The Importance of Target and Audience in Reducing Prejudice and Discrimination through Intergroup Contact and Extended Contact

by

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ABSTRACT

THE IMPORTANCE OF TARGET AND AUDIENCE IN REDUCING PREJUDICE AND DISCRIMINATION THROUGH INTERGROUP CONTACT AND EXTENDED CONTACT

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This dissertation compared the efficacy of intergroup contact theory with an alternative prejudice reduction intervention, extended contact. An experimental manipulation utilizing direct and extended contact conditions was created in an effort to reduce heterosexual Americans’ sexual prejudice and subtle discrimination against the LGBTQ+ group. Over the course of two studies, I tested conditions that could give extended contact an advantage over direct contact; I hypothesized that extended contact with a straight, self-identified ally would make extended contact more effective than direct contact with a gay person. Study 1 was a 2 (participant gender) x 2 (target’s sexual orientation: gay, straight) x 2 (target’s explicit support for LGBTQ+ issues: ally/advocate, non-ally/non-advocate) between-subjects experimental design. Participants viewed a video in which a male target shared facts about sexual orientation and the harmfulness of prejudice and discrimination. Participants completed an online pre-test and a post-test following the manipulation. Study 2 was essentially a replication of Study 1, but an attempt was made to strengthen the manipulation of target’s explicit support for LGBTQ+ issues. Results showed that among participants with low previous contact with the LGBTQ+ group, contact with a gay-identified target resulted in lower subtle discrimination than extended contact. The hypothesis that an ally would make extended contact more effective than direct contact was not supported in
either study. However, Study 1 showed a novel finding that advocacy for LGBTQ+ issues was associated with higher subtle discrimination among those with low to no contact with the LGBTQ+ group, regardless of sexual orientation of the contact. This finding has implications for the application of prejudice reduction interventions in the real world. In addition, a pre-post comparison of prejudice showed that the manipulation itself reduced prejudice, over and above target’s self-disclosed identity, suggesting potential for a video-format prejudice reduction intervention tool that can be distributed through online channels for a large-scale reach.
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CHAPTER 1
INTRODUCTION

Prejudice and discrimination are detrimental to the health and wellbeing of the LGBTQ+ population, and are ongoing social problems (Cochran, 2001; Hatzenbuehler et al., 2010, Hatzenbuehler et al., 2008; Meyer, 2007). Although laws can be created to discourage discrimination against vulnerable populations, prejudice and subtle discrimination are more complicated to eradicate (Abrams, 2010; Herek & McLemore, 2013 for a review; Katz-Wise & Hyde, 2012 for a meta-analysis).

This dissertation is framed within "Trump's America,” wherein the republican President Donald Trump’s administration has achieved power in the US through espousing an ideology of fear and hatred of the “other” (Abramowitz & McCoy, 2018; Edsall, 2019; Leonhardt & Philbrick, 2018). Americans are living in an era of extreme dividedness on issues of race, immigration, and sexual orientation and gender identity. Violence against vulnerable populations is on the rise (Eligon, 2018; Rubin, 2018), but at the same time prejudice (racial prejudice, at least) is declining (Hopkins & Washington, 2019). In this context of social dividedness where a portion of the population appears to be pushing back against the trend toward inclusiveness, work that seeks to reduce prejudice and discrimination may need to take a different approach when engaging citizens on either side of the divide. That is, there may be factors inherent in the backlashing response that change how prejudice reduction interventions are received. Researchers and advocates will need to be aware of the demographic characteristics of those

1 LGBTQ+ is an umbrella acronym for the population of sexual and gender minorities; the acronym stands for lesbian, gay, bisexual, transgender, queer, plus other sexual and gender diverse identities.
whose attitudes we are attempting to change and be prepared to tailor interventions to the audience.

Take, for example, the prejudice reduction approach of intergroup contact. Contact theory is behind one of the most prevalent and replicated types of prejudice reduction intervention, positing that prejudice is reduced through contact between members of the dominant social group (the in-group) and a marginalized social group (the out-group; Allport, 1954). Despite its generalizability and consistent effects at reducing prejudice against various out-groups (Bartoş et al., 2014; Pettigrew & Tropp, 2006), contact has actually been shown to backfire under certain circumstances; after having a negative contact experience or feeling intergroup anxiety, intergroup contact can lead to increased prejudice (Plant & Devine, 2003; Shelton et al., 2009; Stephan, 2014; Stephan & Stephan, 1985; Vonofakou et al., 2007). Also, highly prejudiced people are less likely to voluntarily make contact with an out-group member, making intergroup contact less realistic as an applied prejudice reduction tactic (Hodson, 2011; Hodson et al., 2009). In the studies set out in this dissertation, I test an alternative to the intergroup contact hypothesis that eliminates concerns related to negative intergroup interactions and lack of opportunity for intergroup interactions. This alternative model of prejudice reduction is based on two components: extended contact theory and allyship.

Extended contact is the idea that prejudice can be reduced simply by knowing that an in-group member is friends with a member of the out-group; this theory has been tested before with reliably positive results, but mostly within the context of racial/ethnic prejudice rather than
sexual prejudice or discrimination (see Vezzali et al., 2014 for a review). Given that, in a real-world setting, a prejudiced person may not have opportunity for meaningful and positive contact with a member of a group they are prejudiced against, extended contact is a promising theory in both research and applied contexts. In fact, a growing body of research provides evidence that extended contact lowers prejudice over and above direct intergroup contact (Turner et al., 2007; Vezzali et al., 2014; Vonofakou et al., 2008). There are, however, a number of conditions under which extended contact performs best – for example, the extended contact (i.e., the in-group member who is friends with a member of the out-group) must be a positive role model of intergroup attitudes (Binder et al., 2009; for a review see Brown & Hewstone, 2005; Liebkind & McAlister, 1999; Vezzali et al., 2014).

Although past research has tested some of the conditions necessary for extended contact to have a meaningful impact on prejudice and discrimination, there remains a gap in the literature surrounding who makes an ideal messenger for anti-prejudice/discrimination messaging, and in what context, or with what audience. Will any in-group member suffice to change attitudes? I argue, in accord with a small, but growing, handful of other researchers, that allies - in-group members who actively work to reduce prejudice against marginalized members of out-groups – are a promising potential addition to prejudice reduction interventions (e.g., Gonzalez et al., 2015). I propose that this unique social identity – born of a tested commitment to defend the rights of a marginalized group, is ideal to optimize the effects of extended contact theory and reduce sexual prejudice and discrimination.

Despite the focus on racial prejudice, there are exceptions in newer research showing effects of extended contact on sexual prejudice; e.g., Capozza et al. (2014); Harrison and Michelson (2016)
Given the natural constraints on direct intergroup contact (e.g., lack of opportunity for direct contact, increased anxiety when interacting with out-group), I argue that the ally can serve as a more effective influencer of prejudice reduction among certain heterosexual individuals than lesbian or gay advocates of LGBTQ+ rights. At its core, this argument is based on the premise of the extended contact hypothesis, where limitations of direct contact such as increased intergroup anxiety and threat and decreased trust and opportunity for meaningful contact, are assuaged with interaction with an in-group member as opposed to an out-group member (Dhont & Van Hiel, 2011; Wright et al., 1997). Furthermore, I argue that allies would help reduce prejudice more effectively than heterosexuals who do not identify (or act) as allies. This argument is based on the assumption that an ally models attitudes and behaviours that are supportive of the out-group which gives prejudiced individuals an example of how to change (Eller et al., 2011a; Wright et al., 1997). That is, a heterosexual who does not self-disclose or behave as an ally is not modelling inclusive (or unprejudiced) behaviour. That individual can merely tell people to not be prejudiced or to not discriminate – but cannot show others that doing so is even possible. An anti-prejudice message coming from a place of inaction is not sufficient to change minds or society (Hodson, 2016).

Thus, throughout this dissertation I ask the research question, can an ally delivering anti-prejudice messaging make extended contact more effective than direct contact when applied to the reduction of sexual prejudice and discrimination? I test this research question in two experimental studies that utilize educational videos presented by straight ally, straight non-ally, gay advocate, and gay non-advocate targets. The population of interest is heterosexual Americans, with both low levels of previous direct contact (Study 1) and high levels of previous direct contact (Study 2). Before launching into the present experimental studies, however,
Chapter 2 of this dissertation reviews all relevant literature, including common prejudice reduction techniques, with a focus on the contact paradigm. Chapter 3 provides a detailed account of the design and methodology for Study 1, and Chapter 4 presents the results and discussion for Study 1. Chapters 5 and 6 similarly contain the design and methodology, and results and discussion, respectively, for Study 2. Finally, Chapter 7 contains a thorough general discussion of implications of both studies, and a summary of the important theoretical contributions derived from the entire research program.
CHAPTER 2

LITERATURE REVIEW

Prejudice Reduction

While the goal of this research program is prejudice reduction, it is worth noting that some scholars argue that prejudice is inevitable (see Devine, 1989; Ehrlich, 1973). This perspective is based on the notion that it is a natural human tendency to categorize individuals into groups, and prejudice is an inevitable consequence of doing so. However, whether innate or not, prejudice has harmful consequences for its targets (see for e.g., minority stress theory; Meyer, 1995, 2007). Sexual prejudice (i.e., prejudice against the LGBTQ+ population) and discrimination against the LGBTQ+ population have been implicated as predictive factors in the well documented health and well-being disadvantages for sexual minorities (Bostwick et al., 2014; Hatzenbuehler et al., 2010; Hatzenbuehler et al., 2008; Williams & Mann, 2017). For example, in 16 US states with a constitutional ban on equal marriage (prior to the Supreme Court ruling that marriage is a fundamental right), lesbian, gay and bisexual residents had a higher prevalence of substance use disorders, generalized anxiety disorders, and mood disorders; the prevalence for LGB residents was higher than for heterosexuals in the same state as well as higher than LGB populations in states without a ban on equal marriage (Hatzenbuehler et al., 2010). Some research also shows that perceived prejudice and discrimination negatively impacts the physical health of the LGBTQ+ population, including increasing their risk for cancer and cardiovascular disease (Denton et al, 2014; Lick et al., 2013). The task of changing prejudiced

While most research cited in this proposal relate to the LGBTQ+ population in general (or is not otherwise stated), the Hatzenbuehler et al. (2010) paper specifically references data on lesbians, gay men, and bisexuals (LGB).
attitudes has therefore been the focus of an increasing amount of research in recent years, with intergroup contact theory at the fore (see Bartoš et al., 2014, for a review).

Although I primarily argue the utility of extended contact theory in this research program, it is worth briefly discussing some other common prejudice reduction theories and interventions for comparison’s sake. For ease of comparison, I will maintain a focus on sexual prejudice reduction (as opposed to racialized prejudice or other targets of prejudice) throughout this literature review. While some interesting studies in neuropsychology and psychophysiology have recently attempted to understand sexual prejudice (see Amodio, 2014 for a review; O’Handley et al., 2017), prejudice, discrimination, and prejudice reduction research is most at home in the realm of social psychology (Brown, 2010). Social psychology offers a blend of cognitive social theory (e.g., stereotypes and implicit attitudes) and sociological theory (e.g., intergroup conflict) with which we can thoroughly understand prejudice, discrimination, and prejudice reduction (Callender, 2015; Tajfel, 1982; Robb, 1978).

Prejudice reduction through the lens of cognitive social psychology is about changing the subconscious, or automatic, underpinnings of prejudice through tactics such as suppressing stereotypes or activating counterstereotypic information (Blair, 2001). In a recent review of interventions to reduce sexual prejudice, stereotype awareness and suppression was a fairly uncommon method (only used in 5 out of 159 studies; Bartoš et al., 2014). One reason why this strategy is uncommon may be its tendency to produce rebound effects; that is, attempting to suppress one’s use of stereotypes can sometimes lead to an even stronger inclination to use stereotypes in categorizing people (Macrae et al., 1994). This rebound effect occurs for multiple reasons, including that stereotype suppression requires cognitive resources that eventually run out, and also that individuals can be easily distracted from the focus it requires to suppress a
stereotypical judgment (Macrae et al., 1994). Another problem with this kind of approach to prejudice reduction is that the end goal of such interventions is not to reduce prejudiced beliefs or values, but to avoid acting on the negative stereotypes one holds within their cognitive schema (Nelson, 2016). For long lasting change in the way we think about others, interventions need to involve more than learning to control the expression of our prejudice.

Controlling our use of automatic stereotypes in a laboratory setting is one avenue to reducing prejudice and discrimination, but it may have limitations in application to real world settings. Perhaps prejudice reduction researchers can find more generalizable success by appealing to our social nature. A common type of prejudice reduction intervention involves using an expert or a prototypical in-group member to set tolerance (or lack of prejudice) as a social norm. This approach has been effective in reducing racial prejudice when the manipulations are strong (see Paluck & Green, 2009, for a review). However, appealing to our desire to adhere to social norms (at least in the laboratory setting) has only shown a small effect on reducing sexually prejudiced attitudes (Cohen’s $d=-0.02$, $SE=0.01$; Bartoš et al., 2014). A social norms approach to prejudice reduction also has limitations in real world application: as prejudice is now non-normative in many socially progressive countries, we have seen blatant discrimination be insidiously replaced with subtle discrimination. Prejudice still remains, and people find new ways to exert it without breaking societal norms or expectations for behaviour.

Although normative influence may not be a reliable tool for reducing sexual prejudice on its own, we can derive an important theoretical model from these types of experiments that should contribute to our understanding of how certain individuals can set a new norm and thereby change attitudes of others; that is, we can gain insights from the minority-influence paradigm (Moscovici et al., 1969). Consider the unrealistic situation where everyone was
always influenced by the prevailing norms. In such instances there would be no divergence from those norms, no differing opinions, and the world would not be what it is today. Moscovici et al. (1969) conducted pioneering work with an experiment that questioned why a norm-divergent minority are able influence others above and beyond the true normative influence (held by the majority of people). This question is pertinent to the present investigation of the influencing effects of allies: Allies can be considered a minority influence among social demographics who see sexual prejudice as normative. First let us take a step back from the application of this model and discuss how it was originally determined.

In the laboratory, Moscovici and his colleagues had participants in groups identify the colour of a blue stimulus, where the normative response was, obviously, blue. When Moscovici had a small number of confederates in the group (the minority influence) identify the stimulus as green, some participants were influenced to incorrectly label the stimulus as green as well (8%; 1969). Clearly, humans are susceptible to influence by new ideas that go against the norm. Later research investigated this minority influence effect as it pertained to actual social in-groups and out-groups and found that the effect only exists when the influence comes from an in-group member (David & Turner, 1996; Wood et al., 1994; Wood, 2000). Therefore, in research that attempts to change normative attitudes such as prejudice against an out-group, an in-group member should be more effective than an out-group member at influencing others in the in-group.

As with cognitive approaches that tackle automatic attitudes, social norms do have a role to play in prejudice reduction; however, I argue that the challenge of reducing prejudice and discrimination is more complex than these individual theories are capable of explaining in isolation. Another avenue for consideration in the design of a prejudice/discrimination reduction
intervention is the idea of appealing to intellect and empathy. In Bartoş et al.’s (2014) review, the most common type of intervention for reduction of sexual prejudice was education-based, which operates on the assumption that prejudice is based in ignorance, and that educating about the target of prejudice and the injustice against that group will play on people’s empathy and understanding and change their prejudiced attitudes. These types of interventions do, in fact, show some efficacy. The review reports that educational interventions had a moderate to large effect size on several outcome variables, including attitudes, behaviours and emotions (Cohen’s $d = 0.46, 0.55, \text{ and } 0.36$, respectively), showing reasonable efficacy in reducing sexual prejudice and discrimination (Bartoş et al., 2014).

In one example of educational interventions to reduce sexual prejudice, Lucassen and Burford (2015) provided youth with a 60-minute workshop about sexual diversity and measured valuing and understanding lesbian, gay, and bisexual people in pre- and post-tests. The workshop was produced by Rainbow YOUTH and contained basic cultural competency information such as an introduction to diverse sexual identities and experiences, awareness raising about discrimination, and a goal of developing empathy (Lucassen & Burford, 2015). The intervention showed an increase in both valuing and understanding sexually diverse identities post-workshop, and most participants (95%) believed that the workshop could effectively reduce sexuality-related bullying in schools (Lucassen & Burford, 2015). In a related study by Burford, Lucassen and Hamilton (2017), youth were given a 60-minute workshop about gender diversity (revised version of Rainbow YOUTH’s sexuality workshop), and there was a similar increase in valuing and understanding of gender-diverse (e.g., transgender) people after participating in the workshop (Burford et al., 2017).
Education-based interventions are often used in conjunction with intergroup contact; that is, in the context of a prejudice reduction intervention toward LGB members, a lesbian, gay, or bisexual identified person would present a class, workshop, or other presentation on sexuality to a sample of heterosexual participants (Bartoş et al., 2014). Evidence suggests that these multidimensional interventions seem to work well – Bartoş et al. reviewed 27 studies that used education in conjunction with intergroup contact and found a medium effect size on sexually prejudiced attitudes ($d=0.41$, $SE=0.06$).

Before delving into what a combined education and contact intervention would look like, let us first examine each of these approaches in isolation to understand them more thoroughly. Although common in the literature on prejudice reduction interventions, educational approaches are often studied in field-based research, such as an evaluation of pre-existing diversity training workshops, classes, and materials in schools and workplaces (Paluck & Green, 2009). Therefore, when any theory is used in that context, it is generally theory of teaching and learning relating to changing or improving an existing classroom environment (e.g., transformational learning theory; Mezirow, 1991; Riggs et al., 2011). Because many educational interventions for prejudice reduction take place in educational institutions (Paluck & Green, 2009), there is often a focus on active learning strategies. The following example is a typical style of educational intervention that was effective at reducing sexual prejudice.

Riggs and colleagues (2011) responded to negative outcomes for LGBTQ+ youth in schools by creating a teacher training workshop that was administered in teachers college classrooms. The 3-hour intervention attempted to promote positive attitudes toward LGBTQ+ people, increase knowledge of LGBTQ+ issues, and increase willingness to support LGBTQ+ issues, all of which are common outcome variables with educational interventions for prejudice
reduction. Riggs et al. used a control condition which contained information about children with disabilities in place of LGBTQ+ children. The use of control groups is rare in this kind of intervention, perhaps because many are evaluations of existing workshops for which a naturally occurring control group does not exist.

The Riggs and colleagues (2011) intervention included an experiential activity (participants read an excerpt of a gay person’s experience and then wrote a reflection); large-group discussion focused on diversity and discrimination; more reflection; another experiential activity (participants listed stereotypes and assigned them to negative or positive categories); the facilitator shared information to debunk stereotypes; participants watched a video that armed teachers with tools to appropriately respond to sexual prejudice in their classrooms; and finally, participants engaged in small group discussion to practice these tools. Using a pre- and post-test design, Riggs et al. found that the experimental intervention increased positive attitudes toward LGBTQ+ people, increased knowledge of LGBTQ+ issues, and increased willingness to support LGBTQ+ causes; the control group did not show an increase in any of the outcome variables (Riggs et al., 2011).

