12**, <i>SE</i> = .04                    | Person negativity: <i>b</i> = -2.75***, <i>SE</i> = 0.42<br>Situation negativity: <i>b</i> = -1.19**, <i>SE</i> = .44<br>Person negativity: <i>b</i> = -0.21***, <i>SE</i> = 0.05<br>Situation negativity: <i>b</i> = -0.09, <i>SE</i> = .05 | <i>z</i> = 4.34, <i>p</i> < .001<br><i>z</i> = 0.42, <i>p</i> = .68<br><i>z</i> = 1.41, <i>p</i> = .16<br><i>z</i> = 0.53, <i>p</i> = .60   | Contact scores coded from open descriptions of intergroup interactions   |
| Paolini, Harwood & Rubin (2010)   | White students<br>Young adults   | Non-white confederate<br>Over-65s  | 52<br>240                              | Category salience (ethnicity frequency)<br>Category salience (ethnicity primacy)<br>Episodic category salience     | <i>M</i> = 17.65, <i>SD</i> = 9.94<br><i>M</i> = 6.33, <i>SD</i> = 4.10<br><i>M</i> = 4.51, <i>SD</i> = 1.34   | <i>M</i> = 25.74, <i>SD</i> = 9.34<br><i>M</i> = 9.65, <i>SD</i> = 2.74<br><i>M</i> = 5.27, <i>SD</i> = 1.24   | <i>p</i> < .05<br><i>p</i> < .05<br><i>F</i> (1, 236) = 20.57, <i>p</i> < .001, $\eta^2$ = .08  | Study 1<br>Study 2   |
| Paolini et al. (2014)             | White American students<br>American students<br>Turkish Cypriots   | Latino immigrants<br>Illegal immigrants<br>Greek Cypriots                        | 83<br>76<br>91                         | Category salience  | <i>M</i> = 3.80, <i>SD</i> = 1.10<br><i>M</i> = 3.99, <i>SD</i> = 1.24<br><i>M</i> = 4.02, <i>SD</i> = 1.44  | <i>M</i> = 4.87, <i>SD</i> = 1.28<br><i>M</i> = 4.65, <i>SD</i> = 1.36<br><i>M</i> = 4.77, <i>SD</i> = 1.18  | <i>t</i> = 4.01, <i>p</i> < .001<br><i>t</i> = 2.35, <i>p</i> = 0.21<br><i>t</i> s = 2.70-2.98, <i>p</i> s all < .01  | Experiment 1 (positive vs. negative tv contact manipulation)<br>Experiment 2 (imagined positive vs. negative contact manipulation)<br>Experiment 3 (imagined positive vs. negative contact), multiple models |
|                                   | Croats<br>Bosniaks<br>Serbs  | National outgroups   | 412<br>588<br>382                      | Readiness for reconciliation   | <i>b</i> = 0.20***, <i>SE</i> = 0.05<br><i>b</i> = 0.35***, <i>SE</i> = 0.04<br><i>b</i> = 0.35***, <i>SE</i> = 0.07   | <i>b</i> = -0.28***, <i>SE</i> = 0.05<br><i>b</i> = 0.10, <i>SE</i> = 0.07<br><i>b</i> = -0.01, <i>SE</i> = 0.07   | <i>z</i> = 1.13, <i>p</i> = .26<br><i>z</i> = 4.42, <i>p</i> < .001<br><i>z</i> = 3.44, <i>p</i> < .001   | Study 1  |
|                                   | Northern Irish Protestants<br>Northern Irish Catholics<br>Northern Irish Protestants<br>Northern Irish Catholics<br>Northern Irish Protestants<br>Northern Irish Catholics | Catholics<br>Protestants<br>Catholics<br>Protestants<br>Catholics<br>Protestants | 970<br>970<br>559<br>625<br>559<br>625 | Outgroup attitudes   | <i>b</i> = 1.88***, <i>SE</i> = .48<br><i>b</i> = 5.59***, <i>SE</i> = .51<br><i>b</i> = 4.12***, <i>SE</i> = 1.34<br><i>b</i> = 5.51***, <i>SE</i> = 1.63<br><i>b</i> = 4.46**, <i>SE</i> = 1.41<br><i>b</i> = 3.86, <i>SE</i> = 1.39 | <i>b</i> = -6.30***, <i>SE</i> = .87<br><i>b</i> = -6.40***, <i>SE</i> = .83<br><i>b</i> = -1.29***, <i>SE</i> = 1.50<br><i>b</i> = -6.96***, <i>SE</i> = 2.06<br><i>b</i> = -4.09*, <i>SE</i> = 2.12<br><i>b</i> = -6.85, <i>SE</i> = 2.40  | <i>z</i> = 3.59, <i>p</i> < .001<br><i>z</i> = 0.69, <i>p</i> = .49<br><i>z</i> = 1.41, <i>p</i> = .16<br><i>z</i> = 0.55, <i>p</i> = .58<br><i>z</i> = 0.19, <i>p</i> = .85<br><i>z</i> = 0.88, <i>p</i> = .38 | Study 2<br>Study 3, T1-T2 cross-lagged relationships<br>Study 3, T2 cross-sectional relationships  |
|                                   | White British  | Black British<br>Asian British   | 4269                                   | Outgroup trust   | <i>b</i> = 0.13***, <i>SE</i> = 0.01<br><i>b</i> = 0.13***, <i>SE</i> = 0.01   | <i>b</i> = -0.15***, <i>SE</i> = 0.02<br><i>b</i> = -0.12***, <i>SE</i> = 0.02   | <i>z</i> = 0.71, <i>p</i> = .48<br><i>z</i> = 0.71, <i>p</i> = .48  | Study 4, within-level relationships  |
| Fell et al. (2015)                | University students  | Homeless people  | 55                                     | Outgroup attitudes   | T1: <i>M</i> = 57.23, <i>SD</i> = 14.49, T2: <i>M</i> = 63.23, <i>SD</i> = 12.05, <i>p</i> = .28, <i>d</i> = 0.45  | T1: <i>M</i> = 48.62, <i>SD</i> = 15.54; T2: <i>M</i> = 42.15, <i>SD</i> = 13.11, <i>p</i> = .22, <i>d</i> = 0.45  | <i>z</i> = 0.0001, <i>p</i> = .1  | Experiment 1 (positive vs. negative vs. neutral imagined contact manipulation)   |
|                                   |  |  |  | Outgroup attitudes   | Positive <i>M</i> = 4.05, <i>SD</i> = 0.60, Control <i>M</i> = 3.84, <i>SD</i> = 0.66, <i>p</i> = .025, <i>d</i> = 0.33  | Negative <i>M</i> = 3.34, <i>SD</i> = 0.51, Control <i>M</i> = 3.84, <i>SD</i> = 0.66, <i>p</i> < .001, <i>d</i> = 0.85  | <i>z</i> = 4.57, <i>p</i> < .001  |  |
|                                   |  |  |  | Intergroup anxiety   | Positive <i>M</i> = 2.28, <i>SD</i> = 0.57, Control <i>M</i> = 2.37, <i>SD</i> = 0.71, <i>p</i> = 0.85, <i>d</i> = 0.14  | Negative <i>M</i> = 2.96, <i>SD</i> = 0.55, Control <i>M</i> = 2.37, <i>SD</i> = 0.71, <i>p</i> = .001, <i>d</i> = 0.93  | <i>z</i> = 6.69, <i>p</i> < .001  | Experiment 2 (positive extended contact vs. neutral extended contact vs. no-contact control manipulation)  |
|                                   | Spaniards  | Immigrants   | 353                                    | Desire for future contact  | Positive <i>M</i> = 3.77, <i>SD</i> = 1.23, Control <i>M</i> = 3.79, <i>SD</i> = 1.47, <i>p</i> = 1.00, <i>d</i> = 0.01  | Negative <i>M</i> = 3.18, <i>SD</i> = 1.33, Control <i>M</i> = 3.79, <i>SD</i> = 1.47, <i>p</i> = 1.00, <i>d</i> = 0.44  | <i>z</i> = 1.66, <i>p</i> = .10   |  |
|                                   |  |  |  | Behavioural intentions   | Positive <i>M</i> = 3.29, <i>SD</i> = 1.54, Control <i>M</i> = 3.32, <i>SD</i> = 1.47, <i>p</i> = 1.00, <i>d</i> = 0.02  | Negative <i>M</i> = 2.62, <i>SD</i> = 1.43, Control <i>M</i> = 3.32, <i>SD</i> = 1.47, <i>p</i> = .001, <i>d</i> = 0.48  | <i>z</i> = 1.72, <i>p</i> = .086  |  |
|                                   | Oxford University students   | Oxford Brookes University students   |  | Outgroup attitudes   | T1: <i>M</i> = 64.84, <i>SD</i> = 16.26, T2: <i>M</i> = 72.11, <i>SD</i> = 13.29, <i>p</i> = .003, <i>d</i> = 0.49   | T1: <i>M</i> = 67.05, <i>SD</i> = 16.13; T2: <i>M</i> = 62.12, <i>SD</i> = 12.87, <i>p</i> = .051, <i>d</i> = 0.34   | <i>z</i> = 0.02, <i>p</i> = .98   |  |