While there are many studies assessing education interventions within educational environments, studies implementing an education intervention to the general population are far rarer. When these interventions administered to the general population do occur, they often fall under the umbrella of public pedagogy. Public pedagogy is learning that takes place outside of traditional institutions and thereby is an ideal platform for counter-hegemonic learning and activism (Sandlin et al., 2011; Savage, 2010). Among a variety of public pedagogy approaches, visual media is a common approach. Anti-prejudice messaging through visual media is not a new idea; known as educational entertainment, this means of accessing large populations has been
used by governments, organizations, and researchers for decades in an attempt to educate the masses about social justice and inequalities (e.g., Sesame Street; Paluck & Green, 2009; Singhal et al., 2004).

One popular avenue for educational entertainment through public pedagogy is YouTube – a website for streaming user-created videos (Miller, 2017; Snell, 2014). In a recent series of experiments, Murrar and Brauer (2017) tested the effects of an anti-prejudice YouTube video designed to improve attitudes about Muslims. Murrar and Brauer had participants watch a YouTube video depicting Muslims as a heterogeneous group of individuals who shared personal factoids that the White, Christian audience would be likely to identify with. The video had an overall positive tone, highlighting things that make someone a “good person” (Murrar & Brauer, 2017, p. 14). The experimental design compared four conditions: the viewing of the anti-prejudice video (experimental condition), imagining a positive intergroup interaction with a Muslim and writing a reflection about it (imagined intergroup contact), reading a pro-social scientific excerpt about the changeable nature of religion (intergroup malleability exercise), and finally, one group received no intervention (control). Two of these conditions (imagined intergroup contact and intergroup malleability) were other well-known prejudice reduction interventions that were used as appropriate alternative interventions against which educational entertainment was tested. Participants in the YouTube video group had lower post-test explicit prejudice than the other three groups, suggesting that educational entertainment may be an effective style of prejudice reduction intervention, even when compared to other well-known theories of prejudice reduction (Murrar & Brauer, 2017).

When educational interventions are paired with contact-based interventions, researchers are able to address prejudice reduction in more nuanced ways (e.g., adjusting who delivers
educational content). Intergroup contact – now a well-tested theory – posits that an in-group member’s prejudice can be reduced as a result of meaningful contact with an out-group member (Allport, 1954; Dovidio et al., 2008; Dovidio et al., 2017). This theory was originally developed to explain and promote prejudice reduction in cultural and ethnic in-group/out-group interactions, but a meta-analysis by Pettigrew and Tropp (2006) and review by Bartoş et al. (2014) found particularly strong support for intergroup contact theory in cases of sexual prejudice.

Recent research has proposed that intergroup contact works through the social cognitive mechanism of categorization, in a model known as the common in-group identity model (Gaertner & Dovidio, 2000; Gaertner et al., 2016). This model suggests that contact can either help us to decategorize by seeing the out-group member as an individual and reducing the salience of belonging to different groups altogether, or contact can help us recategorize by seeing the out-group member as one of us, or “we,” instead of “us” vs. “them” (Dovidio et al., 2017). Important to the likelihood of de- or re-categorization occurring – and therefore to the success of intergroup contact - is having a shared goal as part of the interaction (Allport, 1954; Dovidio et al., 2017). Categorization cannot simply remove the salience of the contact’s out-group membership, though; for optimal attitude change toward the out-group rather than just toward the individual, the out-group membership must also remain salient in a dual identity (Gaertner et al., 2016). That is, the in-group member needs to see that the out-group member is similar enough to the in-group to be liked, but at the same time needs to remember that the individual belongs to the out-group so they are able to generalize this likeability, or relatability, to the rest of the out-group.
Researchers have identified some key moderators of intergroup contact that should be considered in any experimental design. First, the out-group contact must be perceived as a typical exemplar of the out-group. As with dual identity salience, this moderating factor is important because it promotes generalization of positive attitudes from the individual out-group member to the rest of the out-group (Dovidio et al., 2017). If the out-group contact is atypical, the perceiver may not generalize positive attitudes to the whole out-group, or may believe that the only reason they reacted positively to the out-group member was because they were atypical of the rest of the out-group. Another key moderator is the valence, or quality, of intergroup contact. High quality and meaningful contact with an out-group member has been reported to have reliable effects on reducing prejudice, but negative intergroup contact can result in worsened intergroup relations (Hayward et al., 2017).

Another important condition of intergroup contact is the likeability of the out-group member, or, when possible, friendship with the out-group member (Vonofakou et al., 2007). This condition is closely related to the moderator of contact valence; the more the in-group member likes the out-group contact, the higher quality the interaction will be. Research supports this assumption: in contact interventions with cultural and ethnic out-groups, the greatest reduction in prejudice resulted from friendship with an out-group member, over other forms of relations (Davies et al., 2011; Pettigrew & Tropp, 2006; Vonofakou et al., 2007).

Intergroup contact theory has been used to explain variability in sexual prejudice with strong and reliable effects. For example, in a benchmark study, Herek and Capitanio (1996) conducted a national survey of heterosexuals to determine the relationship between contact with gay men and lesbians and sexually prejudiced attitudes. Results showed more positive attitudes toward gay men and lesbians among those heterosexuals who had more contact (greater
frequency of contact), closer relationships (higher quality contact), and who had received direct disclosure of sexual orientation (Herek & Capitanio, 1996). In a similar but recent survey of over 1600 heterosexuals, Costa and colleagues (2015) investigated whether greater contact with gay men and lesbians (i.e., how many gay men or lesbians they knew) was associated with more positive attitudes about same-sex parents. Results showed that comfort with gay men and lesbians mediated a positive relationship between contact and support of same-sex families (Costa et al., 2015).

The effects of contact on reduced sexual prejudice are consistent in experimental settings as well as surveys, although experimental investigations are less common (Paluck et al., 2017). In one experiment, white heterosexual college students were assigned to one of two conditions: an induced friendship with a gay or lesbian identified confederate (gender matched), or with a confederate who did not reveal their sexual orientation (control). Participants completed the fast friends procedure to induce feelings of friendship (Aron et al., 1997). Regardless of whether the confederate revealed their sexual orientation prior to or after the fast friends procedure, those in contact with a self-disclosed gay or lesbian showed less sexual prejudice than the control group (Lytle & Levy, 2015). In addition to changed attitudes, this study found that contact with the gay and lesbian confederates resulted in more positive behaviour toward the confederates than in the control condition; participants had friendlier and longer conversations with the confederates post-intervention (Lytle & Levy, 2015).
In a similar vein of research, one recent study compared a panel of transgender speakers and cisgender speakers both giving an educational lecture on transgender identity and found that those who listened to the transgender speakers experienced greater reduction in transphobias (Walch, et al., 2012). These findings suggest that, in the right conditions, intergroup contact can be a powerful tool for sexual prejudice reduction.

As previously reviewed herein, much research has shown that intergroup contact is undoubtedly an effective option for prejudice reduction in experimental scenarios; but in the real world, what option is there to reduce prejudice in individuals who are unlikely to engage in positive interactions with an LGBTQ+ person (e.g., highly prejudiced individuals; Hodson, 2011)? While intergroup contact theory involves an in-group member’s prejudice reduction resulting from meaningful contact with an out-group member, Wright et al. (1997) proposed an extension of this theory several years ago: the knowledge that a fellow in-group member develops a friendship with an out-group member could improve other in-group members’ attitudes toward the out-group. This theory is aptly named extended contact effect.

Given that, in a real world setting, a prejudiced person may not have opportunity for meaningful and positive contact with a member of a group they are prejudiced against, extended contact with an ally becomes especially important. As it turns out, those who do not have opportunity for direct intergroup contact in their real lives are no less likely to have opportunity for extended contact (Hodson et al., 2009). Therefore, extended contact provides a means to

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4 While transgender is a term used to denote a gender expression that differs from sex assigned at birth, cisgender implies that gender expression matches sex assigned at birth; that is, someone born with an anatomically female body and also identifies as a female is known as cisgender.

5 Transphobia is prejudice specifically targeted at transgender or gender-diverse individuals.
reach prejudiced individuals with an anti-prejudice message that is not afforded by traditional intergroup contact.

In fact, a growing body of research provides evidence that extended contact lowers prejudice over and above direct intergroup contact (Turner et al., 2007; Vezzali et al., 2014; Vonofakou et al., 2008). Moreover, extended contact (among racial in-groups/out-groups, at least) may actually pave the way to beneficial direct intergroup contact through reduction of anxiety and positive expectations for how a future direct interaction will go (Gómez et al., 2011; Turner et al., 2008). Additionally, extended contact encourages individuals to think about members of two different groups interacting – the heterosexual in-group member and a gay or lesbian out-group member. This imagined interaction may increase dual-identity salience, which, according to common in-group identity theory as a mechanism for direct contact, is important for generalizing positive attitudes to the entire out-group (Brown & Hewstone, 2005; Vezzali et al., 2014). Combined with lowered intergroup anxiety and a vehicle for positive, educational messaging, extended contact appears to have many advantages over and above direct contact.

Wright et al. (1997) proposed two conditions of extended contact effect: (1) the in-group member directly in contact with a member of the out-group must be a positive in-group role model on intergroup attitudes, and (2) the person in direct contact must be a typical exemplar of the in-group. The latter condition is a stipulation of including the other in the self; that is, in-group members must perceive the person in direct contact with the out-group as similar to themselves (Binder et al., 2009; for a review see Brown & Hewstone, 2005; Liebkind & McAlister, 1999; Vezzali et al., 2014). Allies, by definition, are highly suitable to meet these conditions, enabling the likelihood that extended contact effect will be successful at reducing sexual prejudice.
So, how effective can we expect extended contact effect to be at reducing sexual prejudice? Unfortunately, the research literature does not include many experimental tests of extended contact’s effect on sexual prejudice. Most studies of prejudice against LGBTQ+ people measure direct and extended contact through self-report rather than manipulating the variables. In one interesting exception, Harrison and Michelson (2016) tested attitude change toward marriage equality among football fans in the US using extended contact. This study focused on in-group salience by showing football fans messages supporting marriage equality that were attributed to straight football players (i.e., in-group allies), or the same messages attributed to anonymous speakers (control condition). Football fans who received the message from a salient in-group ally showed more support for equal marriage than did those who received the same message from an anonymous person (Harrison & Michelson, 2016).

Whether by design or as an afterthought, studies of extended contact also test mediators and moderators. Many of these variables are also found in the direct intergroup contact literature, and unsurprisingly, there is considerable overlap. In a 2012 review of extended contact literature, Vezzali and colleagues listed a rather large series of variables that have been included across the literature as mediators and moderators of extended contact and attitude, some tested empirically, others theorized (see Table 1).
Table 1

*Known mediators and moderators of extended contact effect*

<table>
<thead>
<tr>
<th>Mediators</th>
<th>Moderators</th>
</tr>
</thead>
<tbody>
<tr>
<td>inclusion of other in the self</td>
<td>direct contact</td>
</tr>
<tr>
<td>social norms</td>
<td>type of out-group</td>
</tr>
<tr>
<td>in-group identification</td>
<td>membership salience</td>
</tr>
<tr>
<td>out-group self-disclosure</td>
<td>group typicality</td>
</tr>
<tr>
<td>perspective taking</td>
<td>group categorization</td>
</tr>
<tr>
<td>infrahumanization</td>
<td>personality variables (e.g., RWA)</td>
</tr>
<tr>
<td>ignorance about the out-group</td>
<td>initial out-group stereotypes</td>
</tr>
<tr>
<td>intergroup anxiety</td>
<td>in-group identification</td>
</tr>
<tr>
<td>intergroup trust v. threat</td>
<td>closeness to in-group and out-group contacts</td>
</tr>
<tr>
<td>empathy</td>
<td></td>
</tr>
</tbody>
</table>

*Note.* List of variables from Vezzali et al., 2012, p. 322

A general understanding of the variables that may impact the relationship between extended contact and prejudice is important in the development of any new research on the topic so as to eliminate possible confounds or account for possible moderators/mediators that might impact a particular design. As it is impractical to include all of these variables in one experimental design, however, I will limit my discussion here to those variables that are most often included in research on *sexual* prejudice, or are most relevant to the type of experimental manipulation I have designed for this research. Those variables are: previous direct contact, inclusion of other in the self and closeness to out-group contacts, ignorance about the out-group,
and out-group self-disclosure. Many of the moderators from this list are included in the earlier discussion on intergroup contact or in the subsequent discussion on sexual prejudice.

Previous direct contact is perhaps most obviously seen as having potential confounding effects on extended contact; that is, unaccounted for previous direct intergroup contact may explain any measured effects that extended contact has on prejudice and discrimination. If the sample is unbalanced in terms of the proportion who has LGBTQ+ family or friends, effects in a test of extended contact may be inflated. To control for third-variable effects, researchers can measure both direct contact and extended contact (a practice very common in survey research); however, measuring both direct and extended contact in survey research has its own potential problems. Unless the variables are measured and analyzed in a methodology like social network analysis, where the interconnectivity of the contacts is assessed, the direct and extended variables are difficult to tease apart and may result in multicollinearity problems. For example, the direct contact that a participant reports may be the same person with whom their reported extended contact is in contact with (e.g., the extended contact and direct contact are both colleagues at the same workplace; Munniksma et al., 2013). One way to address this potential issue is to control for previous direct contact in the recruitment stages of research, and to conduct separate analyses on participants with and without previous direct contact (Pettigrew et al., 2007).

Direct contact can be more than a confounding variable, though, and has been tested as a moderator in several studies. Cameron and colleagues (2011) tested valence of direct contact as a moderator of extended contact; specifically, high quality (e.g., friendship) and low quality (e.g., acquaintances) contact. In their research, the out-group was Indian-English children, and the in-group was white children in a British elementary school, and the extended contact intervention consisted of stories that portrayed friendships between the two groups of children. Extended
contact was moderated by previous direct contact in that extended contact had a greater effect on prejudice reduction among children with less high quality direct contact; this study found that children with high levels of direct contact (e.g., more frequent contact) did not benefit from the extended contact interventions due to ceiling effects in positive perceptions of cross-group friendships (Cameron et al., 2011). This same pattern of moderation between direct contact and extended contact has also been consistently shown when measuring frequency of contact as opposed to quality of contact. Extended contact is more effective among those with no or infrequent direct contact than among those with frequent direct contact in populations such as a Dutch in-group and Muslim immigrant out-group, Germans and foreigners in Germany; Catholics and Protestants in Northern Ireland, and Americans and Mexicans in California, among others (Christ et al., 2010; Dhont & Van Hiel, 2011; Eller et al., 2012). Extended contact appears to have a stronger effect on reducing out-group prejudice for participants with no or limited previous direct contact with members of another group, as opposed to those who have more previous direct contact.

Feeling psychologically close to a contact is another well-tested mediator and moderator of intergroup and extended contact on attitudes toward the out-group (Capozza et al., 2014; Eller et al., 2011a; Hodson et al., 2009). In essence, when we have a close relationship with someone, parts of their identity overlap with ours – there is a sense of inclusion of the other in the self (Aron et al., 1992). When we include others in the self (or feel close to others), we generally extend positive feelings usually felt toward ourselves, such as liking and empathy (Lemm, 2006; 6)

6 Some studies refer to friendship as a moderator (or mediator) of contact (e.g., Hodson, Harry, & Mitchell, 2009), but friendship as a variable is limited in that it excludes other types of close relationship, and it is less precisely defined than IOS. Therefore, I use the term closeness, measured using the IOS scale.
Turner et al., 2008). This inclusion of the other in the self also has the ability to generalize feelings about a close contact with the social group to which that contact belongs, creating a mechanism for prejudice reduction (Capozza et al., 2014). The conflation of closeness and inclusion of the other in the self is not coincidence; the theory of inclusion of other in the self (and resultant scale, the IOS scale) was developed based on the framework of interpersonal closeness (Aron et al., 1992). Reliably, the IOS scale has tapped into people’s sense of closeness and connectedness to others and has been the most commonly used closeness measure in contact literature (Aron et al., 1992; Turner et al., 2010).

Closeness is most easily understood as a moderator of direct contact on attitudes, but it also functions as a moderator of extended contact. The inclusion of a fellow in-group member in the self can be extended to an out-group when a person learns that their close in-group contact is friends with an out-group member, thereby extending that sense of inclusion to the entire out-group (Turner et al., 2008). Past research has focused on both the closeness to the in-group contact who has the out-group friend (e.g., how close the respondent is with a straight friend who knows a gay person) and the perceived closeness of the in-group contact with their out-group friend (e.g., how close a straight friend is with an unknown gay person; see Vezzali et al., 2014, for a review). Although both have been considered in the measurement and testing of extended contact hypothesis, the latter, as a measure, requires respondents to speculate on or estimate the closeness of a relationship that they are not personally part of, and therefore, in terms of validity, the former measure of closeness in extended contact may be preferable.

One way to increase the sense of closeness between an individual and an LGBTQ+ out-group member is through the out-group member’s personal self-disclosure of their sexual orientation / gender identity (Miller, 2002; Turner et al., 2007). Self-disclosure is naturally built
into manipulations based on intergroup contact theory – because sexual orientation is not something that can necessarily be given away by visual cues, the out-group contact must verbally identify as an out-group member to the research participant. Beyond laboratory manipulations (i.e., in the real world), knowledge of a person’s sexual orientation or gender identity can follow active self-disclosure or passive disclosure; examples of passive disclosure include being “outed” by another individual, or declining to deny another’s suspicions of one’s LGBTQ+ status (White et al., 2017). This difference in how one comes to know of the sexual orientation / gender identity of a contact has implications for sexual prejudice: learning about a contact’s identity directly from that contact (i.e., active self-disclosure) is associated with more positive attitudes toward lesbians and gay men than learning about a contact’s identity from a third party source (Herek & Capitanio, 1996). Research on how self-disclosure affects sexual prejudice tends to highlight the role of increased trust and empathy that results from having an out-group member share something personal and potentially vulnerable about themselves (Turner et al., 2007). Self-disclosure is therefore an important factor in creating a high quality contact scenario due to its ability to strengthen the sense of closeness to the contact, and ultimately its potential to affect attitudes toward the out-group.

The next variable of interest, ignorance about the out-group, has been theorized but not actually tested as a mediator/moderator of extended contact on prejudice, to the best of my knowledge. However, this variable is relevant to the discussion of educational interventions. In their initial conception of extended contact effect, Wright et al. (1997) theorized that ignorance about the out-group was an underlying mechanism of the effect. Ignorance about the out-group is also a known mediator of direct intergroup contact (Pettigrew & Tropp, 2008). The concept is straightforward – ignorance leads to prejudice because misinformation and stereotypes paint a
negative picture of the out-group (Stephan & Stephan, 2013; Wright et al., 1997). Reducing ignorance is related to recategorization of out-group members as one learns more about the similarities shared between the out-group and the self, rather than the differences (Stephan & Stephan). It is also related to reduced anxiety about interacting with the out-group, because anxiety is worsened by the unknown (Stephan & Stephan).