Note. \**p* < .05. \*\**p* < .01. \*\*\**p* < .001 denotes the larger effect (positive or negative contact) for significant comparisons

The argument that negative contact is stronger than positive contact is rather unspecific. Perhaps the most obvious interpretation is, 'a single instance of negative contact will have a greater impact than a single instance of positive contact'. However, this interpretation is still largely impractical, because of the vast array of different possible forms of positive and negative contact (see Barlow et al., 2012). For example, it would be unsurprising to find that being physically assaulted by an outgroup member would have a greater impact on attitudes towards that outgroup than being smiled at in the street. Although it might be valid to compare the effect sizes of pairs of specific positive and negative contact experiences, the results would not be particularly interesting, since the goal of the psychologist is to find generalisable effects. It should however be noted that one context in which this interpretation is useful is experimental research, where experimenters are able to expose participants to single instances of contact in a controlled fashion.

A second possible interpretation of 'negative contact is stronger than positive contact' is 'the sum total of one's negative contact experiences will have a greater impact than the sum total of one's positive contact experiences'. This is a more generalisable proposition, but is considerably harder to test. In their second study, Barlow et al. (2012) operationalised this question using general valenced contact items ("On average, how frequently do you have [POSITIVE/GOOD]/[NEGATIVE/BAD] contact with [Outgroup members]?", Barlow et al., 2012, Study 2). Such questions allow the respondents to decide for themselves what constitutes a positive or negative contact experience. In doing so, this approach captures respondents' interpretation of particular outgroup interactions (the same interaction may be interpreted as positive or negative contact depending on the context and the individuals in question). One might also argue that general valenced contact questions incorporates a form of inherent weighting, in that respondents are likely to base their answers on the contact experiences that are most meaningful to them (or at least, most memorable). Thus, strictly

speaking one could argue that the hypothesis tested by such questionnaire items is, 'one's most memorable (salient, affecting) negative contact experiences will have a greater impact than one's most memorable positive contact experiences'.

This of course assumes a) that respondents are equally able and/or inclined to recall positive and negative contact experiences and b) that only explicitly recalled experiences will have an impact on outgroup attitudes. Both of these assumptions are questionable given the evidence for a) valence asymmetry effects in memory (see Baumeister et al., 2001) and b) the unique role of implicit attitudes in determining outgroup impressions and behaviour (e.g., Dovidio, Kawakami, & Gaertner, 2002; Lambert, Payne, Ramsey, & Shaffer, 2005; see also Gawronski & Bodenhausen, 2006). Overall, although general valenced contact items like those used by Barlow et al. (2012) are probably the most straightforward means of testing contact valence asymmetry in a questionnaire, there are too many potential confounds to their construct validity (or perhaps more accurately, their construct specificity) for them to be useful when it comes to determining the source of any asymmetries that might emerge.

In terms of the issue of contact valence asymmetry, my survey data help to illustrate that the evidence for the dominance of negative contact is mixed, but the questionnaire measures they use are subject to the methodological caveats outlined above. For this reason, my surveys cannot contribute much to the question of contact valence asymmetry, at least in terms of the additive models that have been primarily used up to this point. On the other hand, in my experimental manipulations of valenced contact, I have found some preliminary evidence that one of the confounds affecting valence asymmetry research may be between the strength of an effect, and the ease of its induction. This represents a unique contribution of experimental research to the contact valence asymmetry debate, wherein the ability to manipulate individual instances of contact opens up hypotheses which would be difficult to test using survey data.

### **Valenced contact strength versus ease of elicitation**

In both Experiments 1 and 2, my manipulation checks showed that participants rated the negative conditions (negative imagined contact in Experiment 1, and negative extended contact in Experiment 2) as significantly less positive/more negative experiences than the control conditions (neutral contact in Experiment 1, and no-contact in Experiment 2). In both experiments, there was no significant difference in experience ratings between participants in the positive contact and control conditions. From a methodological standpoint, this indicates that the positive contact manipulations were unsuccessful. This lack of positive contact effects is for the most part mirrored in the outgroup attitude ratings, with participants in both Experiments 1 and 2 showing no differences in outgroup attitudes (Experiment 1), contact desire, behavioural intentions and outgroup anxiety (Experiment 2) between the positive and control conditions. There was a significant positive contact effect for outgroup attitudes in Experiment 2, and consistent with Barlow et al.'s (2012) hypothesis the effect size was significantly smaller than for negative contact. This was a relatively isolated finding however, and the general lack of positive contact effects precludes the use of these data in response to the question of valenced contact asymmetry (in order to test the relative strengths of two effects, both those effects need to be elicited).

However, as discussed in Chapter Five, the lack of experimentally induced positive contact effects may itself be a relevant finding. In particular, it is notable that in both Experiments 1 and 2, the stimuli used to provide the positive and negative contact manipulations were closely matched, with only a few key words differing between the conditions. That an experience can not only be rated as significantly less pleasant, but also have measurable consequences for outgroup attitudes based on the presence of the words 'negative, tense and uncomfortable' (as opposed to 'positive, relaxed and comfortable') suggests the presence of a valence asymmetry in the processing of contact-related

information. In his review of the general valence asymmetry literature, Baumeister et al. (2001) discusses the idea that a bias towards negative information provides an adaptive advantage, because the consequences of negative experiences tend to be greater than the benefits of positive encounters. Put in extreme terms, arguably the ultimate negative consequence - death - has significantly greater impact on an organism than any possible positive experience (see also S. E. Taylor, 1991). In the context of intergroup contact, negative information about the outgroup may receive greater processing in order to avoid potential harm to the individual or their group relative to positive information, the benefits of which are likely to have less of an impact.

This informational importance explanation is perhaps more relevant to Experiment 2, in which participants were very directly exposed to information about the outgroup. The account is somewhat harder to reconcile with Experiment 1, in which the outgroup 'information' was entirely generated by the participants themselves in their imagined interaction. In this case, Paolini et al.'s (2010) 'perceived fit' hypothesis may be more useful. In this account, negative experiences fit more readily with outgroup schema (which tend to be negative) than positive experiences, which might mean that participants will be more likely to accept experimentally induced negative contact due to its closer proximity to their normative expectations. This explanation works well for imagined contact (it should be easier to imagine something which matches your preconceptions), and also benefits from being easier to test (by measuring contact outcome expectancies for example) than the informational importance account.

However, there are a number of issues with the perceived fit hypothesis. Firstly, at a methodological level it may be less applicable to experimentally induced direct contact, due to the social expectations surrounding interactions with strangers. In piloting Experiment 3, it took a great deal of fine tuning in order to create a negative contact encounter that

participants would find believable. During debriefing many pilot participants reported that they simply did not believe that a stranger would behave so unpleasantly towards them with seemingly no provocation. Whilst this issue depends greatly upon the dispositions of the social groups in question (unprovoked hostility being more normative in some contexts than others), the laboratory setting itself may reinforce expectations that fellow participants will behave in a civil manner (quite apart from the behaviour of the experimenters, with regards to which anyone with a passing knowledge of psychological research will appreciate that all bets are off).

Secondly, the idea that negative actions by outgroup members have greater normative fit with their group schema relies on the negativity of that schema. Paolini et al. (2010) qualify their hypothesis by limiting it to negatively perceived outgroups, and refer to self-categorization theory (J. C. Turner et al., 1987) as an explanation for why outgroups are more likely to have negative schemas than ingroups. This account therefore requires some evidence that the outgroup in question is indeed viewed negatively by one's participants. In my experiments, mean scores for attitudes towards the outgroup either did not significantly differ from the scale midpoint (Experiment 1  $t(54) = 13.37, p = .18$ ), or were significantly more positive (Experiment 2  $t(352) = 20.78, p < .001$ ; Experiment 3  $t(76) = 10.39, p < .001$ ). This either suggests that participants had a generally neutral or positive view of the outgroup, discounting the perceived fit explanation, or that self-report measures of outgroup attitudes do not access the true content of a person's outgroup schema. If the former is the case, we must look to alternative explanations for the ease with which negative contact may be elicited relative to positive contact. If the latter is true, a key question for researchers interested in contact valence asymmetry will be establishing a means of measuring the valence of individuals' internal outgroup representations. In either case, care must be taken in future

experimental work on valence asymmetry to ensure that both positive and negative contact manipulations produce discernible effects.

### **The interaction of positive and negative contact**

Of the seven studies reported in this thesis, five provide evidence for some kind of interaction between positive and negative contact. Three of the survey studies (Studies 1, 2 and 3) support the buffering hypothesis (although only Study 3 includes a strict statistical test that discriminates between buffering and positive augmentation). All four of the survey studies support the positive augmentation hypothesis (although once again, the results of Studies 1 and 2 could equally represent a buffering effect). Study 4 demonstrates a contextual version of positive augmentation, with the effect of individual-level positive contact increasing at high levels of neighbourhood-level negative contact. Interestingly, Study 4 also provides evidence for a negative augmentation effect for the Black outgroup only. Experiment 2 also provides evidence for the negative augmentation hypothesis (it is the only experiment in which there were any significant interaction effects). Due to the differences in methodology and results between each of these studies, I will consider each of their contributions to the issue of valenced contact interactions in turn.