In an intervention to reduce prejudice against an ethnic out-group, international students at a UK university acted as the in-group extended contact between their (international) family members and white British university students (Eller et al., 2011b). The intervention was designed to lower prejudice against white British university students among international families. Results showed that extended contact via the international students studying in the UK reduced prejudice as expected, and this effect was mediated by lower self-perceived ignorance about the white British students (among other variables, including lowered anxiety; Eller et al., 2011b).

These potential mediators/moderators play an important role in how an educational, extended contact intervention is expected to work to reduce sexual prejudice. An in-group member serves as an approachable and trusted messenger of positive information about the out-group. An in-group member can also model a positive intergroup relationship with the out-group, showing that in-group members can have positive interactions (even friendships) with members of the out-group. For these reasons, extended contact within the context of an educational intervention is a promising way to reduce sexual prejudice among the heterosexual population.
Sexual Prejudice and Subtle Discrimination

The previous section highlighted the main components of prejudice reduction research, but we have yet to delve deeper into theories that give us a better understanding of what, exactly, prejudice reduction means. Next, I will discuss what prejudice and discrimination is, and how it can be operationalized and measured. Prejudice has been most recently conceptualized by social psychologists as “an individual-level attitude toward groups and their members that creates or maintains hierarchical status relations between groups” (Dovidio et al., 2010, p. 7). Sexual prejudice is therefore an attitude toward marginalized groups based on sexual identities such as lesbian, gay, bisexual, or transgender (Herek & McLemore, 2013).

Discrimination is the behavioural expression of prejudiced attitudes, manifested as the negatively biased treatment toward individuals based on their group membership (Abrams, 2010; Dovidio et al., 2010). When research attempts to reduce prejudice, it usually seeks to also reduce discrimination, whether measured in the research design or just hoped for as a real-world consequence of the research. As Dixon and colleagues put it, “the aim of prejudice-reduction interventions is not merely to get dominant group bigots to have nicer thoughts and feelings about members of other groups” (Dixon et al., 2016, p. 6). Although the concepts are absolutely related, factors such as social desirability bias and motivation to control behaviour affect the attitude-behaviour correspondence and necessitate measuring both prejudice and discrimination if one truly wants to determine the reduction of both (Fingerhut, 2011; Kite & Whitley, 2016).

While the term sexual prejudice refers only to prejudice associated with sexual orientation, transgender and other gender-diverse individuals are often included under the umbrella of sexual minorities (e.g., the LGBT social grouping); therefore, I chose to include transgender individuals from the umbrella of sexual prejudice for the purposes of this study.
Discrimination can be blatant or subtle. The former is obvious, and often hostile, including verbal or physical assault, or refusing services or employment to someone based on their race, sexual orientation, or other marginalizing status. The latter is much more insidious; legal protections exist in Canada to protect LGBTQ+ individuals from blatant discrimination (e.g., human rights, employment, etc.), but subtle discrimination is impervious to legal protections because it can be hidden or disguised. In a process similar to the concepts of aversive racism or benevolent sexism, subtle discrimination against members of the LGBTQ+ population may present itself in the form of microaggressions, insults, or negative biased treatment that may be unintentionally perpetuated by individuals who are not explicitly prejudiced (Nadal et al., 2010; Sue, 2010). For example, a heterosexual individual telling a gay friend to act straight for a job interview may be intended as a helpful suggestion, but it represents a form of subtle discrimination because it suggests that being gay is inferior to being straight, and it perpetuates the blatant discriminatory pattern of denying jobs to people who belong to what are seen as inferior groups.

Although not necessarily intentionally harmful (in contrast with blatant discrimination), subtle discrimination can be detrimental to psychological wellbeing, can lead to health disparities, and workplace inequities (see Jones et al., 2013 for a meta-analysis). In fact, research has shown that subtle forms of discrimination are just as harmful to people’s psychological wellbeing as overt discrimination (Jones et al., 2013) and some studies have found that subtle discrimination is even more prevalent than its overt counterpart (Rankin et al., 2010; Van Laer & Janssens, 2011; Woodford et al., 2014).

Measurement of prejudice and discrimination requires an awareness of these nuances in operationalization and expression of the constructs. Prejudice, for instance, can be measured
with traditional, self-report measures, or with implicit assessment tools that measure unconscious attitudes (Herek & McLemore, 2013). The most widely used self-report measure of sexual prejudice are the attitudes toward lesbians (ATL) and attitudes toward gay men (ATG) scales by Herek (1988), which include items such as *Female [male] homosexuality is a perversion*, rated on a Likert scale of agreement/disagreement. Another common explicit measure is the attitude thermometer, which is set up like a temperature thermometer with higher temperatures indicating more positive attitudes toward gay men and lesbians. This simple scale has been found to be effective at measuring homophobia (Chonody, 2013; Esses et al., 1993).

Self-report measures are more common than implicit assessments, but they come with the risk that responses will be skewed to appear more socially desirable; that is, a highly prejudiced person may attempt to make themselves seem less prejudiced on a self-report questionnaire if that person is aware of social norms against being overtly prejudiced. However, many researchers report that the correlation of such explicit prejudice measures with social desirability is low enough to give an accurate estimate of actual prejudice levels (Morrison & Morrison, 2003; Siebert et al., 2014). For example, the ATL scale was correlated at a mere $r = .11$ with an impression management scale measuring tendency toward biased responding (Siebert et al., 2014).

Researchers concerned with the potential bias in self-report may choose to measure implicit prejudice instead. For example, the implicit association test (IAT; Greenwald et al., 1998) is a timed, computerized task in which participants are asked to pair concepts such as good and straight, or bad and gay. Implicit bias is measured by response time; that is, if a person is quicker to pair good + straight than good + gay they are assumed to be biased in favour of people who are straight – the ease and therefore speed with which associations are made are indicative
of bias (Nosek et al., 2014). The IAT has been found to more reliably predict discriminatory behaviours than its explicit, self-report counterparts (Rudman & Ashmore, 2007). One criticism of using the IAT to measure prejudice is that it specifically measures unconscious associations, which some researchers believe have not yet been empirically equated to the more complex concept of prejudiced attitudes or discriminatory behaviours (Azar, 2008).

Another option for researchers who are concerned with social desirability bias but who want to tap into explicit attitudes rather than unconscious bias is to select a more modern scale that is less obviously negative in its wording. For example, the sexual prejudice scale (Chonody, 2013) includes many items related to current LGBTQ+ issues and utilizes more reverse scored items to minimize the negative tone: e.g., Retirement benefits should include the partners of gay men. There are many scales and subscales available to measure sexual prejudice, homophobia, transphobia, heterosexism, homonegativity, and a plethora of other related concepts. It is up to the researcher to determine what scale is most appropriate with their operationalization of prejudice.

Measuring discrimination can be more complicated than prejudice because discrimination is a behaviour – people may behave differently under the watchful eye of a researcher than they might in a natural setting. Using deception to measure discrimination or discriminatory intentions is one way to avoid response bias. A commonly used measure for this purpose is the budget cuts task, where participants are tasked with cutting the budgets of various organizations due to an ostensible budget crisis, and cutting the budget of a target organization (such as the LGBTQ+ student society) over other organizations is equated with discrimination against that target group (Son Hing et al., 2002).
Another common way to measure discriminatory intentions is to test willingness to hire a target group member versus an in-group member for a hypothetical employment opportunity. In one study, heterosexual participants were shown one of two experimentally manipulated candidates running for mayor – one was gay, the other straight, but otherwise the candidates were identical (Morrison & Morrison, 2011). A behavioural intentions scale was created that asked how likely participants would be to vote for the candidate. A greater likelihood of voting for the gay candidate was equated with lower discriminatory intentions (Morrison & Morrison, 2011).

One way of measuring discrimination is to assess the rates and experiences of people who are discriminated against (e.g., everyday discrimination scale; Forman, Williams, & Jackson, 1997; or major experiences of discrimination scale; Williams et al., 2008). However, this method of measurement cannot be used to determine if an intervention has an effect on a person’s likelihood of committing discrimination. As with measures of prejudice, the most appropriate measure of discrimination depends on the goals of the research. In studies focusing on the advantaged group members who discriminate against an outgroup – and the conditions which might alter their likelihood of discriminating (e.g., experiences of intergroup contact) – measurement methods such as voting or hiring intentions or a budget cuts task are ideal.

Although anyone can be prejudiced or discriminate against sexual and gender minorities (e.g., even LGBTQ+ individuals can hold internalized prejudice against their own social identity), certain demographic variables are particularly predictive of sexual prejudice and discrimination. One of the most telling variables is gender: men are consistently more prejudiced against and discriminatory toward sexual and gender minorities than are women, and tend to show less reduction in prejudice after an intervention (Ahrold & Meston 2010; Finlay &
The literature also suggests that there are interaction effects of gender, such as heterosexual men being more prejudiced against gay men than against lesbians (Herek, 2000; Herek & Gonzalez-Rivera, 2006; Norton & Herek, 2013; Rampullo et al., 2013). These gender differences have been attributed to the socially constructed concepts of masculinity and femininity; that is, sexual prejudice among heterosexual men is a mechanism to re-affirm their masculinity (Glick et al., 2007). Furthermore, reaffirming one’s prototypical gender identity is much more of a social pressure for men than it is for women (Basow & Johnson, 2000; Glick et al.; Herek & McLemore, 2013). In addition to gender expressions (i.e., masculinity and femininity), having traditional gender role attitudes has also been found to predict sexual prejudice and discrimination among both men and women (Basow & Johnson, 2000; Goodman & Moradi, 2008).

Religiosity is also strongly and consistently related to sexual prejudice (Cluse-Tolar et al., 2004; Finlay & Walther, 2003; Hinrichs & Rosenberg, 2002). In an important past study by Allport and Ross (1967), individuals who attended church regularly were found to have more prejudiced attitudes than those who did not attend church. Interestingly, the relationship between churchgoers and prejudice was curvilinear. This pattern was explained by two types of religious orientation: intrinsic and extrinsic. Intrinsic religious orientation is the mindset that religion serves as an end in itself, and people with an intrinsic orientation are more likely to be kind to all human beings on behalf of their religions (Allport & Ross, 1967). An extrinsic religious orientation is a means to social status; so, if one could gain status by being prejudiced (if prejudice against a group was a social norm), the extrinsically oriented religious person would be more likely to be prejudiced (Allport & Ross, 1967).
Interestingly, sexual prejudice and religiosity seem to follow a different pattern than racial and ethnic prejudice and religiosity. Among U.S. Christians, those who are prejudiced against other races or ethnicities are more likely to have an extrinsic religious orientation (Allport & Ross, 1967). Those who are prejudiced of sexual minorities, on the other hand, are more likely to have an intrinsic religious orientation (Hunsberger & Jackson, 2005). This is likely because an intrinsically oriented religious person is motivated by the dictates of their religion, and while their religion might condemn hatred for others, it might also condemn homosexuality on grounds of morality and thereby support hatred of sexual minorities (Whitley, 2009).

Another important construct closely related to religiosity is right-wing authoritarianism (RWA; Johnson et al., 2011). Right-wing authoritarians believe in punishing evildoers, obeying authority, and upholding conventional values and morals, but these values are not necessarily driven by religious belief (Altemeyer & Hunsbrger, 1992). Most RWA measures are composed of three personality dimensions: conventionalism, submission to authority, and aggression toward norm violators (Altemeyer, 1981; Funke, 2005; Van Hiel et al., 2007). Right-wing authoritarianism is a commonly considered covariate or moderator in studies looking at the relationship between religiosity and sexual prejudice, and is also correlated with self-reported anti-LGBTQ+ behaviour/discrimination (Ford et al., 2009, Goodman & Moradi, 2008; Jonathan, 2008, Laythe et al., 2001; Stefurak et al., 2010). Furthermore, measuring right wing authoritarianism is particularly relevant to the current U.S. social climate where conventional views and aggression toward norm violators is espoused by the political administration and its supporters (Abramowitz & McCoy, 2018; Edsall, 2019; Eligon, 2018; Leonhardt & Philbrick, 2018; Rubin, 2018).
Allies

Up to this point I have covered the factors generally associated with prejudice and discrimination reduction and the details associated with their effectiveness. There is one additional and vital component that remains – the component that makes this research unique in the field of prejudice reduction via extended contact: the ally as an in-group contact. The ally represents an in-group contact who explicitly supports LGBTQ+ issues, and actively works to change prejudiced attitudes among their fellow in-group members. Research has not yet addressed the nuances of the extended contact’s identity, and it seems pertinent to test the importance of a contact’s explicit support for LGBTQ+ issues. With this gap in the research, extended contact theory may be misused; just as when direct intergroup contact backfires due to a negative interaction, the benefits of extended contact could be lost, or even reversed, if the in-group member is ambivalent about LGBTQ+ issues (Mazziotta et al., 2015; Shelton et al., 2009). The ally is the quintessential representation of this type of explicit support for the out-group.

An ally is a member of a privileged group who challenges oppression against other groups even if that oppression is what provides their own group with privilege (Bishop, 2002; Washington & Evans, 1991). For example, an ally could be a white person who challenges racism, or a heterosexual who challenges sexual prejudice and discrimination. A key component of many definitions of ally is action; that is, an ally is someone who acts as an agent for reducing prejudice in others, and who works to end oppression (Brooks & Edwards, 2009; Herek & McLemore, 2013; Ji, 2004; Washington & Evans, 1991).

Ally is a social identity (Ji & Fujimoto, 2013). Social identity theory states that self-concept is created by membership in certain groups or social categories (Luhtanen & Crocker, 1992; Tajfel, 1982; Tajfel & Turner, 2001). An individual holds many social identities at one
time; for example, an ally also holds a heterosexual identity, as well as ethnic, socioeconomic, and other identities (Burke, 2003). Research suggests that there are two primary types of social identity: ascribed and achieved identity (Deaux et al., 1995). An ascribed identity is automatically assigned based on characteristics of the individual; for example, a First Nations person is ascribed to that identity because they were born into it – it is unchangeable. An achieved identity is one that must be actively attained, such as Engineer, or parent (Deaux et al., 1995; Jenkins, 2008). An ally is an achieved social identity, because one must actively seek social justice for a marginalized group to identify as an ally. The ally identity has become so enmeshed in the sexual and gender identity social group that in some cases the LGBTQ+ acronym has grown in include an A for ally.8 Given their connection to both the in-group (in this case, heterosexual) and the out-group (in this case, LGBTQ+), allies are in a unique position to bridge the social power gap between groups.

Often, an individual acts as an ally to protect a close friend or family member who belongs to the LGBTQ+ group from the harms of prejudice and discrimination (Edwards, 2006; Russell, 2011; Vernaglia, 1999). This type of ally may look like a parent with a transgender child who marches in a Pride Parade holding a sign that says “I love my trans child;” or someone with a gay best friend who actively and verbally defends the legitimacy and value of their friend’s identity to their broader social circle, for example. This motivation is known in the literature as an ally for self-interest; it is labeled self-interest because the individual wants to protect someone

8 There is no consensus on the full acronym (i.e., LGBTTI2QAA, and other variations). In some cases the A stands for asexual or agender, and in other cases an additional A is for ally.
they care about, as opposed to a more social-level drive to eliminate inequalities on a larger scale (Bishop, 2002; Duhigg et al., 2010; Goldstein & Davis, 2010).

In addition to having someone in particular to stand up for, allies may be motivated by perceived social injustice. This introspective, socially-aware motivation can be considered a next level in ally identity development, and it usually follows a trigger event (i.e., a “concrete, real-life encounter with oppression and privilege”). As a next step in ally development, a truly engaged ally will self-reflect on their privilege and their values and will choose to favour social justice over maintaining their privilege (Bishop, 2002; Edwards, 2006; Duhigg et al., 2010, p. 10). That is, the most socially conscious motivation to become an ally is based on an understanding of privilege and oppression as a problematic system and a desire to change that system (Goldstein & Davis, 2010).

In practice, allies are ordinary people who, through one means or another, have come to realize that an out-group is unjustly treated, and has set out to defend that group even if it means putting themselves at odds with the norms and values of their own in-group. The ability of allies to bridge the gap between the in-group and the out-group makes them ideal to explain extended contact’s effects. Their membership in the heterosexual in-group provides ample interaction opportunities, the root of their motivation is likely to involve an out-group friend or family member, and their commitment to social justice makes them ideal for spreading anti-prejudice messaging to other in-group members. Prejudice reduction researchers may be able to learn from allies and use the concept of ally to increase efficacy through experimental interventions.

**Summary**

Intergroup contact theory offers a well-tested and reliable intervention to reduce prejudice, but it has its limitations. First, highly prejudiced individuals are unlikely to have the
opportunity for positive interactions with a member of the out-group they are prejudiced against, and if the interaction is not high quality and positive, it can lead to more prejudice (MacInnis & Page-Gould, 2015; Vonofakou et al., 2007). Second, interacting with an out-group member can induce feelings of anxiety over anticipation of the interaction going badly, or not knowing what to expect of the interaction (Wright et al., 1997). In these cases, it may be beneficial to rely on extended contact for optimal prejudice reduction conditions. According to extended contact theory, simply observing or knowing of a fellow in-group member having a successful contact or friendship with an out-group member is enough to reduce prejudice (Wright et al., 1997). The ally is a prime example of an in-group member in contact with, and in support of, the out-group. The incorporation of the ally identity has not yet been studied within the context of extended contact effect, despite its potential benefits over and above intergroup contact theory. This program of research will explore the effectiveness of allies in reducing prejudice and discrimination against gay men and lesbians; it will also explore the impact of several key mediating variables that have been shown to influence prejudice reduction.
CHAPTER 3

STUDY 1: METHODOLOGY

Design and Hypotheses

Study 1 was described to participants as a review of an educational video; random assignment to 1 of 4 videos in which an actor/target self-disclosed differently tested the effects of identity of a person delivering anti-prejudice messaging on explicit prejudice and subtle discrimination. This study utilized a 2 (gender of participant: male, female) x 2 (sexual orientation of target: straight, gay) x 2 (explicit support for LGBTQ+ issues: ally/advocate, non-ally/non-advocate) between-subjects experimental design. The levels of the two manipulated independent variables are combined across four different manipulations, as outlined in Table 2.