*Studies 1 and 2.* As discussed in Chapter Two, Studies 1 and 2 provide evidence for the general existence of interaction effects, but their results cannot be used to support specific interaction hypotheses due to the cross-sectional nature of their data (though they do at least provide some disconfirmation of the poisoning/negative augmentation hypotheses). That being said, the existence of interaction effects of any kind has significant implications for the issue of contact valence asymmetry. In their 'contact caveat' Barlow et al. (2012) state that “any reductions in prejudice associated with positive contact may be counteracted by increases in prejudice that co-occur with (even limited amounts of) negative contact.” (p. 2). This essentially represents an additive model of valenced contact, with the valence of one’s

outgroup attitudes determined by the sum of all positive contact experiences, minus the sum of all negative contact experiences. The results of Studies 1 and 2, however, demonstrate that the additive model is an oversimplification. The presence of interaction effects (of any sort) means that the impact of each individual instance of positive contact is altered by past exposure to negative contact (or vice versa). Contact is therefore not an additive process but an iterative one, in which each interaction with the outgroup is framed by the context of prior experience.

**Study 3.** Study 3 provides the first clear evidence in support of specific interaction hypotheses. Specifically, the two-wave longitudinal design allowed me to test the effect of prior positive contact on subsequent negative contact, and the effect of prior negative contact on subsequent positive contact. In doing so, I found significant interaction effects that supported the buffering and positive augmentation hypotheses respectively. These hypotheses both provide a rather optimistic take on the value of (positive) intergroup contact, in contrast to the concern put forward by Barlow et al. (2012) that it may be devalued in situations where negative contact is also present. Although all the data I have gathered support the notion that negative contact does increase prejudice, Study 3 shows that this effect is significantly weakened by the presence of positive contact. What is more, positive contact not only acts to buffer against the detrimental effects of negative contact, but its own beneficial effects appear to be augmented when levels of negative contact are high. The buffering and augmentation effects found in Study 3 demonstrate that in situations where people are likely to be exposed to negative contact (e.g., in socially diverse neighbourhoods), far from being invalidated, positive contact is more important than ever.

The presence of order effects in Study 3 also reinforces the idea of treating valenced contact as an iterative process. If the effect of each instance of contact is altered by previous intergroup experiences, then in order to accurately test these interactions it is necessary to

capture the causal sequence of contact interactions. The two time points recorded in Study 3 allowed me to unpack the cross-sectional interaction profiles found in Studies 1 and 2, providing a better indication of the kinds of internal processes they might represent. However, testing valenced contact interactions with two (or even more) waves of longitudinal data still only provides a 'slice' of the respondents' ongoing psychological progression. In the time between waves, respondents will have numerous contact experiences which are then aggregated in responses to the survey questions. Because of this, although my longitudinal data have helped to distinguish between different interaction hypotheses, they do not truly model the iterative effect of valenced contact on respondents' outgroup schema. In future, techniques such as diary studies could be used to provide data for latent growth curve analyses (see Meredith & Tisak, 1990) that could more realistically model the ongoing interaction of positive and negative contact.

**Study 4.** Unlike in Study 3, the vector for testing valenced contact interactions in Study 4 was not contact order, but the source of contact information. By applying a multilevel framework to a survey measuring positive and negative contact amongst respondents drawn from numerous neighbourhoods across the UK, I was able to test the interaction of valenced contact information from different sources (i.e., personal experience vs. social context). In this case, instead of past contact providing a moderating context for subsequent contact as in Study 3, contact occurring within the respondents' neighbourhood provided a moderating context for their own direct contact interactions with the outgroup. Here, as in Study 3, I found evidence for a positive augmentation effect, whereby respondents in neighbourhoods with high levels of contextual negative contact were more affected by personal positive contact experiences. I also found evidence for a negative augmentation effect for the Black outgroup (i.e., respondents in neighbourhoods with high levels of positive contact were more personally susceptible to the effects of negative contact). Finding such

cross-level contextual interactions adds an additional dimension to my previous argument that valenced contact does not fit an additive model. Whilst Study 3 shows that the effects of valenced contact are not additively summed across time, Study 4 demonstrates that outgroup attitudes are not determined by an additive sum of contact information from different sources. That is, the effects of contextual-level contact are not simply added to those of individual-level contact. Instead, neighbourhood-level contact provides a moderating context for personal contact experiences, altering their effects based on relative valence.