Table 2

Summary of manipulated conditions

<table>
<thead>
<tr>
<th>Level</th>
<th>Sexual Orientation of Target</th>
<th>Type of Intergroup Contact</th>
<th>Support for LGBTQ+ Issues</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ally</td>
<td>Straight</td>
<td>True extended contact, positive and close relationship with out-group friend.</td>
<td>Explicit support, personally passionate about reducing prejudice/discrimination</td>
</tr>
<tr>
<td>Advocate</td>
<td>Gay</td>
<td>Direct contact</td>
<td>Explicit support, personally passionate about reducing prejudice/discrimination</td>
</tr>
<tr>
<td>Non-ally</td>
<td>Straight</td>
<td>Technically extended contact but neutral and weak relationship with out-group member.</td>
<td>Not explicitly supportive of LGBTQ+ issues</td>
</tr>
<tr>
<td>Non-advocate</td>
<td>Gay</td>
<td>Direct contact</td>
<td>Not explicitly supportive of LGBTQ+ issues</td>
</tr>
</tbody>
</table>
Because the effect of direct intergroup contact is so well supported in the literature (see for e.g., Bartoş et al., 2014; Paluck et al., 2017), I would be remiss if I did not hypothesize a main effect of target’s sexual orientation; although not a unique aspect of my theoretical framework, testing this hypothesis serves as a manipulation check, in a sense, to ensure that the experimental model works as expected based on what is already known about intergroup contact’s efficacy in the literature. Therefore, I predicted that, across all levels of target’s explicit support for LGBTQ+ issues, the gay target would produce lower prejudice and discrimination scores than the straight target (H1). I did not expect a main effect of target’s support for LGBTQ+ issues, because the collapsing of gay and straight targets within the two levels of this condition may confound the overall effects; that is, the superior effectiveness of gay targets in both the advocate and non-advocate levels may balance out any differing effects of the straight ally versus straight non-ally to a point where a main effect of support for LGBTQ+ issues is undetectable. My main thesis in this research program is that allies can increase the effects of extended contact over and above direct contact with a gay target. Therefore, I predicted a two-way interaction of target’s support for LGBTQ+ issues and target’s sexual orientation, such that the straight ally target would produce lower prejudice and discrimination scores than all other conditions (H2).

Consistent with the plethora of sexual prejudice research, I predicted multiple effects associated with the gender of participant. I hypothesized a main effect of gender of participant, such that men would have higher prejudice and discrimination scores than women (H3). Furthermore, this main effect would be qualified by a two-way interaction of participant gender and sexual orientation of target showing that men have higher prejudice and discrimination than women against the gay target (H4); and that men in the gay target groups have higher prejudice
and discrimination than men in the straight target groups (H5). Finding these interactions would suggest that extended contact with an ally is particularly important for men in reducing sexual prejudice and discrimination.

**Methodology**

**Population Selection and Recruitment**

The population of interest for this program of research was people living in the United States who were diverse in terms of state of residence and basic demographic variables. There are two main reasons why I focused on recruiting a sample from the United States. Firstly, for optimal intergroup contact conditions, participants need to have a common in-group with the targets, and nationality is an ideal identity to utilize (Allport, 1954; Gaertner & Dovidio, 2000; Wright et al., 1997). An American identity was chosen instead of a Canadian identity in an effort to avoid floor effects in the generally more progressive Canada; political attitudes, along with governmental policies and laws regarding same sex issues, such as marriage, have been shown to be generally less progressive in the United States versus Canada (Morrison et al., 2009; Pew Research Center, 2013). Furthermore, a meta-analysis of intergroup contact's effects on sexual prejudice found overall stronger effects of contact when studies were conducted in the United States versus any other country included in analysis (Smith et al., 2009).

Because of the experimental nature of the research, a large number of participants was needed over a relatively short timeframe. Amazon’s Mechanical Turk (MTurk) was used for recruitment in order to reach the large sample required. MTurk is an online network connecting employers and researchers to people who complete short, online tasks for monetary compensation. An online recruitment method was particularly important given that recruitment was in a different country from the researcher’s own location. This method of recruitment is
growing in popularity among academic researchers and has consequently been well tested for reliability and representativeness (Paolacci & Chandler, 2014). In tests of MTurk’s psychometric quality, Buhrmester and colleagues (2011) found a mean alpha of $\alpha = .87$ and high test-retest reliabilities at 3 weeks apart ($r = .88$). MTurk participants have also been found to be more demographically diverse than typical internet recruited samples or university student samples (Buhrmester et al., 2011; Paolacci & Chandler, 2014).

The use of MTurk allowed me to pre-screen a sizeable overall sample of 7094 participants; pre-screening on such a large scale was necessary to select only participants who did not have previous contact with members of the LGBTQ+ group, were not themselves members of the LGBTQ+ group, and to oversample toward an equal distribution of educational attainment.

**Materials**

**Pre-screening Survey**

The pre-screening survey (see Appendix A) included a Reflect Me scale (Vagias, 2006) to select out members of the LGBTQ+ group and included the question: “To what extent is this true of you: I am a member of the LGBTQ+ community.” LGBTQ+ was defined in the survey. Possible responses were (1) untrue of me, (2) I don’t know, and (3) true of me, and only those who responded with a score of 1 met this eligibility requirement.

To control for pre-existing direct intergroup contact, participants were asked, “Do you personally know anyone who identifies as LGBTQ+?” Based on past literature, I also assessed frequency of interaction with an LGBTQ+ person on a scale from 1 = “never” to 5 = “more than once a week;” Capozza et al., 2014; Dhont & Van Hiel, 2011; Mazziotta et al., 2015). I chose to control for previous direct contact by recruiting only those with no (or limited) previous contact.
because past literature has shown that extended contact is most effective when the sample has infrequent or low quality previous contact (Cameron et al., 2011; Christ et al., 2010; Dhont & Van Hiel, 2011; Eller et al., 2012). Therefore, anyone who responded “no” to knowing a member of the LGBTQ+ group were automatically eligible to participate further, but also, those who responded “yes” were eligible if they interacted with a member of the LGBTQ+ group less than once a month. This frequency was selected for eligibility somewhat arbitrarily, based on the notion that contact less than once a month reflects a more distant relationship; however, ultimately I chose to enroll participants with up to this level of contact because pilot testing showed an insufficient number of potential participants with no previous contact (i.e., 2% of the pilot sample had no previous contact, 5% had contact less than once a year). A more reasonable proportion of the pilot sample (28.6%) had contact less than once a month, and therefore this frequency offered a measure of low contact that did not eliminate an unreasonable proportion of the sampling pool. On some occasions (n=10), participants claimed to have contact with a member of the LGBTQ+ group more than once a month but also said that they did not personally know anyone who was LGBTQ+. I retained the data from these participants because contact theory functions best when contact is close and meaningful, and previous contact is thus less likely to confound study results if participants’ LGBTQ+ contact is not personally known (Gómez et al., 2011; Hodson et al., 2009; for a review, see Turner et al., 2010).

The pre-screening survey was also used to oversample participants with low-educational achievement on account of findings from the pilot studies. That is, the sample was highly skewed with high educational attainment; furthermore, education was negatively correlated with RWA, suggesting that retaining a highly educated sample would also have led to a sample with an unrepresentatively low level of RWA. The decision to oversample for low education was also
supported by the literature which highlights a negative relationship between education and prejudice (Hagendoorn & Nekuee, 2018; Herek & Gonzalez-Rivera, 2006; West & Cowell, 2015). My goal in oversampling low educational attainment was to ensure an appropriate representation of all types of education. In this initial phase, only those participants who had vocational/trade school as their highest level of education were invited to complete the rest of the survey. Once approximately half of the required sample was collected following this criterion, the education eligibility criteria was dropped from the pre-screening survey, opening participation to any and all levels of educational attainment.

**Pre-test**

The second component of the recruitment process included a pre-test (see Appendix B) that measured basic demographic questions not already covered in the pre-screening survey (e.g., gender, age, ethnicity). In lieu of an income question, participants completed the MacArthur scale of subjective social status (Adler & Stewart, 2007), wherein they subjectively ranked themselves in comparison to other Americans on their social status regarding money, education, and the degree to which they feel their jobs are respected.

In addition to basic demographic questions, participants were assessed on their baseline levels of intergroup anxiety when thinking about interactions with a gay man. As a common moderator in extended and direct contact research, the intergroup anxiety variable was included in an attempt to control for pre-existing anxiety that might impact how a participant responded to the target in the gay target manipulation. Intergroup anxiety was measured using a variation of the Intergroup Anxiety Toward African Americans scale that had been modified for a gay target (IATAA; Britt et al., 1996). This scale has been modified in the past to measure anxiety toward Muslims ($\alpha = .86$; Murrar & Brauer, 2017), as well as toward lesbians and gay men (Intergroup...
Anxiety Toward Homosexuals Scale; Muñoz, 2003). In Muñoz’s modification, the original 11 item scale was expanded to 29 items to include opportunities to respond for both lesbians and gay men, as well as some general homosexual target items. Because my study design involved contact with a gay man (and not with a lesbian), I adhered to the original 11 item version, including only the items specifying gay men. An example of the scale items is, “I just do not know what to expect from a gay man.” The scale items are rated on a 5-point scale from (1) strongly disagree to (5) strongly agree, and items were averaged to create a mean score for each participant. Greater values are indicative of greater anxiety. Internal reliability in my sample was acceptable at $\alpha=.82$.

Finally, the right wing authoritarianism scale (covariate) was also measured in the pre-test. Right wing authoritarianism (RWA) was measured using Altemeyer’s (2006) scale. This 22-item scale is scored from (-4) very strongly disagree to (+4) very strongly agree, making a total of 9 possible scores. An example item is, “the established authorities generally turn out to be right about things, while the radicals and protestors are usually just ‘loud mouths’ showing off their ignorance.” The first 2 items in this scale are considered test items and are removed from analysis, with the remaining 20 items summed to create a total RWA score for each participant. Greater values are indicative of greater authoritarianism. An abundance of past literature has shown that this scale has high internal reliability, at least when used in predominantly white populations (Bizumic & Duckitt, 2018; Edwards & Leger, 1995; Saunders & Ngo, 2017). Data from the present sample showed consistently high reliability, $\alpha = .95$.

**Manipulation**

Consistent with a strategy used in other prejudice reduction research (see, for e.g., Murrar & Brauer, 2017), the manipulation involved viewing an educational video about sexual
orientation, and about the prejudice against LGBTQ+ people. An actor played a different role in four videos, corresponding to the levels of the target’s explicit support for LGBTQ+ issues variable. The levels were differentiated by a self-disclosure by the actor. Aside from the self-disclosure at the beginning of each video, all videos were identical. Table 3 shows the statements used to set up the conditions:
Table 3

*Self-disclosure scripts for 4 experimental conditions*

<table>
<thead>
<tr>
<th>Version</th>
<th>Self-Disclosure Script</th>
</tr>
</thead>
</table>
| Version 1       | **Straight Ally**

“Hi, I’m Michael, I’m from Anaheim, California, I’m a law student, and I’m making this video for an assignment for a class on anti-discrimination and labor law. **I plan to work as a human rights lawyer to fight for rights for the LGBT community.** You might assume I’m gay because I care about this cause, but **I’m straight. I have a close friend who’s gay,** and he has experienced prejudice and discrimination. **I’m an ally for gay rights, trying to make positive change** so other gay, lesbian, bi and trans people don’t have to go through the same thing.” |

| Version 2       | **Gay Advocate**

“Hi, I’m Michael, I’m from Anaheim, California, I’m a law student, and I’m making this video for an assignment for a class on anti-discrimination and labor law. **I plan to work as a human rights lawyer to fight for the rights for my fellow LGBT community.** As a gay man, I have experienced prejudice and discrimination and **I want to make positive change** so other gay, lesbian, bi and trans people don’t have to go through the same thing.” |

| Version 3       | **Straight Non-Ally**

“Hi, I’m Michael, I’m from Anaheim, California, I’m a law student, and I’m making this video for an assignment for a class on anti-discrimination and labor law. **I plan to work as a lawyer** when I graduate. **Even though I did know someone in my high school who was gay, that was the extent of my connection** to the gay community. So, a lot of the information we researched for this video was new to me. If it’s new for you too, I hope you find it as interesting as I did.” |

| Version 4       | **Gay Non-Advocate**

“Hi, I’m Michael, I’m from Anaheim, California, I’m a law student, and I’m making this video for an assignment for a class on anti-discrimination and labor law. **I plan to work as a lawyer** when I graduate. Even though **I’m a gay man,** a lot of the information we researched for this video was new to me, and if it’s new for you too, I hope you find it as interesting as I did.”

The American in-group identity was made salient through the self-disclosure that the actor is from Anaheim, California. The city was chosen at random from a State whose population has an accent similar to that of many Canadians, thus enabling the use of a Canadian actor (Harbeck, 2014).
Aside from enabling the actor to create priming for the various experimental conditions, the decision to include the actor’s self-disclosures is that this strategy was expected to make the actor more likable; even via a computer screen, personal self-disclosures have shown to increase liking (Kashian et al., 2017).

The remaining content of the videos was based on other similar educational strategies and workshops to reduce sexual prejudice (Murrar & Brauer, 2017; Riggs et al., 2011; see also Paluck & Green, 2009 for a review). The actor gave a brief description of sexual orientation and statistics about how diverse the population really is with regards to sexuality. The psychological phenomenon of stereotype formation was briefly described, and an example of a debunked negative gay stereotype was provided. To tie the video into the law school assignment guise introduced in Table 3, some facts were shared about the current status of US federal LGBTQ+ legal rights. Examples were provided about how prejudice and discrimination affect LGBTQ+ people negatively, as well as some ideas of how to respond to sexual prejudice and discrimination when it is observed.

The video maintained a positive tone – despite the necessity of including negative facts about how people are impacted by prejudice and discrimination, the actor was directed to avoid using a disapproving or condemning tone when addressing the heterosexual audience, and to keep the mood of the video conversational (e.g., “did you know that the human brain has ways to take mental short cuts when we approach someone or something new? It’s because of our super speedy brains that we end up stereotyping people…;” see Appendix C for full video transcript).

A professional videographer assisted in the making of the video, but the video was edited to have a less professional appearance and to be believable as a graduate class project made by non-film students. A pilot study conducted prior to the full study confirmed that the quality of the video
was attention-grabbing and visually pleasing but was also believable as a video made by law students for a class project. Details of the pilot study are described in a separate section.

**Post-Manipulation Survey**

The main survey conducted post-manipulation began with decoy items to distract from the main outcome measures of prejudice and discrimination. Participants were given the opportunity to describe, in open-ended format, what they liked and did not like about the video, and to share three things they learned from the video (Appendix D). In addition to functioning as distractor items, these open-ended questions served as an opportunity for participant reflection and a manipulation check of attention to the video content. The Reysen likability scale (Reysen, 2005) was also included as a manipulation check to ensure that any effects found were not the result of simple differences in the perceived likeability of the target across conditions. This measure was also important to include because liking the target is important to the success of both intergroup and extended contact (Davies et al., 2011; Turner et al., 2007). The Reysen Likability Scale is an 11-item scale rated on 7-points ranging from (1) *very strongly disagree* to (7) *very strongly agree* (Reysen, 2005); items were averaged to create a mean score for each participant, with greater values indicative of higher rated likeability. Data from the present study showed good reliability for this scale (**α** = .92).

The first outcome variable, explicit sexual prejudice, was measured using a modified version of Chonody’s (2013) Sexual Prejudice Scale. Two subscales of the original sexual prejudice scale were retained, totalling 20 items on a 5-point Likert scale from 1 “strongly disagree” to 5 = “strongly agree.” Items were averaged to create a mean score for each participant, where greater values are indicative of more prejudiced attitudes. Chonody’s original (2013) sexual prejudice scale contains four individually validated subscales which account for
four well known factors in prejudice formation: stereotypes, valuation, affect, and social equality beliefs (also known as symbolic). In tests of this four-factor conceptual model, the affective and symbolic components have been found to be the most uniquely predictive of prejudiced attitudes, above and beyond stereotypic beliefs (Haddock & Zanna, 1999). Because of the predictive ability these factors, I only used the affective-valuation subscale and the social equality beliefs subscale to assess sexual prejudice in an effort to minimize the length of the survey.

Previously reported reliability for the sexual prejudice subscales is strong; the scale as a whole has also performed well in an expert panel test of content and face validity and showed predictive validity with contact with an LGBTQ+ person, along with Herek’s Attitudes Toward Lesbians and Gays Scale (ATLG), the evaluation thermometer of attitudes, and the Old Fashioned Sexism Scale (Haddock & Zanna, 1999; Swim et al., 1995). My own reliability analysis showed both the affective-valuation subscale and the social equality beliefs subscale for attitudes toward gay men had Cronbach alpha coefficients of .95; for attitudes toward lesbians, .93 for the affective-valuation subscale, and .90 for social equality beliefs.

The second dependent variable, subtle discrimination, was measured using a budget cuts task which has been used successfully in other studies (on discrimination against homosexuals in Haddock et al., 1993; and racism in Son Hing et al., 2002). In this task, participants read a statement about how the government is forcing a 20% cut to social services, and we, the researchers, are interested in hearing the public’s views on how the limited funds should be allocated. Furthermore, participants were told that their suggestions made for budget allocation would be presented to policy makers and may actually inform budget decisions. The premise chosen for this task is a version of the original design by Haddock et al.’s (1993) budget cuts experiment. While the original version of the budget cuts task utilized fictional budget cuts to
university organizations, I presented a list of U.S. national organizations because participants were from a U.S. non-university sample. Participants were given a list of 5 real social service programs/organizations and their descriptions, along with the actual funding these organizations received in the past year, including one LGBTQ+ organization (Korte, 2017; see Appendix D, Table D1 for organization names, descriptions, and funding allocations). Participants were asked to reduce and re-allocate the funds for the 5 organizations in order to reduce the overall funding by 20%; the more money cut from the LGBTQ+ organization is associated with greater subtle discrimination.

**Pilot studies**

Now that the research content has been comprehensively defined, the pilot studies that were conducted prior to Study 1 data collection can be clearly and briefly described. Two pilot studies were conducted to assess the quality of the manipulation and believability of the deception, as well as to determine typical MTurk sample norms on our variables of interest. For the first pilot, 40 participants were recruited through MTurk and completed the full Study 1 protocol (i.e., pre-screening survey, manipulation, and post-manipulation survey) as well as an additional pilot survey (see Appendix E for separate pilot study survey). However, the pilot did not employ a 2-week break following the pre-screening survey as was the case in the actual study. The whole series of surveys (without the separate pilot survey) took participants an average of 24 minutes to complete (mean and median were the same), with a minimum time of

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9 The LGBTQ+ organization was the Human Rights Campaign (with service to LGBTQ+ population made explicit in description), and the 4 decoy organizations were: Great Lakes Restoration Initiative; African Development Foundation; National Wildlife Refuge Fund; U.S. Interagency Council on Homelessness.
10.88 minutes, and maximum time of 38.11 minutes. This assessment provided the estimate that the main study would take participants 24 minutes to complete. Pilot participants were paid $5.00 USD to complete the series of surveys. Participants were adequately diverse on some variables, with an age range of 24 to 67 (M=38), and a range of locations with 40 participants located across 20 of the United States. A large majority of participants identified as White (85%). Education was another variable that lacked diversity in the pilot sample. The majority (82%) of participants had completed at least some college or university; because higher education is negatively correlated with sexual prejudice, I was concerned that a sample high in education would result in floor effects for our manipulation (Kent et al., 2013). This issue influenced my recruitment decision in Study 1.