***Experiment 2.*** The results of Experiment 2 reinforce the role of contact source established in Study 3. Experiment 2 once again demonstrates an interaction between direct and indirect contact. This time, personal experience (prior direct contact) provides the moderating context for a novel instance of extended contact (the experimental manipulation). This effect may be related to the phenomenon whereby the effects of extended contact are moderated by levels of direct contact. Several studies have shown that extended contact is more effective for those with little direct contact experience contact (e.g., Christ et al., 2010; Dhont & Van Hiel, 2011; Eller et al., 2012), with the accepted explanation being that we tend to base our view of a particular outgroup on our most immediate experiences with that group (i.e., when it is available, direct contact trumps extended contact). In Experiment 2 I show a version of this effect that takes into account contact valence. For participants with high levels of direct positive contact, exposure to negative extended contact information had a greater effect on outgroup attitudes. This provides further evidence for the existence of negative augmentation effects, and also reinforces the point that different sources of contact information interact in a non-additive fashion to determine outgroup attitudes.

***Explanations for interaction effects.*** Having established evidence for several specific valenced contact interaction hypotheses, I am now in a better position to discuss possible causal explanations. With regards to the buffering hypothesis, Paolini et al. (2014) have

suggested one possible mechanism relating to group category salience during contact interactions. In a series of four studies, Paolini et al. (2014) reported evidence that positive contact reduces the effect of negative contact on group category salience. The authors explained this finding in terms of positive contact reducing the perceived normative fit of negative outgroup behaviour to their participants' outgroup schema. By reducing the strength of the association between negative behaviour and the outgroup schema, Paolini et al. (2014) posit that prior exposure to positive contact will reduce the likelihood of subsequent negative contact interactions being generalised to the outgroup as a whole.

A condition of testing this hypothesis is the ability to demonstrate a measureable change in the content of people's outgroup schema as a result of positive contact. Unfortunately, as discussed in the previous section on the valenced contact asymmetry hypothesis, there may be a disconnect between the content of a person's outgroup schema, and the attitudes they report towards that group. For example, someone may maintain strongly negative internal representation of a particular outgroup (and thus expect them to behave negatively in any contact interactions), but may still report neutral or slightly positive attitudes towards the group due to considerations of social desirability, or even a genuine principle of not prejudging others based on their group membership alone. As reported in the previous section, the mean attitude scores for most outgroups in my studies were located around or slightly above the scale midpoint. In order to test the perceived fit explanation for the buffering hypothesis, a direct measure of the outgroup schema must be developed. With regards to Paolini et al.'s (2014) findings on category salience, another key step in validating the perceived fit explanation would be to test a model in which negative contact predicts outgroup attitudes via group category salience, moderated by prior positive contact.

As for the positive augmentation hypothesis, Birtel and Crisp (2012), who also found a positive augmentation effect for sequential positive and negative imagined contact

interactions, suggest an explanation based on emotion processing and the theory of exposure therapy. They propose that in order for intergroup anxiety (and thus potentially prejudice, see W. G. Stephan & Stephan, 1985) to be reduced, anxiety must first be induced, and then corrected. In this way, negative (anxiety-inducing) contact followed by positive (anxiety-reducing) contact allows the negative associations with the outgroup to be corrected. However, within the relatively limited scope of the studies available in this thesis (which were not designed with the intention of testing mediators), I have not found evidence to support Birtel and Crisp's (2012) emotion processing account. Specifically, in Experiment 2 I found no interaction between the experimental extended contact manipulation and prior direct valenced contact with intergroup anxiety as the dependent variable. This was particularly notable given that I did find significant interaction effects for all three of the other dependent variables. Of course, the isolated results from one experiment do not warrant immediate rejection of the emotion processing account, and given its central role in (positive) contact theory, intergroup anxiety should certainly be involved in future research on the mediators of valenced contact effects.

As an alternative to Birtel and Crisp's (2012) emotion processing account, Fiske's (1980) extremity hypothesis may provide another explanation for augmentation effects. As discussed in Chapter One, the extremity hypothesis states that since the world we inhabit generally involves more positive than negative experiences, negative stimuli have a greater impact because they deviate more from our psychological 'neutral point' than do positive stimuli. Applying this idea to intergroup contact, and specifically to valenced contact interactions, I would argue that the position of a person's psychological neutral point relating to a particular outgroup will depend on their contact experiences with members of that group. This naturally leads to a dynamic system in which positive contact with outgroup members

will shift the neutral point in the positive direction, which in turn increases the impact of subsequent negative contact (and vice versa), i.e., an augmentation effect.