This pilot study found that Right Wing Authoritarianism scores were lower than expected based on past research. Altemeyer (2006) shares RWA standards for both Canadian university students (M = 75) and an average American sample (M=90). The mean RWA score of my pilot data was M = 56.58 (SD = 34.36) and the lowest score was 20. This lowest possible score was not an outlier, nor was it due to a failure to respond to the question meaningfully (all participants retained in this sample passed attention checks). Given the social context of the population the sample was drawn from (i.e., “Trump’s America;” Abramowitz & McCoy, 2018), it was somewhat surprising to find that our sample scored almost 20% lower than expected for their demographic.

Because the RWA scores were lower than expected based on past studies, I conducted an ANOVA to determine the effect of various demographic variables on RWA. Although the sample was too small to warrant a meaningful statistical test, upon a visual analysis of the mean differences it became clear that level of education impacted the outcome variables. Specifically,
those with high education had lower RWA and prejudice scores. With the large majority of participants either in college/university or with degrees and advanced degrees, an assumption was made that the MTurk sample I tapped into was of a more progressive, left-leaning mindset because of their level of education, and perhaps also due to current participation in college/university culture (Henry, 2009). To test this prediction, a second pilot study was conducted wherein the sample was selected based on education, as well as the original eligibility requirements of being heterosexual/cisgender and knowing an LGBTQ+ person.

Fortunately, the pilot test of the manipulation (Pilot 1) did not result in any report from participants of problems with the materials or design; for example, the surveys functioned and were easy to read and interpret, and random assignment to conditions was successful. The actor was acceptably likeable with a mean of $M = 5.97$, $SD = .88$, on a scale from 1 to 7 with 7 being the most likeable. A series of manipulation checks given after the debriefing assessed the believability that the actor was a law student and that the video was for a class project (85.7% believed both deceptions), as well as the believability of the actor’s disclosed sexual orientation (94.3% believed) and that the actor was American (88.6% believed; see Appendix E for pilot question wording and scales).

Finally, the pilot survey assessed the perceived attractiveness and masculinity of the actor. Attractiveness was assessed on the premise that moderately attractive confederates are more influential, likeable, and are perceived as more credible sources of information (Horai et al., 1974; Pornpitakpan et al., 2017). I measured attractiveness of the actor used in the video with a scale from $1 = “extremely unattractive”$ to $10 = “extremely attractive;”$ the resulting mean for attractiveness was a moderate $M=7.95$, $SD=1.39$. Masculinity was assessed alongside the manipulation check of believed sexual orientation. I posited that, based on stereotypes of
effeminate gay men, the actor would be more believable as gay or straight in the various conditions if he was moderately masculine; if too masculine, he might not be believably gay in the gay conditions, and if too feminine, he might not be believably straight in the straight conditions. While the masculinity factor is based on stereotypes and therefore would not be sufficient to identify someone as gay or straight in the real world, the masculinity (or, rather, effeminacy) stereotype of gay men is widespread (Bosson et al., 2006; Fingerhut & Peplau, 2006). Therefore, masculinity was included as a secondary means of ensuring the actor was believable as either gay or straight, in addition to asking outright if participants believed the actor’s self-disclosed sexual orientation. As with attractiveness, masculinity was measured on a scale from 1 = “feminine” to 10 = “masculine,” and the mean for masculinity was similarly moderate (M = 6.69, SD = 1.76).

In a second, much shorter, pilot study, eligibility criteria were tightened and RWA was reassessed on a sample with lower educational attainment and no previous direct contact with the LGBTQ+ group. This second pilot study took participants 4 minutes to complete. First, 628 participants were paid $0.15 to complete a 30-second, 3-item pre-screening tool. The pre-screening tool selected only those who did not have any college/university experience, had either never met a member of the LGBTQ+ group, or had made contact with a member of the LGBTQ+ group less than once a month. Eligible participants then completed only the RWA scale and the sexual prejudice scale from the original pre-test survey. Forty-one eligible participants were paid $1.00 to complete this 4-minute shortened survey. As predicted, RWA scores and prejudice scores from the less educated sample were in line with previous research (M = 89.36, SD = 42.8; and M = 2.65, SD = 1.47 respectively), which informed my decision to oversample for lower education in Study 1.
Participants

To determine the required sample size for this 2x2x2 between-subjects design, I conducted a power analysis with the program GPOWER based on estimates of effect sizes in past literature (Erdfelder, Faul, & Buchner, 1996). Two meta-analyses on the effect of intergroup contact on reduction of explicit sexual prejudice suggested an effect size of $r = 0.27$ (Bartoš et al., 2014; Pettigrew & Tropp, 2006), and a meta-analysis on extended contact effect has shown an average effect of $r = .42$ on reduction of explicit (racial) prejudice (Lemmer & Wagner, 2015). In a conservative approach, I estimated a small to medium effect size, slightly lower than effect sizes reported previously. Sample size calculations were conducted for ANCOVA because GPOWER does not provide an option to calculate for MANCOVA. To adjust for the added dependent variable, D’Amico and colleagues (2001) recommend dividing the alpha by 2 (i.e., $\alpha = .025$ rather than $\alpha = .05$) when using the ANCOVA setting in GPOWER to calculate sample size for a MANCOVA. Thus, I calculated power for an ANCOVA with 8 groups and 1 covariate, with an expected medium effect size ($Cohen\’s f = .20$), alpha set at $\alpha = .025$, and 95% power to detect an effect. The results of the power analysis suggested I would need a sample size of $n = 381$ participants (approx. 47 in each cell). Participants were screened to meet eligibility requirements (on the factors of nationality, education, sexual orientation, and contact with members of the LGBTQ+ group), and in order to achieve an acceptable final sample size, we ran 7094 individuals through an initial pre-screening survey. The MTurk website claims that researchers can access 500,000 participants through the service; however, using a capture-

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10 Cohen’s $f$ is the effect size statistic used in GPOWER for ANCOVAs. A value of $f = .20$ is a small to medium effect size, comparable to an $r$ value of .25.
recapture analysis (a statistical estimation method), Stewart et al. (2015) estimated the size of the participant pool at approximately 7,300 participants for any given study. I had, therefore, approached the limits of the participant pool.

Of the 7094 people who were pre-screened, 545 eligible participants completed the pre-test, and of those, 396 returned for the manipulation/post-manipulation survey (73% retention). Of those, 4 were deleted from the sample after contacting the research team to notify us of a technical malfunction, and we were unable to determine if the video had functioned properly for them. Another 3 were deleted due to a branching error in the sexual orientation selection criteria (these 3 responded that they “did not know” if they were LGBTQ+ and were mistakenly allowed to continue on in the study). Finally, 4 participants requested that their data be withdrawn after reading the debriefing information. All participants passed the attention checks. The resulting sample was n=386.

Forty-three of the United States were represented, and representation ranged from \( n = 1 \) to \( n = 30 \) participants per state. The only states not represented were Alaska, Mississippi, Montana, New Mexico, North Dakota, Vermont, and Wyoming; the unrepresented states are among the least populated of the United States and also represent both democratic and republican political leanings. I therefore do not believe that my findings would have been different had these 7 states been represented in my data.

Participants ranged in age from 19 to 80 with a mean age of \( M = 40, SD = 12.8 \), and as with most survey research, more women than men completed the study (57.3% and 41.7%, respectively). Low educational attainment (i.e., less than college) was oversampled on account of the findings from the pilot studies; this decision was also supported by the literature which highlights a negative relationship between education and prejudice (Hagendoorn & Nekuee,
2018; Herek & Gonzalez-Rivera, 2006; West & Cowell, 2015). My goal in oversampling low educational attainment was to ensure an appropriate representation of all types of education. This sampling strategy resulted in 57.5% with no more than a vocational/trade school diploma, and 42.5% with at least some college. The most common level of education among the lower educated participants was a high school diploma (68.4% of low-education group), and the most common level of education among the higher educated participants was an undergraduate college/university degree (51.2% of high-education group). See Table 4 for a complete breakdown of levels of education. Sexual orientation and gender identity were also selection criteria, resulting in a sample that was self-reported as 100% heterosexual and cisgender.

In terms of race/ethnicity, the majority of the sample identified as White (80%). The remaining 20% was comprised of Black or African American (8%), Asian (3.9%), Native Hawaiian or Pacific Islander (1%), multiracial (3.2%), and Other (3.1%). Participants were given the option to select any and all races that they identified as, and those who selected multiple races/ethnicities were respected for their unique identities and not re-categorized into one of the pre-determined racial/ethnic categories (Jackson, 2010). Although race can be a predictor of sexual prejudice, the relationship between these variables is inconsistent and may not be useful to predicting sexual prejudice scores over and above variables such as education and previous contact with LGBTQ+ people (Baunach et al., 2009). Overall, the lack of racial/ethnic diversity in the sample was not expected to hinder my ability to detect effects on prejudice.
Table 4

Distribution of Educational Attainment

<table>
<thead>
<tr>
<th>Educational Attainment</th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Eighth grade (or less)</td>
<td>1</td>
<td>.3</td>
</tr>
<tr>
<td>Some high school</td>
<td>5</td>
<td>1.3</td>
</tr>
<tr>
<td>High school diploma</td>
<td>151</td>
<td>39.5</td>
</tr>
<tr>
<td>Vocational/trade school</td>
<td>63</td>
<td>16.5</td>
</tr>
<tr>
<td>Some college/university</td>
<td>44</td>
<td>11.5</td>
</tr>
<tr>
<td>Undergraduate college/university degree</td>
<td>83</td>
<td>21.7</td>
</tr>
<tr>
<td>Master’s degree (graduate school)</td>
<td>27</td>
<td>7.1</td>
</tr>
<tr>
<td>Professional degree (e.g. MD, DVM, DDS, PhD)</td>
<td>8</td>
<td>2.1</td>
</tr>
</tbody>
</table>

Participants were asked to compare themselves to their fellow Americans on subjective social status (Adler & Stewart, 2007), and rated themselves an average of $M = 4.77$, $SD = 1.66$, which is slightly below centre on the 10-point scale. Participants overall had a relatively low mean score of intergroup anxiety toward gay men, $M = 2.20$, $SD = .74$, on the 5-point scale. An independent samples t-test showed that men had a discernibly higher level of baseline intergroup anxiety than women when thinking about interacting with a gay man, $M_m = 2.44$, $SD = .77$, and $M_w = 2.03$, $SD = .67$, respectively, with a difference of .40, 95% CI [.26, .55], $t(380) = 5.45$, $p < .001$.

Procedures

MTurk members completed the 3-item pre-screening survey, and eligible participants were then directed to complete the remainder of the pre-test (including demographics, RWA, and
IATH). Two-weeks after completing the pre-test, participants were invited to complete the (ostensibly unrelated) manipulation and post-manipulation survey. MTurk enables the researcher to offer subsequent tasks to qualifying participants without identifying any relation to the previous task; in this way, participants can be selected without knowing that they are participating in two parts of the same study. Participants were paid $0.15 for the <1-minute pre-screening tool, $0.50 for the 5-minute pre-test, and $4.00 for the 24-minute manipulation and post-manipulation survey.

The first step of the pre-screening/pre-test survey and the manipulation/post-manipulation survey was to acquire consent via a consent form on the first page of the survey. Participants were given an accurate description of what participation would involve, but for the manipulation/post-manipulation survey, they were given a false back story about why the research was being done. The first instance of deception for the manipulation involved telling participants that the video they were about to view was created by law students at a major American college and was meant to share information about a group that was recently engaged in legal action for human rights. Participants were (accurately) informed that, following the viewing of the video, they would be asked to answer some questions about what they saw and what they liked or did not like. After giving consent to participate, participants were taken to a new page and told that they were selected to view the video about the LGBTQ+ population from an ostensible list of other groups/videos. Following the viewing of the video, participants were directed to a series of survey items.

Deception was also utilized in the framing of the survey questions; the prejudice items were explained as helping the people behind the creation of the video (not yet revealed as researchers) understand specific details about individual participants’ perspectives that may
influence their assessment of the video. Finally, the budget cuts portion of the task deceived participants with a backstory about the law class also doing public policy advocacy and requesting the public’s opinion on funding allocations to various organizations. Following completion of the budget cuts task, participants were debriefed. In the debriefing form, participants were told exact details of the purpose of the research; that is, whether straight allies are more effective at reducing prejudice and discrimination than a gay advocate. Participants were also told at this point that their responses to a survey they completed 2 weeks prior were linked to the one they just completed so that researchers could look at mediating effects of some personal difference variables. In addition to a full disclosure of the purpose of the research, participants also received an explanation as to why they were deceived and were given the opportunity to withdraw their data from the study.
CHAPTER 4

STUDY 1: RESULTS AND DISCUSSION

Preliminary Analysis

Data Cleaning

There was no missing data to adjust for. Reverse scored items were recoded in SPSS and the data were visually inspected using graphs followed by preliminary analyses to ensure there were no obvious errors that may have occurred during export or cleaning. All scale reliabilities were adequate, and statistical assumptions were met (see Appendix F for scale reliabilities, and Appendix G for tests of statistical assumptions relevant to MANOVA).

Descriptive Statistics

The psychometrics of all scales were as expected based on past literature (see Table 5 for means and standard deviations of main variables), with the exception of RWA scores, which were only slightly lower than a typical American sample as measured in 2006 ($M=90$ then vs. $M=84.24$ now\(^\text{11}\); Altemeyer, 2006). Prejudice scores in my sample were slightly positively skewed, as is expected when measuring explicit prejudice, potentially due to socially desirable responding (Brown, 1995; Er-rafiy et al., 2010). Due to the skewed prejudice scores, I was attentive to the possibility that a floor effect could impact results. All variables were correlated with each other with no concerns about multicollinearity (see Table 6 for correlations).

\(^{11}\) For context, the theoretical minimum score on the RWA is 20, and the maximum is 80.
Table 5

Means and Standard Deviations of Main Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>84.24</td>
<td>36.96</td>
</tr>
<tr>
<td>Sexual Prejudice</td>
<td>2.29</td>
<td>1.14</td>
</tr>
<tr>
<td>% Cut from HRC</td>
<td>32.6%</td>
<td>31.3%</td>
</tr>
</tbody>
</table>

Note. % Cut from HRC is the mean percent cut from the LGBTQ+ rights organization when instructed to cut 20% from 5 organizations overall.

Table 6

Correlations

<table>
<thead>
<tr>
<th>RWA</th>
<th>Sexual Prejudice</th>
<th>% Cut from HRC</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>1</td>
<td>.461**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>.245**</td>
</tr>
<tr>
<td>Sexual Prejudice</td>
<td>.461**</td>
<td>1</td>
</tr>
<tr>
<td>% Cut from HRC</td>
<td>.245**</td>
<td>.570**</td>
</tr>
<tr>
<td></td>
<td></td>
<td>1</td>
</tr>
</tbody>
</table>

Note. **Correlation is significant at the .001 level (2-tailed)

The budget cuts task was different from the other measures in that the values are reported in dollar amount as opposed to a score on a Likert type scale. In the budget cuts task, only one organization (the Human Rights Campaign; HRC) was of importance. However, I compared the effects of all organizations to ensure that the cuts made to the HRC appeared meaningful in relation to the others; ideally, the manipulated conditions would only have an effect on HRC to show that the other organizations were truly irrelevant from the model. A 2 (gender: male,
female) x 2 (target’s sexual orientation: gay, straight) x 2 (target’s support for LGBTQ+ issues: ally/advocate, non-ally/non-advocate) MANOVA was conducted with percent cuts to the 5 social organizations as the dependent variables. I found an effect of condition on the percent cuts to the LGBTQ+ organization (see Appendix H for details and results of this analysis), and none of the decoy organizations were affected by condition. As such, HRC can be assumed to be a reasonable measure of subtle discrimination toward the LGBTQ+ group.

Table 7

Comparison of Mean Budget Cuts by Organization

<table>
<thead>
<tr>
<th>Organization</th>
<th>Mean Percent Cuts</th>
<th>SD</th>
<th>Starting Budget (in Million $)</th>
<th>Mean Cut (in Million $)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Human Rights Campaign (Target)</td>
<td>32.6%</td>
<td>31.3%</td>
<td>46</td>
<td>15.00</td>
</tr>
<tr>
<td>Great Lakes Restoration Initiative</td>
<td>26%</td>
<td>26.2%</td>
<td>40</td>
<td>10.42</td>
</tr>
<tr>
<td>African Development Foundation</td>
<td>42.7%</td>
<td>35.2%</td>
<td>26</td>
<td>11.10</td>
</tr>
<tr>
<td>National Wildlife Refuge Fund</td>
<td>-9.8%</td>
<td>65.4%</td>
<td>13</td>
<td>-1.27</td>
</tr>
<tr>
<td>U.S. Interagency Council on Homelessness</td>
<td>-231.6%</td>
<td>347.4%</td>
<td>4</td>
<td>-9.27</td>
</tr>
</tbody>
</table>

Note. A negative value signifies money added to the organization’s fund.

Table 7 shows the mean percent cut from each organization, as well as the amount that was cut in millions of dollars. Percentages are calculated for each organization separately and therefore do not sum to 100; e.g., USICH had a mean of 231.6% added to its budget - the size of this percentage likely reflects the smaller starting budget of this organization, and not that the organization itself is particularly meaningful to participants. The fact that participants added to some budgets rather than reducing budgets across all organizations is a consequence of this task.
operating as a zero-sum game, where adding funds to one organization requires taking funds from another so that the overall funding allocation could be reduced by 20%; however, the only organizational cut used in hypothesis testing was the percent cut to the HRC – an examination of organizational cuts in relation to each other was not part of the study design. Differences across organization were shown only to provide a clearer picture of how the tool functioned, not to imply that the decoy organizations were key indices.

**Building the Model**

Based on the occurrence of intergroup anxiety as a mediator or moderator of extended contact in past literature (Eller et al., 2011a; Gómez et al., 2011), I tested the importance of a baseline measure of intergroup anxiety toward gay men to determine if it should be included in the final MANCOVA model. However, despite a gender difference in intergroup anxiety (see results of analysis on page 56), intergroup anxiety did not moderate the effect of either experimental condition on sexual prejudice or subtle discrimination (see Appendix I for analysis details and results). Thus, intergroup anxiety was not included in further analysis.

The addition of right wing authoritarianism as a covariate in the model was both necessary and potentially problematic. I included RWA as a covariate because it is a known predictor of sexual prejudice, but not one that would help me to answer my research question; thus, its covariate status allowed me to adjust for its effects. However, past research has shown that RWA can moderate the effect of extended contact on prejudice; one study in particular found that extended contact was more effective among those high in RWA than those low in RWA (Dhont & Van Hiel, 2011). Therefore, I also needed to be cognizant that RWA could have interacted with the independent variables, thus violating the homogeneity of regression slopes assumption of ANCOVA (Herek & McLemore, 2013; Vezzali et al., 2014). Because of the risk
that RWA moderated or interacted with my independent variables, I conducted a test of moderation (see Appendix J) as well as a test of the homogeneity of regression slopes to ensure that it was, in fact, independent from the IVs (see Appendix G, Tables G7 and G8). Results of these tests confirmed that it was independent.