This explanation is similar to the perceived fit hypothesis proposed by Paolini et al. (2014, 2010), in that it describes an internally maintained representation relating to the outgroup that is constantly updated in response to novel contact experiences. Both hypotheses also hold expectations about outgroup behaviour as a key mechanism by which the internal psychological state translates into outgroup-directed attitudes and behaviour. However, the difference between the two theories lies in how the internally-derived expectations moderate the effect of subsequent contact. According to the extremity hypothesis, contact which runs counter to expectations will deviate from the outgroup neutral point, and will thus have a greater effect. Each valenced contact encounter that shifts the outgroup neutral point (in the positive or negative direction) will therefore *increase* the effectiveness of future contact of the opposite valence. Conversely, according to the perceived fit hypothesis, contact with an outgroup member which matches expectations will fit with the outgroup schema, and will therefore be more likely to generalise to the outgroup as a whole. Each valenced contact encounter that shifts the outgroup schema (positively or negatively) will therefore *decrease* the effectiveness of future contact of the opposite valence.

Note that these hypotheses are partially, but not completely mutually exclusive. As evidenced by Study 3, it is possible for buffering and positive augmentation effects to co-occur (because they each use different moderators: positive contact for buffering, and negative contact for positive augmentation). In this case, it is plausible that both perceived fit and extremity processes could be at work. However, given that I have also found evidence for the existence of negative augmentation effects (which cannot co-occur with buffering effects), neither explanation will be able to account for all instances of valenced contact interactions. The extremity, perceived fit, and emotion processing hypotheses provide a

starting point for investigating the mechanisms behind valenced contact interactions, but I expect more potential explanations to emerge as research in this area progresses.

### **Caveats and qualifications**

Apart from the methodological limitations of the studies that I have discussed in Chapters Two to Five, a number of general qualifications apply to the findings I have presented in this thesis. Firstly, although I have found repeated evidence for the existence of valenced contact interaction effects, the effect sizes have been generally small. The  $\beta$  values for the moderation terms in the survey studies ranged from .11 (Study 2, Protestant respondents) to .29 (Study 3, T1 positive contact \* T2 negative contact, Protestant respondents), and the only significant interaction effect from the experimental studies (Experiment 2) was extremely small, partial  $\eta^2 = .04$ . The particularly small effect size in Experiment 2 is perhaps unsurprising, given that this interaction was between a survey measure of participants' overall levels of positive contact prior to the experiment, and the experimental manipulation of a single instance of valenced contact. I would not necessarily expect participants' overall levels of positive contact to have a dramatic impact on the way they reacted to my extended contact manipulation. That being said, the fact that a small but significant interaction effect did emerge lends some support to the idea of valenced contact interactions as incremental, iterative processes, whereby slight alterations to the effects of individual contact experiences build over time to produce large overall effects of the kind observed in Studies 1-4. As for Experiments 1 and 3 in which significant interactions did not emerge, their sample sizes were much smaller than Experiment 2, meaning that their potential for identifying such small effects was limited.

Secondly, as discussed in Chapter Two, I found significant differences in the profiles of valenced contact effects and interactions between the various social groups sampled in my data. Not only were there group differences within studies (e.g., Serbs showing a significant

interaction effect in Study 1, but not Croats or Bosniaks), but also across studies (e.g., Catholics showing significant a interaction effect in Study 2, but not in Study 3). Taking these group differences into consideration alongside the various different interaction profiles I have observed (buffering, positive augmentation and negative augmentation), and the different causal explanations I have discussed, it would seem that valenced contact interactions are not a simple, unitary phenomenon. In the case of group differences, their existence indicates that a key question for future research on valenced contact effects should be the contextual and demographic factors that influence their emergence at the group (and indeed the individual) level. The results of Study 4 demonstrate the effect of social context on the main effects of valenced contact, but it is equally important to determine the moderators of interaction effects themselves (i.e., under what conditions do they occur, and what factors determine the precise nature of the interaction).

### **Future Directions**

As a nascent field of study, almost every aspect of valenced contact invites further research. With over sixty years of positive contact research to catch up on, there are many theoretical and methodological aspects of negative contact which I have had to pass by in order to maintain a clear narrative focus for this thesis (and in order to complete the work within the three years of funding). In closing therefore, I will highlight the areas which I consider to be of the most immediate importance for further study based on my own findings, and those of other researchers working in the field.

With regards to the question of contact valence asymmetry, experimental research provides a valid means of comparing discrete instances of positive and negative contact. Stimulus matching can be used (in combination with thorough manipulation checks) to ensure that any asymmetry in the effects that emerge results from the contact process itself.

As I found over the course of my three experiments, particular care must be taken during piloting to ensure that both the positive and negative contact manipulations produce significant effects on the dependent variable. That both positive and negative contact can affect outgroup attitudes is not in doubt, but in order to compare their relative effect sizes both processes need to be successfully induced within the same paradigm.