**Main Analysis**

A multivariate analysis of covariance (MANCOVA) was conducted on the overall model to test for effects of all independent variables (i.e., sexual orientation of the target, target’s explicit support for LGBTQ+ issues, and gender of the participant) on both dependent variables (i.e., sexual prejudice and subtle discrimination). Before testing the model in a MANCOVA, several tests were conducted to determine whether there were any violations of assumptions associated with MANCOVA. To begin, a correlation analysis determined that the two dependent variables were appropriately correlated with each other at a moderate level (see Table 6; Meyers, Gampst, & Guarino, 2006). Also, Box’s test showed that the covariance matrices were equal, with a Box’s M value of 35.26, and a non-significant p value of .031 (Box’s M is significant at p < .005; Huberty & Petoskey, 2000). Finally, tests were conducted to ensure that the covariate did not violate the assumption of homogeneity of regression slopes (see Appendix G, Tables G7 and G8). No violation was observed for either dependent variable in the model. Thus, I am confident that the relevant statistical assumptions of the MANCOVA test were satisfied.

At a multivariate level, the MANCOVA showed statistically discernible effects of target’s sexual orientation, Pillai’s Trace = .03, $F(2, 371) = 6.62$, $p < .05$, $\eta^2 = .034$, and target’s

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12 The use of the term *statistically discernible* is recommended by Benning (2019) at a time in statistical science when confidence intervals and Bayesian statistics are replacing the significance testing framework. This
support for LGBTQ+ issues, Pillai’s Trace = .018, $F(2, 371) = 4.18, p < .05, \eta^2 = .018$, indicating a difference in the level of prejudice and/or discrimination between gay and straight targets, and ally/advocate and non-ally/non-advocate targets. The MANCOVA also found a statistically discernible interaction between target’s sexual orientation and participant gender, Pillai’s Trace = .02, $F(2, 371) = 4.18, p < .05, \eta^2 = .022$, indicating that the effect of one of these independent variables varied for each level of the other independent variable.

Prior to conducting follow-up analyses of covariance (ANCOVA) to explore the effects of the model on each dependent variable, I used Levene’s test of equality of error variances to ensure that the homogeneity of variance assumption was met for both sexual prejudice ($F_{7,373} = 1.27, p = .26$) and subtle discrimination ($F_{7,373} = 1.22, p = .29$). Follow-up univariate tests of between-subjects effects showed a main effect of target’s sexual orientation on subtle discrimination ($F_{1,372} = 9.34, p < .05, \eta^2 = .024$), but not on sexual prejudice ($F_{1,372} = .00, p = .99, \eta^2 = .000$). A look at the estimated marginal means confirmed H1: participants cut more from the LGBTQ+ organization’s budget when they viewed a straight target, M=.37, SD=.31, 95% CI [.32, .41], versus a gay target, M=.28, SD=.31, 95% CI [.23, .32], suggesting less subtle discrimination following the manipulation with a gay target than with a straight target (see Figure 1, and Table 8). This finding supports intergroup contact theory, rather than extended contact hypothesis, in that direct contact with a member of the out-group was associated with less discrimination than extended contact via a member of the in-group.

dissertation, as well as my training as a graduate student, occurred prior to (and during) this shift in statistical testing and interpretation; therefore, I include both probability testing and confidence intervals in my analyses.

13 When interpreting subtle discrimination, note that means are of the percent of funding cut from the LGBTQ+ organization’s budget; therefore, higher means translate to more subtle discrimination.
Figure 1. Comparison of budget cuts for gay target, $M=.28$, $SD=.31$, 95% CI [.23, .32], versus straight target, $M=.37$, $SD=.31$, 95% CI [.32, .41].

Table 8

*Subtle Discrimination (Budget Cuts) Scores by Condition*

<table>
<thead>
<tr>
<th>Condition</th>
<th>F</th>
<th>p</th>
<th>Partial $\eta^2$</th>
<th>Level</th>
<th>Estimated Means</th>
<th>95% CI [Upper bound, Lower bound]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Orientation</td>
<td>9.34</td>
<td>.002</td>
<td>.024</td>
<td>Gay</td>
<td>.28</td>
<td>[.23, .32]</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Straight</td>
<td>.37</td>
<td>[.32, .41]</td>
</tr>
<tr>
<td>Support</td>
<td>4.20</td>
<td>.041</td>
<td>.011</td>
<td>Ally/Advocate</td>
<td>.36</td>
<td>[.31, .39]</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>Non-Ally/Non-Advocate</td>
<td>.29</td>
<td>[.25, .34]</td>
</tr>
</tbody>
</table>

*Note.* Orientation: target’s sexual orientation; Support: target’s support for LGBTQ+ issues.
Despite not making a formal hypothesis regarding main effect of target's support for LGBTQ+ issues (due to the potential confounding effect of target's sexual orientation), the model did reveal a surprising finding regarding target's support for LGBTQ+ issues: participants discriminated more when they viewed a target who was an ally/advocate, $M=.36$, $SD=.022$, 95% CI [.31, .39] than when they viewed a target who did not explicitly support LGBTQ+ issues, $M=.29$, $SD=.022$, 95% CI [.25, .34], $F_{1,372} = 4.20$, $p < .05$, $\eta_p^2 = .011$ (see Figure 2, and Table 8). Although this finding could suggest that an ally may not be the most ideal messenger for an anti-discrimination message, there is a chance that the gay advocate condition was behind the higher discrimination scores in the ally/advocate category, and that the straight ally target might still produce the lowest discrimination scores compared to the other three combinations of sexual orientation and support. To test this prediction, I explored whether there was an interaction between sexual orientation of target and target's support for LGBTQ+ issues (H2). However, an interaction was not found, $F_{1,372} = 2.35$, $p = .12$, $\eta_p^2 = .006$.

![Figure 2](image-url). Figure 2. Comparison of budget cuts for ally/advocate, $M=.36$, $SD=.022$, 95% CI [.31, .39], versus non-ally/non-advocate, $M=.29$, $SD=.022$, 95% CI [.25, .34].
Consistent with findings from the plethora of sexual prejudice research, I predicted multiple effects of gender of participant. I hypothesized a main effect of gender of participant, such that men will have higher prejudice and discrimination scores than women (H3); surprisingly, no main effect of gender was found for sexual prejudice, $F_{1,372} = .015, p = .90, \eta^2_p = .000$; or subtle discrimination, $F_{1,372} = .003, p = .95, \eta^2_p = .000, M = .57, 95\% \text{ CI [.53, .63]}$. However, I also hypothesized a two-way interaction of participant gender and sexual orientation of target, such that men would be more prejudiced and exhibit more subtle discrimination than women against the gay target (H4), and that men in the gay target groups would be more prejudiced and exhibit more subtle discrimination than men in the straight target groups (H5). An interaction between participant gender and target’s sexual orientation was confirmed, $F_{1,372} = 4.14, p < .05, \eta^2_p = .011$, but only on the sexual prejudice outcome (not on subtle discrimination, $F_{1,372} = .375, p = .54, \eta^2_p = .001$). See Figure 3 for an illustration of the interaction between gender and sexual orientation on sexual prejudice.

![Image: Illustration of interaction between gender and sexual orientation on sexual prejudice]
Figure 3. Interaction of participant gender and target’s sexual orientation on sexual prejudice scores.

I conducted post-hoc tests of simple effects (i.e., pairwise comparisons) to determine which group differences could explain the interaction. Results showed that no group was discernibly different from another at the alpha = .05 level. Level of prejudice among men was not significantly different between gay and straight conditions, $F_{1, 376} = 1.72, p = 1.89, M = .21, 95\% \text{ CI } [-.10, .52]$; similarly, level of prejudice among women were not significantly different between gay and straight conditions, $F_{1, 376} = 2.97, p = .08, M = -.23, 95\% \text{ CI } [-.50, .03]$. Conversely, level of prejudice within the gay target conditions was not significantly different between men and women, $F_{1, 376} = 2.36, p = .12, M = .23, 95\% \text{ CI } [-.06, .52]$, nor was level of prejudice within the straight target condition different between men and women, $F_{1, 376} = 2.13, p = .14, M = -.21, 95\% \text{ CI } [-.50, .07]$. The results of these simple effects analyses indicate that, despite the significant interaction in the ANCOVA, the mean differences across the four groups were not sufficient to provide a clear explanation for the interaction.

However, despite this outcome, one particular mean difference is worthy of comment. Women in the straight target condition had higher prejudice than women in the gay target condition ($M = 2.41$ vs. $M = 2.17$, respectively; see Table 9 for details of the simple effects analysis), suggesting that women, in particular, may be more positively impacted by a direct contact versus an extended contact when it comes to reducing sexual prejudice. Overall though, these analyses indicate that hypotheses 4 and 5, regarding higher prejudice and discrimination among male participants, were not supported.
Table 9

*Pairwise Comparisons of Participant Gender and Target Sexual Orientation on Sexual Prejudice*

<table>
<thead>
<tr>
<th>Participant Gender</th>
<th>EM Mean of Gay Target Condition</th>
<th>EM Mean of Straight Target Condition</th>
<th>Mean Difference</th>
<th>95% CI [Lower bound, Lower bound]</th>
<th>F</th>
<th>df</th>
<th>p-value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Female</td>
<td>2.17</td>
<td>2.41</td>
<td>.236</td>
<td>[-.033, .505]</td>
<td>2.98</td>
<td>1, 376</td>
<td>.085</td>
</tr>
<tr>
<td>Male</td>
<td>2.40</td>
<td>2.19</td>
<td>.210</td>
<td>[-.104, .525]</td>
<td>1.73</td>
<td>1, 376</td>
<td>.189</td>
</tr>
</tbody>
</table>

**Discussion**

In this study, I set out to test whether extended contact could be a suitable alternative to direct contact when applied to the reduction of sexual prejudice and discrimination. Although some past research comparing direct with extended contact have found direct intergroup contact to be superior at reducing prejudice (Paolini et al., 2007; Vezzali et al., 2014), the point of my research question, and the research argument proposed in much of the past literature on extended contact, is that there are certain conditions under which extended contact should have advantages over and above direct contact (e.g., high baseline prejudice, low direct contact opportunity, high intergroup anxiety; Turner et al., 2008; Vezzali et al., 2014; Vonofakou et al., 2008).

To support my prediction, I expected the data to show an interaction between the target's sexual orientation and the target's explicit support for LGBTQ+ issues, such that the straight ally condition would result in lower prejudice and discrimination than either of the gay target conditions (advocate or non-advocate), but the straight non-ally condition would result in more
prejudice and discrimination than either of the gay target conditions. However, the data did not support my hypothesis that target’s support for LGBTQ+ issues would interact with target’s sexual orientation to make straight allies effective at reducing prejudice and discrimination; instead, this study provided support for direct intergroup contact in that watching a video with a gay target was more effective than a straight target at reducing subtle discrimination, regardless of target’s support for LGBTQ+ issues. Furthermore, the data showed that when a target, either gay or straight, showed explicit support for LGBTQ+ issues, subtle discrimination actually increased. That is, my original thesis statement on the unique efficacy of allyship appears to be more complex than this study was able to investigate.

While the previous two main effects were found on subtle discrimination, sexual prejudice as an outcome garnered slightly different results with an interaction between participant gender and target’s sexual orientation; however, despite a statistically discernible interaction in the MANCOVA (and follow-up ANCOVA), tests of simple effects failed to show discernible differences across any of the four means considered in this analysis. The post-hoc test of simple effects showed a possible trend whereby women appeared more prejudiced in the straight target condition than in the gay target condition (M = .23, 95% CI [-.033, .505]). Once again, this trend would only serve to support intergroup contact over extended contact, this time in the context of sexual prejudice rather than subtle discrimination. It appears, based on the data from this study, that direct contact with an LGBTQ+ person is the best way to reduce subtle discrimination (and potentially sexual prejudice), and that allyship may not perform in the way I expected when applied to a prejudice reduction intervention.
The Issue of Anti-Advocacy

It is surprising that straight allies and gay advocates alike seemed to incite more discrimination than individuals who made no such pledge to personally support LGBTQ+ people (see Table 8 for means). Although no research has yet investigated the efficacy of straight allies in reducing sexual prejudice and discrimination, my hypothesis on this idea was informed by research on male allies against sexism. Specifically, men who are allies against sexism tend to be more effective at reducing sexism than their female counterparts – they are seen as more reasonable and persuasive (Cihangir et al., 2014; Drury & Kaiser, 2014). However, one could consider the apparent anti-advocacy effect found in the present study not as a reaction to allies, specifically, but in a broader sense, as a reaction to feeling pushed toward a progressive cultural shift. This phenomenon is known as cultural backlash, which is defined as a strengthening of opposing viewpoints as a reaction to progress in the realm of equal rights and anti-prejudice (Gilmour, 2018; Inglehart & Norris, 2016). In the present study, the greater amount of subtle discrimination in the conditions with explicit support for LGBTQ+ issues could be seen as a cultural backlash effect against seeing LGBTQ+ rights advocated for.

Another theory relevant to this explanation of higher discrimination in the ally/advocate condition is that of reactance. The theory of reactance is based on a feeling of threatened personal freedom, and a motivation to act to restore that freedom (Miron & Brehm, 2006). Reactance can be related to anti-prejudice and anti-discrimination work with the notion of threat to religious freedoms. Specifically, some members of religious groups in the United States have recently exhibited reactance by refusing to serve LGBTQ+ customers in their religious and non-religious establishments, citing that anti-discrimination legislation threatens their religious freedoms (Thoreson, 2018). Thus, the finding of increased discrimination in the ally/advocate
conditions could be due to participants feeling that the target in these conditions is threatening their freedom to discriminate by admitting his own efforts to end this discrimination.

Another example of reactance affecting anti-prejudice work comes from a study that investigated the concept of anti-prejudice interventions that increase prejudice (in the context of racism; Legault et al., 2011). This research found that a controlling intervention – where participants were urged to comply with social norms of non-prejudice – actually increased prejudice in comparison to the control condition. An autonomy intervention, which encouraged participants’ inner motivation for prejudice reduction, was more effective at reducing prejudice than the control. This study attributed the effects to reactance toward being urged to change, and suggests that perhaps allies and advocates are too controlling, while attempting to place external motivations for change onto their audience.

Although only preliminary (as it is so far not replicated), this finding of increased discrimination against an ally/advocate has implications for those attempting to reduce prejudice and discrimination in both research and applied realms, but particularly for those doing applied work. That is, if disclosing that you are working to reduce prejudice and discrimination against LGBTQ+ people can potentially turn an audience off of hearing or accepting your message, the resulting suggestion might be to engage in anti-prejudice work in a covert or deceptive manner as in experimental research in a laboratory setting. This suggestion may not be practical or reasonable to applied anti-prejudice work, because this work is fundamentally carried out by allies and advocates; consider again the definition of ally – it is someone who works to end prejudice in others (Brooks & Edwards, 2009; Herek & McLemore, 2013; Ji, 2004). Therefore, this finding could suggest a fatal flaw in initiatives that advocate for LGBTQ+ people – that is, the overt advocating itself. Of course, with such a preliminary finding and a lack of explanatory
data, the relationship between explicit support for LGBTQ+ issues and increased subtle discrimination needs to be investigated further to determine if the effect holds in replications, for other out-groups, and with other audiences.

Considerations for Subsequent Studies

The present sample was selected for their lack of frequent previous contact with members of the LGBTQ+ group. I chose to control for previous contact in this manner because past research has suggested that extended contact interventions do not always have measurable effects on those with previous direct contact (Cameron et al., 2011; Christ et al., 2010; Dhont & Van Hiel, 2011; Eller et al., 2012). I also speculated in designing the study that those with LGBTQ+ friends would have a lower baseline of sexual prejudice and therefore might be more susceptible to floor effects; in other words, the manipulation would be unlikely to lower prejudice among those with already low prejudice. Alternatively, I could have included all degrees of previous direct contact in the sample and attempted to account for its effects in the model.

Also relevant to issues of control, I chose not to include a traditional control condition wherein participants could be randomly assigned to view a video completely unrelated to sexual orientation or prejudice and discrimination. Instead, I opted to compare the straight ally condition to the gay advocate condition while effectively treating the gay non-advocate and straight non-ally conditions as baseline measures. I chose this simpler 2x2x2 model (adding a control condition would have made the model 2x2x3) so that I could focus on testing more variables within the sample size limits I knew I was facing (with strict criteria for participating, I was pushing the capacity of MTurk’s available sampling pool for any given study; Stewart et al., 2015). However, in order to determine whether or not the manipulation was having a positive impact overall (beyond simply being able to state whether extended contact had more or less of
an impact than direct contact), I decided to incorporate a baseline measure of sexual prejudice into the Study 2 design.

Another area for adjustment in Study 2 is to strengthen the manipulations in an attempt to increase salience for participants. One seemingly obvious way to strengthen the manipulations is to conduct a face-to-face intervention where participants can interact with the target. However, despite the literature’s historical focus on in-person interventions, the benefits of computer-mediated contact are fast being supported by research (Dovidio et al., 2017; White et al., 2014). Furthermore, computer-mediated contact can be facilitated through visual media platforms such as YouTube, and from there shared further through networking or recruitment platforms such as MTurk (in the present case) to targeted audiences (Miller, 2017; Snell, 2014). Thus, computer-mediated contact can be considered a more practical intervention approach if aiming to have a broader impact by reaching masses (Paluck & Green, 2009; Singhal et al., 2004). Also, given the required sample size for the model, face-to-face contact between participant and target was not logistically feasible either in Study 1 or in subsequent studies. Another way to strengthen the manipulation, while maintaining the same study design, is to make the manipulation script more personal and passionately worded. Therefore, Study 2 used stronger wording in the self-disclosure script in the manipulation of explicit support for LGBTQ+ issues in an effort to seek clarity on the main effect of target’s support for LGBTQ+ issues.

Finally of note is the significant interaction of participant gender with target’s sexual orientation. I chose to speculate on the mean differences, regardless of the lack of statistical evidence that they were indeed meaningfully different. One benefit of doing so was to inform my analysis of Study 2 – and future research - by highlighting the different effect that participant gender may have on the manipulated variables. If women’s prejudice scores are closer to being
measurably different than men’s prejudice scores, subsequent analyses should not assume that men are the most likely gender to produce effects – this focus on men’s attitudes is ubiquitous in the sexual prejudice literature (i.e., men are reported as more prejudiced than women, and more prejudiced against gay men than against lesbian women; Ahrold & Meston 2010; Norton & Herek, 2013; Rampullo et al., 2013). This finding suggests a change in perspective for all those conducting prejudice research; the assumption that men produce the most interesting effects on prejudice may be outdated.

The results of Study 1 left me unable to answer my research question with any certainty, although the findings had promise as they hinted at interesting new developments. The tendency for participants to exhibit less discrimination in the gay target conditions was in line with intergroup contact literature; but, my primary hypothesis, whereby I predicted an interaction of allyship and sexual orientation, which was designed to highlight an exception to the superiority of direct contact, was not supported: straight allies were not more effective at reducing prejudice and discrimination than the other conditions. The finding that allyship/advocacy increased discrimination rather than decreased it was contradictory to relevant literature within the purview of this dissertation (e.g., Cihangir et al., 2014), but the introduction of concepts such as cultural backlash and psychological reactance may provide speculation as to why this effect was found. Finally, although gender interacted in a predictable way with target’s sexual orientation, the lack of a main effect of gender on sexual prejudice left me perplexed – gender is one of the most reliable predictors of sexual prejudice in the literature (Ahrold & Meston 2010; Norton & Herek, 2013; Rampullo et al., 2013). There was also inconsistency in the criterions, with different independent variables showing effects on either prejudice or discrimination. With these
unanswered questions and unresolved conclusions, a second study was needed to attempt to resolve the questions that arose from the data and provide a stronger test of my hypotheses.
CHAPTER 5

STUDY 2: METHODOLOGY

Design and Hypotheses

Study 2 utilized the same design as Study 1, wherein participants viewed an educational video under the guise of reviewing a class assignment. In an adjustment made from Study 1, however, participants were randomly assigned to 1 of 6 (not 1 of 4) versions of the video. The actor/target self-disclosed differently across the 6 videos to test the effects of identity of a person delivering anti-prejudice messaging on explicit prejudice and subtle discrimination. This study used a 2 (gender of participant: male, female) x 2 (sexual orientation of target: straight, gay) x 3 (explicit support for LGBTQ+ issues: none, moderate, strong) between-subjects experimental design.

In Study 2, the only selection criteria for recruitment was that participants were heterosexual and cisgender; participants were not screened for education or previous intergroup contact as they had been in Study 1. I retained the main hypotheses from Study 1 to determine, among other things, if the findings from the previous study were generalizable to a population with a range of previous contact experience. Although my main research question remains that extended contact (via a straight target) could be more effective at reducing prejudice and discrimination than direct contact (via a gay target) if that straight target is an ally for LGBTQ+ rights, the literature suggests that at its most basic level, disregarding mediating conditions, intergroup contact is more effective than extended contact (Broockman & Kalla, 2016; Paolini et al., 2007). Therefore, I hypothesized a main effect of sexual orientation of target, such that the gay target would produce lower prejudice and discrimination scores than the straight target across all levels of support for LGBTQ+ issues (H1). It is through a hypothesized two-way
interaction that I aimed to address my research question: I hypothesized a two-way interaction of support for LGBTQ+ issues and sexual orientation of target, such that stronger support is associated with lower prejudice and discrimination when the target is straight (ally), but not when the target is gay (advocate; H2). I also expected to find a main effect of participant gender, such that men would have higher prejudice and discrimination scores than women (H3). Despite this main effect not being supported by Study 1 data, the empirical support for gender effects is strong in the established literature, and suggests that a different sample may yield the expected results.

**Methodology**

**Materials**

**Pre-screening survey.** In addition to basic demographic questions, the pre-screening survey for Study 2 included the same measure of sexual orientation / gender identity that was used in Study 1: “To what extent is this true of you: I am a member of the LGBTQ+ community” (for a summary comparison of the materials in Study 1 versus Study 2, see Appendix K). Basic demographic questions were included in the pre-screening survey but were not used to screen participants; rather, the placement of these questions functioned to obscure the topic of the study (see Appendix L for pre-screening tool). Had the pre-screening survey only included a question about sexual orientation/gender identity, those not interested in research based on sexual orientation may have opted to not proceed with the rest of the study. Despite the placement of the demographic questions in the pre-screening survey, the only eligibility requirement was that participants not be members of the LGBTQ+ community.

**Pre-test.** The Study 2 pre-test expanded on the contact questions that had been limited to screening devices in Study 1 (see Appendix M for complete Study 2 pre-test). Participants were
asked detailed questions about previous direct and extended contact with members of the LGBTQ+ group, and a measure of closeness to existing direct and extended contacts was introduced. Five questions, borrowed directly from Baunach and colleagues (2009), assessed previous direct contact with a member of the LGBTQ+ group. These questions encompassed type of relationship to the contact (e.g., parent, sibling, cousin, friend), how many direct contacts one has in total, and the frequency of interaction with LGBTQ+ people in general (see Table 13 for specific items). Four questions measured quantity of extended contacts in a measure adapted from Turner et al (2008). Questions included, "how many of your straight friends have friends who are LGBT?" and "How many members of your family (including parents, brothers and sisters, cousins, etc.) have friends who are LGBT?" Responses were recorded on a scale from 1 = “none” to 10 = “over ten.” Although I did not expect to control for previous extended contact due to its subjective, extrapolative nature (e.g., asking someone to recall how many out-group friends one believes that their friend has), I aimed to measure direct and extended contacts using multiple questions used previously in the contact literature to allow me to explore the effect of these measures in supplementary analyses (Baunach et al., 2009; Capozza et al., 2014; Gómez et al., 2011).

Closeness with contacts was measured using the Inclusion of Other in the Self Scale, which is a pictorial measure showing 7 Venn diagrams with increasing overlap from the first diagram (no overlap) to the last (almost full overlap; IOS; Aron et al., 1992). Circles in the diagram are labeled "self" and "other," and participants are asked to select the diagram that best represents their relationship with a target. I asked participants to respond to the IOS scale while thinking about the direct contact they were closest with, and then asked the participants to identify their relationship to that contact (e.g., brother, best friend, colleague). When closeness
with the out-group is measured in the contact literature, the IOS scale is used almost exclusively as it is a concise and psychometrically sound way of measuring closeness, or degree of interconnectedness, between groups (see Turner et al., 2010 for a review). Test-re-test reliability in published psychometric tests was \( r = .83 \), and it has been found to be significantly correlated with other closeness scales such as the relationship closeness inventory (\( r = .22 \)), the subjective closeness index (\( r = .34 \)), and the positive emotions about other scale (\( r = .45 \); Aron et al., 1992). In addition, the IOS scale does not seem to be affected by social desirability (correlated -.04 with self-deception scale and .05 with impression management scale; Aron et al., 1992).

Due to the importance that past research has placed on religion as a covariate to sexual prejudice, I included a 5-item religion measure in the pre-test (Cluse-Tolar et al., 2004; Finlay & Walther, 2003). The Duke University Religion Index (DUREL) includes three subscales: subscale 1 measures frequency of attendance at religious services, and subscale 2 measures frequency of private religious activities (i.e., How often do you spend time in private religious activities, such as prayer, meditation, or Bible study?). Subscale 3 includes the final 3 items of the scale and measures intrinsic religious orientation (e.g., My religious beliefs are what really lie behind my whole approach to life; Koenig & Büsßing, 2010). With my sample, the 3 items of subscale 3 performed well in a test of internal reliability (\( \alpha = .94 \)).

The pre-test also included Alemeyer's (2006) Right Wing Authoritarianism Scale (RWA), an adapted version of Muñoz’s (2003) Intergroup Anxiety Toward Homosexuals Scale (IATH; as used in Study 1), and a baseline measure of sexual prejudice which was an exact copy of the Sexual Prejudice Scale used as a dependent variable in the post-test (Chonody, 2013).

**Manipulation.** The manipulation consisted of the same series of videos used in Study 1 (see Appendix C for manipulation script), but with the addition of two versions containing more
strongly worded self-disclosures (see Table 10 for strong self-disclosure scripts; also see Table 3 on page 45-55 for a review of the four original self-disclosure scripts). All six versions were recorded at the same time with the same actor, and as with the original four versions, the two more strongly worded videos differed only by a few brief sentences during the self-disclosure portion of the script. These six videos make up the three levels of the explicit support for LGBTQ+ issues variable (see Table 11).

Table 10

*Stronger versions of self-disclosure script*

<table>
<thead>
<tr>
<th>Version</th>
<th>Self-Disclosure Script</th>
</tr>
</thead>
<tbody>
<tr>
<td>Version 5</td>
<td>“Hi, I’m Michael, I’m from Anaheim, California, I’m a law student, and I’m making this video for an assignment for a class on anti-discrimination and labor law. Although I’m straight, my best friend from high school is gay, and I’ve seen him experience a lot of homophobic bullying, harassment, and discrimination. I feel strongly that people should not be bullied, harassed, or discriminated against because of their sexual orientation, and I want to do whatever I can to help the victims of this kind of discrimination. So, I plan to work as a human rights lawyer to fight for rights for the LGBT community. I’m an ally for gay rights, trying to make positive change.”</td>
</tr>
<tr>
<td>Version 6</td>
<td>“Hi, I’m Michael, I’m from Anaheim, California, I’m a law student, and I’m making this video for an assignment for a class on anti-discrimination and labor law. As a gay man, I’ve experienced homophobic bullying, harassment, and discrimination. I feel strongly that people should not be bullied, harassed, or discriminated against because of their sexual orientation, and I want to do whatever I can to help other victims of this kind of discrimination. So, I plan to work as a human rights lawyer to fight for the rights for my fellow LGBT community.”</td>
</tr>
</tbody>
</table>
Table 11

*Levels of explicit support for LGBTQ+ issues*

<table>
<thead>
<tr>
<th>Video version</th>
<th>Level of Explicit Support for LGBTQ+ Issues Variable</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Straight non-advocate</td>
<td>None</td>
</tr>
<tr>
<td>2. Gay non-advocate</td>
<td></td>
</tr>
<tr>
<td>3. Straight ally</td>
<td>Moderate</td>
</tr>
<tr>
<td>4. Gay advocate</td>
<td></td>
</tr>
<tr>
<td>5. Strong straight ally</td>
<td>Strong</td>
</tr>
<tr>
<td>6. Strong gay advocate</td>
<td></td>
</tr>
</tbody>
</table>

**Post-manipulation survey.** The post-manipulation survey was unchanged from Study 1 (see Appendix D). It included the same open-ended questions for what participants liked and did not like about the video, as well as the Reysen (2005) likeability scale. It also included Chonody's (2013) explicit sexual prejudice scale, and the budget cuts task to measure subtle discrimination (Haddock et al., 1993).

**Participants.** A power analysis was conducted using GPOWER to determine how many participants were needed to capture reliable effects of the manipulation within this 2x2x3 design. The closest estimate that could be calculated for power in a MANCOVA was derived from a power analysis for an ANCOVA, adjusting the alpha for the addition of a dependent variable (D’Amico et al., 2001). I reduced the estimated effect size from what I expected in Study 1 due to the lower effect sizes that were found. Accordingly, values in GPOWER were set at 12 groups.
and 1 covariate, with an expected small to medium effect size ($f = 0.14$), alpha set at $\alpha = 0.025$, and 95% power to detect an effect. The results of the power analysis suggested I would need a sample size of $n = 774$ participants (approx. 65 in each cell). A total of 1558 individuals completed the pre-screening survey; 75 of those were excluded because their IP addresses were flagged as fraudulent, 15 were excluded because they were outside of the US, 11 were excluded for failing more than one attention check, and 335 were excluded because they identified as a member of the LGBTQ+ population. Of the remaining 1122 eligible individuals who completed the screener, 813 eligible respondents completed the manipulation and post-manipulation survey, resulting in a 73% retention rate. From that dataset, 4 participants withdrew from the study after debriefing, and their data was deleted. As a last measure of conscientious responding, participants were asked in the debriefing form to select one of two statements about their participation: "I was paying attention to the survey items and my responses were made thoughtfully;" or "You probably don’t want to use my responses / I wasn’t paying attention to the survey items" - 3 participants selected the latter and were also deleted from the dataset. The final sample was $N = 806$ with even distribution across conditions.

Forty-seven U.S. states were represented in the sample, the mean number of participants from each state was $M=24.71$, and representation ranged from $n = 1$ to $n = 49$ per state. The only states not represented were Rhode Island, Vermont, and Wyoming. Participants ranged in age from 18 to 77 with a mean age of $M = 38.24$, $SD = 12.07$, and more women than men participated in the study (57.1% and 42.9%, respectively). The majority of participants had

---

14 Small to medium effect sizes in Study 1 (Partial eta squared effects sizes from $\eta^2 = 0.011$ to $0.024$) were translated into a small to medium Cohen’s $f$ for the purpose of matching GPOWER’s input field.
college or university degrees (see Table 12). Oversampling for low educational attainment was not conducted due to limited recruitment resources as well as a limited participant pool, as was discovered in recruiting for Study 1.

Table 12

*Educational Attainment Distribution*

<table>
<thead>
<tr>
<th>Educational Attainment</th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Some high school</td>
<td>5</td>
<td>0.6</td>
</tr>
<tr>
<td>High school diploma</td>
<td>79</td>
<td>9.8</td>
</tr>
<tr>
<td>Vocation/trade school</td>
<td>27</td>
<td>3.3</td>
</tr>
<tr>
<td>Some college/university</td>
<td>232</td>
<td>28.8</td>
</tr>
<tr>
<td>Undergraduate college/university degree</td>
<td>348</td>
<td>43.2</td>
</tr>
<tr>
<td>Master's degree (graduate school)</td>
<td>82</td>
<td>10.2</td>
</tr>
<tr>
<td>Professional degree (e.g., MD, DVM, DDS, PhD)</td>
<td>32</td>
<td>4.0</td>
</tr>
</tbody>
</table>

Sexual orientation and gender identity were selection criteria, resulting in a sample that was 100% heterosexual and cisgender. The sample was majority white (80%), with very few other ethnicities/races reported. Asian participants made up 7.8% of the sample, followed by Black or African American participants at 5.8%. The remaining 5% were made up of $n = 3$ American Indian or Alaska Native, $n = 2$ Native Hawaiian or Pacific Islander, and various self-reported mixed races. Participants were asked to compare themselves to their fellow Americans on subjective social status (Adler & Stewart, 2007), and rated themselves an average of $M =$
5.21, $SD = 1.71$, on a 10-point scale; participants considered themselves to be of average social status when compared to others.

Participants had a relatively low mean score of intergroup anxiety toward gay men, $M = 1.2$, $SD = .72$, on the 5-point scale. An independent samples t-test showed that men had a discernibly higher level of baseline intergroup anxiety than women when thinking about interacting with a gay man, $M_m = 2.14$, $SD = .70$, and $M_w = 1.88$, $SD = .72$, respectively, with a difference of .26 (95% CI, .16, .36), $t(804) = 5.11$, $p < .001$.

Religiosity was assessed in three subscales; frequency of attendance at religious services, frequency of private worship, and intrinsic religious orientation. Most participants (42.43%) reported never attending religious services (e.g., church), and another large proportion of the sample (20.97%) only attended once a year or less. Participants’ frequency of private worship (e.g., prayer), on the other hand, was somewhat bimodal with 38.96% never engaging in private worship, and 26.55% engaging in private worship more than once a week (see Appendix N for a visual representation of the distribution of religious attendance, Figure N1; and private worship, Figure N2). The third subscale, intrinsic religious orientation, had a mean of $M = 2.75$, $SD = 1.44$, which is relatively low on a scale from 1 to 5 with 5 being the strongest intrinsic religious orientation.

**Previous direct contact.** As can be seen in Table 13 below, most participants had some direct contact with a member of the LGBTQ+ group prior to engaging in the study. The survey questions regarding previous contact sought to bring clarity to the forms of previous contact, and the closeness of those contact relationships. Previous contact responses were not used in the main analyses due to missing data (i.e., the 14.9% of participants who did not know any LGBTQ+ people did not provide data for the remaining direct contact questions) and unequal
group sizes (e.g., the proportions of people who had an LGBTQ+ friend (86.4%) versus those who did not have an LGBTQ+ friend (13.6%) were drastically different). However, the previous contact data is an important component of the sample demographics and is also useful supplementary information to aid in interpretation of the results.

Notably, the most common form of direct contact is friendship; 86.4% of those who knew an LGBTQ+ person had an LGBTQ+ friend, whereas having an LGBTQ+ family member was less common at 48.5%. Furthermore, most LGBTQ+ family contacts were extended family (e.g., cousin, aunt, uncle) as opposed to immediate family. The LGBTQ+ proportion of participants' friendship networks was relatively small, with most having only a few LGBTQ+ friends (52.2% had 3 or fewer, and at the extreme end, 3.9% reported having more than 10). Frequency of contact was also measured, with 62.9% (of the total sample) interacting with their LGBTQ+ contact at least once a month. These results show that direct contact is a common occurrence, and it impacts the large majority of the sample.
Table 13

*Distributions of previous direct contact items*

<table>
<thead>
<tr>
<th>Survey Item</th>
<th>n</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Respondent knows someone who is LGBTQ+</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>686</td>
<td>85.2</td>
</tr>
<tr>
<td>No</td>
<td>120</td>
<td>14.9</td>
</tr>
<tr>
<td>Has an LGBTQ+ family member</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Percentage of n = 686</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>333</td>
<td>48.5</td>
</tr>
<tr>
<td>No</td>
<td>352</td>
<td>51.4</td>
</tr>
<tr>
<td>If yes, what family member(s) (select all)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0 father</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td>0 mother</td>
<td>1</td>
<td>0.3</td>
</tr>
<tr>
<td>36 sister</td>
<td>36</td>
<td>10.8</td>
</tr>
<tr>
<td>21 brother</td>
<td>21</td>
<td>6.3</td>
</tr>
<tr>
<td>Immediate family total</td>
<td>58</td>
<td>17.4</td>
</tr>
<tr>
<td>23 aunt</td>
<td>23</td>
<td>6.9</td>
</tr>
<tr>
<td>32 uncle</td>
<td>32</td>
<td>9.6</td>
</tr>
<tr>
<td>172 cousin</td>
<td>172</td>
<td>51.7</td>
</tr>
<tr>
<td>Other extended family</td>
<td>121</td>
<td>36.3</td>
</tr>
<tr>
<td>Extended family total</td>
<td>348</td>
<td>104.5</td>
</tr>
<tr>
<td>Respondent has an LGBTQ+ friend</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Yes</td>
<td>593</td>
<td>86.4</td>
</tr>
<tr>
<td>No</td>
<td>93</td>
<td>13.6</td>
</tr>
</tbody>
</table>
Table 13

Continued

Number of LGBTQ+ friends a

<table>
<thead>
<tr>
<th>Number of Friends</th>
<th>Count</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>1-3</td>
<td>358</td>
<td>52.2</td>
</tr>
<tr>
<td>4-6</td>
<td>169</td>
<td>24.6</td>
</tr>
<tr>
<td>7-9</td>
<td>21</td>
<td>3.1</td>
</tr>
<tr>
<td>10 or more people</td>
<td>45</td>
<td>6.6</td>
</tr>
</tbody>
</table>

Frequency of contact with an LGBTQ+ person % of total sample (N = 806)

<table>
<thead>
<tr>
<th>Frequency of Contact</th>
<th>Count</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Never</td>
<td>46</td>
<td>5.7</td>
</tr>
<tr>
<td>Once a year or less</td>
<td>76</td>
<td>9.4</td>
</tr>
<tr>
<td>Once a year to once a month</td>
<td>177</td>
<td>22.0</td>
</tr>
<tr>
<td>Once a month to once a week</td>
<td>244</td>
<td>30.3</td>
</tr>
<tr>
<td>More than once a week</td>
<td>263</td>
<td>32.6</td>
</tr>
</tbody>
</table>

Note. Child and grandparent were unintentionally left out of the list from which participants selected family LGBTQ+ family members; n = 23 participants listed a child as their closest LGBTQ+ contact in the open-ended portion of the IOS scale (none listed a grandparent).

a Response variable was open-ended, but quantities are reported here for 1-3, 4-6, 7-9, and 10+ with the understanding that 1-3 is enough to make a significant impact on prejudice and the higher quantities are somewhat irrelevant (Bond & Compton, 2015; Herek & Capitanio, 1996).