In particular, the results of Experiments 1 and 2 provided some indication that negative contact effects may be easier to elicit than positive contact effects. I would like to see this line of research pursued, perhaps by extending the experimental paradigms I have used to include valenced contact stimuli of different intensities. This may present a challenge in terms of maintaining stimulus believability, especially in direct contact experiments, in which it may be difficult to persuade participants that extremely intense negative interactions are genuine. However, if different induction thresholds for positive and negative contact were identified, it would help to provide a more concrete mechanism behind the rather nebulous idea that negative contact is ‘stronger’ than positive contact.

Testing valenced contact thresholds could also provide a valuable starting point for experimental research on interaction effects. Due to the small effect sizes involved, such research would need to be conducted with large samples, but if successful could provide a direct means of testing the extent to which people’s internal outgroup schema alter the effect of novel instances of contact. For example, it may be possible to test the effect of prior valenced contact on the threshold for inducing positive vs. negative contact (possibly mediated by contact outcome expectancies). The complexity of such a theoretical model would also dramatically increase the sample size required, so may be easier to test using extended contact manipulations like the one used in Experiment 2, which could potentially be distributed as part of a questionnaire without the need for direct experimenter supervision or confederates.

Although experimental research would provide the most direct means of testing the effect of prior valenced contact on subsequent intergroup interactions, it would likely be more practical to test questions of moderation and mediation using survey studies. As far as future survey research on valenced contact is concerned, I would propose three key goals. Firstly, replication of the various interaction effects identified in this thesis should be a priority. Although I found at least two examples of each interaction type (buffering, positive augmentation and negative augmentation), this is still an extremely limited pool of data upon which to base theories of valenced contact. The goal of replication feeds into the second key objective, which is to continue expanding the range of social groups in which valenced contact interactions are tested. Ideally, such research should take attempt to select sample groups based on factors that might moderate the interaction effects in question. For example, one might hypothesise that positive augmentation only occurs when the outgroup in question is sufficiently salient. This might require researchers to target respondents from areas with different levels of physical segregation, to determine whether the direct visibility of the outgroup might moderate positive augmentation effects.

Finally, survey research provides an ideal venue for testing the mediating processes behind valenced contact interactions. This could be achieved using very similar survey designs to those in this thesis, with the simple addition of potential mediating variables such as intergroup anxiety (emotion processing hypothesis), group category salience (perceived fit hypothesis), or outcome expectancies (perceived fit/extremity hypothesis). With regards to the perceived fit/extremity hypotheses in particular, a focus of this line of research should be identifying a suitable measure of respondents' internal representations of the outgroup. As discussed previously, these hypothetical outgroup schema appear to be distinct from outgroup attitudes as they are traditionally measured, but are central to current explanations for the interaction effects I have identified. Possible candidates for such measures could be implicit

attitudes such as those accessed by the Implicit Association Test (Greenwald, McGhee, & Schwartz, 1998). Alternatively, the content of respondents' internal outgroup representations could be inferred from their responses on measures of contact outcome expectancies.

### **Summary and Conclusions**

The stated aims of this thesis were to increase the volume of available data on the effects of positive and negative contact, and to test for their possible interaction. I have found evidence for significant effects of negative contact in every one of the seven studies presented in this thesis, providing strong support for the importance of negative contact as a vital component of contact theory. I have also found significant interactions between positive and negative contact in five of the seven studies. The presence of interaction effects suggests that positive and negative contact should not be treated as additive constructs. Instead, contact can be more accurately modelled as an ongoing iterative process whereby the effects of each novel interaction with an outgroup member are influenced by past experience with that outgroup.

Of the interaction effects I have found, several different interaction profiles have emerged. In my survey studies, the most common effects supported the buffering and positive augmentation hypotheses. Buffering effects showed that the effect of negative contact may be reduced when levels of positive contact are high. Positive augmentation effects showed that the effect of positive contact may be increased when levels of negative contact are high. Contrary to the contact 'caveat' proposed by Barlow et al. (2012), these types of interaction demonstrate that, far from being invalidated when negative contact is present, in such situations positive contact becomes more important than ever. However, in Study 4 and Experiment 2, I also found evidence for negative augmentation, in which the effect of negative contact is increased when positive contact is high. The negative augmentation

hypothesis is mutually exclusive with the buffering hypothesis, and taken in conjunction with the significant differences in the occurrence of interactions between different sample groups, this illustrates another overall finding of my thesis – that valenced contact interactions are neither universal, nor unitary phenomena. My thesis therefore provides a ‘first pass’ at valenced contact interactions, demonstrating strong evidence for their existence, but also the imperative need for further study.

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