Participants were also asked about their inclusion of other in the self with their closest direct contact, as closeness with intergroup contacts have been shown in many studies to mediate out-group attitudes (for a review see Turner et al., 2010). Results showed that participants were only moderately close with their direct contacts (IOS $M = 3.08, SD = 1.67$; see figure 4 for
pictorial interpretation). The IOS scale shows that the relationships between my participants and their previous direct contact are neutral on average.

![Figure 4. Pictorial representation of mean inclusion of other in the self with direct contact.](image)

It is worth noting that the RWA mean was lower in Study 2 than in Study 1 (M=69.18, SD=34.32 vs. M=84.24, SD=36.96, respectively). Despite evidence from the pilot study suggesting that level of education may be responsible for differences in RWA, in this study greater education does not seem to be the cause of the lower RWA scores; those with less than college had an RWA mean of $M = 64.9$, and those with at least some college had an RWA mean of $M = 78.9$. Rather, RWA scores seem to be most related to whether or not participants know someone who is LGBTQ+: $M = 88.25$ for those who do not know an LGBTQ+ person, and $M = 65.84$ for those who do, $t(804) = 6.78, p < .05, M = 22.40, 95\% \text{ CI} [15.91, 28.89]$.

**Procedures.** MTurk workers were paid $0.10 for responding to 6 demographic questions in the pre-screening survey: they provided their age, state of residence, ethnicity, educational attainment, gender, and sexual orientation/gender identity. Eligible participants were next paid $0.75 for the 8-minute pre-test, which contained detailed questions about direct and extended contact, religion, as well as the RWA scale, intergroup anxiety, and sexual prejudice. Sexual prejudice was measured both in the pre-test and post-test with a two-week interval between surveys. As in Study 1, the manipulation and post-test were presented to pre-test participants as
an ostensibly separate study. Consent procedures and the manipulation/post-manipulation survey were the same as in Study 1.
CHAPTER 6

STUDY 2: RESULTS AND DISCUSSION

Preliminary Analysis

Data Cleaning

As was the case in Study 1, there was no missing data in the data set. Reverse scored items were recoded manually, and the data were visually inspected using graphs and preliminary analysis to ensure there were no obvious errors that may have occurred during export or cleaning. All scale reliabilities were adequate (i.e., \( \alpha > .90 \)), and statistical assumptions were met (see Appendix O for scale reliabilities, and Appendix P for tests of statistical assumptions relevant to MANOVA).

Descriptive Statistics

The sexual prejudice scale performed as expected based on past literature, but RWA scores were lower than expected. The RWA mean was \( M = 69.18, SD = 34.32 \); out of a maximum possible score of 120, the mean in this data set is 15 points below the mean from Study 1, and 20 points below the 2006 average reported by Altemeyer (2006). See table 14 for means and standard deviations of all main variables, and table 15 for scale correlations.
Table 14

*Means and standard deviations of main variables*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>69.18</td>
<td>34.32</td>
</tr>
<tr>
<td>Pre-test Sexual Prejudice</td>
<td>2.04</td>
<td>1.03</td>
</tr>
<tr>
<td>Post-test Sexual Prejudice</td>
<td>1.85</td>
<td>0.93</td>
</tr>
<tr>
<td>% Cut from HRC</td>
<td>24.7</td>
<td>28.2</td>
</tr>
</tbody>
</table>

*Note.* % Cut from HRC is the mean percent cut from the LGBTQ+ rights organization when instructed to cut 20% from 5 organizations overall; a mean of greater than 20% suggests that more was cut from the LGBTQ+ rights organization than was necessary to complete the budget reallocation task.

Table 15

*Correlations*

<table>
<thead>
<tr>
<th></th>
<th>RWA</th>
<th>Pre-Sexual Prejudice</th>
<th>Post-Sexual Prejudice</th>
<th>% Cut from HRC</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>1</td>
<td>.826**</td>
<td>.761**</td>
<td>.427**</td>
</tr>
<tr>
<td>Pre-test Sexual Prejudice</td>
<td>.826**</td>
<td>1</td>
<td>.906**</td>
<td>.407**</td>
</tr>
<tr>
<td>Sexual Prejudice</td>
<td>.761**</td>
<td>.906**</td>
<td>1</td>
<td>.501*</td>
</tr>
<tr>
<td>% Cut from HRC</td>
<td>.427**</td>
<td>.470**</td>
<td>.501**</td>
<td>1</td>
</tr>
</tbody>
</table>

*Note.* **Correlation is significant at the .001 level (2-tailed)
As noted in the previous chapter, the budget cuts task required participants to reallocate budgets among 5 social organizations; 1 of those organizations was the LGBTQ+ organization used to measure subtle discrimination (Human Rights Campaign; HRC), and 4 were used as decoy organizations. I compared the effects of all organizations to test whether the cuts made to the HRC were meaningful over and above the decoys (see Table 16). To assess how the budget cuts tool functioned with the other variables in the model, a MANOVA was conducted with target's support for LGBTQ+ issues and target's sexual orientation as independent variables, and the percent cuts to the 5 social organizations as the dependent variables. As expected, I found a significant effect of target's sexual orientation on the percent cuts to the LGBTQ+ organization (see Appendix Q). Although most of the decoy organizations were not affected by the variables of interest, there were two exceptions: the National Wildlife Refuge Fund (NWRF) was impacted by target's sexual orientation, and the Great Lakes Restoration Initiative (GLRI) was impacted by participant’s gender (see Appendix Q, Tables Q2 and Q4 for detailed results). The meaningfulness of gender and sexual orientation on these decoy items is interesting, but the NWRF and GLRI remain theoretically irrelevant to the LGBTQ+ organization, and I surmised that the inclusion of these decoys should not have impacted responses to the LGBTQ+ organization item.
Table 16

Comparison of Mean Budget Cuts by Organization

<table>
<thead>
<tr>
<th>Organization</th>
<th>Mean Percent Cuts</th>
<th>SD</th>
<th>Starting Budget (in Million $)</th>
<th>Mean Cut (in Million $)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Human Rights Campaign (Target)</td>
<td>24.7%</td>
<td>28.2%</td>
<td>46</td>
<td>11.37</td>
</tr>
<tr>
<td>Great Lakes Restoration Initiative</td>
<td>27.8%</td>
<td>26.6%</td>
<td>40</td>
<td>11.10</td>
</tr>
<tr>
<td>African Development Foundation</td>
<td>41.3%</td>
<td>34.4%</td>
<td>26</td>
<td>10.75</td>
</tr>
<tr>
<td>National Wildlife Refuge Fund</td>
<td>-2.7%</td>
<td>62.1%</td>
<td>13</td>
<td>-0.35</td>
</tr>
<tr>
<td>U.S. Interagency Council on Homelessness</td>
<td>-171.7%</td>
<td>300.5%</td>
<td>4</td>
<td>-6.87</td>
</tr>
</tbody>
</table>

Note. A negative value signifies money added to the organization’s fund.

Building the Model

In earlier analysis of the baseline intergroup anxiety variable, a gender difference was found in intergroup anxiety scores (see page 85 for details and results of analysis). Thus, tests of moderation were conducted to rule out anxiety’s role as a moderator of the experimental conditions on sexual prejudice or subtle discrimination. An interaction term was created for the product of each independent variable with intergroup anxiety, and separate regressions were conducted for each interaction term – none of which were significant predictors of the dependent variables, suggesting that intergroup anxiety had no moderating effects (see Appendix R for results of moderation analysis). I also tested RWA for moderating effects, as it is occasionally treated as a moderator rather than a covariate in related research (e.g., Dhont & Van Hiel, 2011).
Moderation analysis showed that RWA did not moderate the effects of condition on sexual prejudice or subtle discrimination (see Appendix S for results of moderation analysis).

A baseline measure of sexual prejudice was included in the pre-test to ensure that the manipulation had an effect in the desired direction. Therefore, a paired samples t-test was conducted to compare sexual prejudice before and after the manipulation. Post-test prejudice scores ($M = 1.84, SD = 0.93$) were significantly lower than pre-test prejudice scores ($M = 2.04, SD = 1.03$), $t(805) = 12.28, p < .001$, with a difference of .19 (95% CI, .14, .22), confirming that the educational portion of the experimental set-up did have the intended effect overall. The impact of the manipulated conditions on the amount of change in sexual prejudice from pre- to post-test was also investigated. To test this, a difference score was calculated between pre- and post-test sexual prejudice, and this difference score was entered as a dependent variable in an ANOVA with the three independent variables (gender, target’s sexual orientation, and target’s explicit support for LGBTQ+ issues). The difference between pre- and post-test sexual prejudice scores was not impacted by condition.15

Analysis

I conducted a multivariate analysis of covariance (MANCOVA) on the overall 2x2x3 model, which included gender of participant (male, female), sexual orientation of the target (gay, straight), and target’s explicit support for LGBTQ+ issues (none, moderate, strong) as independent variables; right wing authoritarianism (RWA) as a covariate; and sexual prejudice and subtle discrimination as outcome variables. As in Study 1, RWA significantly predicted both

15 gender, $F(1, 794) = 2.77, p = .096$; target’s sexual orientation, $F(1, 794) = 0.11, p = .732$; target’s explicit support for LGBTQ+ issues, $F(1, 794) = 0.20, p = .812$. 
sexual prejudice, \( F(1, 672) = 11.34, p < .001, \eta^2 = .692 \) and subtle discrimination \( F(1, 672) = 2.80, p < .001, \eta^2 = .36 \) and was by far the strongest predictor of both outcome variables. Before testing the model in a MANCOVA, several tests were conducted to ensure that all assumptions associated with MANCOVA were met. First, a correlation analysis determined that the two dependent variables were appropriately correlated with each other at a moderate level (see Table 15, above). Also, Box’s test showed that the covariance matrices were equal, with a Box’s M value of 35.23 and a non-significant \( p \) value of .381. Finally, tests were conducted to ensure that the covariate did not violate the assumption of homogeneity of regression slopes with either dependent variable (see Appendix P, Tables P8 and P9). No violation was observed. Thus, I am confident that the statistical assumptions of the MANCOVA test were satisfied.

The MANCOVA showed statistically discernible effects of participant gender, Pillai’s Trace = .025, \( F(2, 792) = 10.30, p < .001, \eta^2 = .025 \), and target’s sexual orientation, Pillai’s Trace = .010, \( F(2, 792) = 4.11, p < .05, \eta^2 = .010 \), indicating a difference in the level of prejudice and discrimination between male and female participants, and gay and straight targets, but not in the manipulated level of target’s support for LGBTQ+ issues, Pillai’s Trace = .005, \( F(2, 793) = 1.08, p = .36, \eta^2 = .003 \).

Prior to conducting follow-up analyses of covariance (ANCOVA) to explore the effects of the model on each dependent variable, I used Levene’s test of equality of error variances to ensure that the homogeneity of variance assumption was met for both sexual prejudice (\( F(11, 794) = .66, p = .77 \)) and subtle discrimination (\( F(11, 794) = .89, p = .54 \)). Follow-up univariate tests of between-subjects effects were then run. When I tested for between-subjects effects in two separate ANOVAs, only gender of participant retained its statistically discernible effect (H3). There was a main effect of gender on subtle discrimination in that men cut more of the LGBTQ+
organization’s budget than women did ($M = 28\%$ vs. $M = 21\%$, respectively), $F_{1, 793} = 18.97, p < .001$, $\eta^2_p = .023, M = .07, 95\%$ CI [.04, .11]. There was also a main effect of gender on sexual prejudice in that men were more prejudiced than women ($M = 1.87$ vs. $M = 1.83$, respectively), $F_{1, 793} = 6.32, p < .05, \eta^2_p = .008, M = .10, 95\%$ CI [.02, .19].

**Discussion**

Study 2 was designed to strengthen the manipulation used in Study 1 by adding a third condition in which the target expressed stronger support for LGBTQ+ issues. The findings showed that strengthening the manipulation did not lead to detectable effects on this condition. However, despite a lack of main effects of either manipulated variable, gender of participant was found to impact sexual prejudice and subtle discrimination (H3). Study 2 provided support for an effect that is ubiquitous in the literature on sexual prejudice and discrimination – that men are more prejudiced (and discriminate more) against the LGBTQ+ outgroup than women (Ahrold & Meston 2010; Nagoshi et al., 2008; Norton & Herek, 2013).

The results contribute to the existing literature on gender as a predictor of sexual prejudice, and, particularly, as a predictor of discrimination against the LGBTQ+ outgroup when measured using a budget cuts task. To the best of my knowledge, the budget cuts task used herein has only been used twice before – once to measure discrimination against the LGBTQ+ outgroup, and once against a racial outgroup (Haddock et al., 1993; Son Hing et al., 2002). The finding that gender effects – known so well in the research on sexual prejudice – are replicated on this measure suggests a degree of convergent validity of the budget cuts tool.

There is much to consider in terms of why main effects were not found for the manipulations (discussed in Chapter 7, below); however, regardless of the unsupported hypotheses, Study 2 provides a unique measure of effectiveness of the anti-prejudice educational
video used in the design. When a baseline measure of sexual prejudice was compared with a post-test measure of sexual prejudice, overall sexual prejudice scores were lower after viewing the video. At the very least, there is potential in the educational portion of the video as an intervention for future prejudice reduction initiatives, either in research or in the real world. There is value in educational entertainment as a prejudice reduction tool, particularly in the ability to share educational videos through wide-spread and far reaching platforms such as YouTube and other social media outlets. A single 5-minute video, such as the one created for and used in this dissertation, can reach large amounts of viewers in very little time and with great convenience, and there is a place for future research to conduct a thorough evaluation of the efficacy of this, or similar, anti-prejudice videos as an intervention.
CHAPTER 7

GENERAL DISCUSSION

General Discussion

This dissertation focused on the pervasive social issues of prejudice and subtle discrimination against the LGBTQ+ group. Data collection occurred within the context of “Trump’s America,” which indicates a social climate of extreme dividedness on many social issues, including that of acceptance versus intolerance of sexual and gender minorities (Abramowitz & McCoy, 2018; Edsall, 2019). Despite the fact that prejudice is decreasing in America overall, incidents of violence against vulnerable populations is rising, which reflects this social divide in individual perceptions of the “other” (Hopkins & Washington, 2019; Rubin, 2018). In this social context, initiatives intending to reduce prejudice and discrimination need to be designed to appeal to their specific audience.

In this dissertation I focused on the prejudice reduction theory of intergroup contact, wherein simply having positive contact with a member of the out-group can reduce prejudice toward that out-group (Allport, 1954). My hypotheses drew on the findings that intergroup contact can backfire under certain circumstances – for instance, if the in-group member experiences anxiety about interacting with the out-group member, prejudice can increase rather than decrease (Shelton et al., 2009; Stephan, 2014). Furthermore, in-group members who have high levels of pre-existing prejudice toward the out-group are unlikely to come into contact with the out-group in a natural setting, and if they do, the contact is not as likely to be positive in nature (Hodson, 2011; Hodson et al., 2009). Thus, I posited that a more effective form of intergroup contact was extended contact, wherein prejudice toward an out-group is reduced simply by knowing about a fellow in-group member’s friendship with an out-group member.
The known benefits of extended contact include reduced effect of intergroup anxiety on the contact scenario, as well as increased likelihood of the contact scenario occurring in a natural setting (i.e., individuals who do not know any member of the LGBTQ+ group are no less likely to know a fellow in-group member who has LGBTQ+ friends; Hodson et al., 2009). However, despite the advantages that extended contact can have over direct contact in the given scenarios, it remains a weaker version of contact with the out-group by virtue of being indirect. I hypothesized that an extended contact who identified as an ally would create a stronger version of the extended contact scenario. That is, hearing about an in-group member’s relationship with an out-group member could have a stronger influence on prejudice if that in-group member was a strong supporter of the out-group’s rights, as opposed to an ambivalent friend of an individual out-group member.

I tested my research question in two experimental studies. Study 1 tested for effects of three variables on sexual prejudice and subtle discrimination: participant gender, target’s sexual orientation (gay, straight), and target’s explicit support for LGBTQ+ issues (ally/advocate, non-ally/non-advocate). Study 2 was essentially a replication of Study 1, but an additional, stronger level of target’s support for LGBTQ+ issues was added; the non-ally/non-advocate condition became the no support condition, ally/advocate became moderate support, and the additional level was a new, more passionately worded, strong support condition. More importantly, the samples for the two studies differed in participants’ previous contact with the out-group: Study 1 participants had no- to low-previous contact, and Study 2 recruited participants with all degrees of previous contact. Despite the fact that the two samples were contained in two separate studies and therefore were not statistically compared, the differences in the samples play an important part in the final interpretation of the results.
Comparing and Contrasting of Two Studies

Perhaps the most intriguing finding of this dissertation is the fact that the two studies provided such different main effects, despite having only subtle differences in design. In Study 1, the videos featuring a gay target resulted in less subtle discrimination than those featuring a straight target, whereas in Study 2, no difference was detected between the gay target and straight target groups (H1). Although I had hypothesized that extended contact would have a more significant effect at reducing prejudice and discrimination than would direct contact when the target was a straight ally (versus non-ally), the interaction I had hypothesized did not come to approach significance for either study (H2). Finally, no main effect of participant gender on prejudice and discrimination was found in Study 1, but small effects of gender were found on both prejudice and discrimination in Study 2 (H3).

In terms of the apparent lack of effectiveness of extended contact as an experimental approach to reduce prejudice and discrimination in Study 1, the take-home message should be that direct contact appears to work to reduce subtle discrimination against the out-group. I expected that extended contact would require certain conditions to show an effect over and above direct contact, and it is apparent that those conditions were not met. For example, I had anticipated that an extended contact who explicitly supported LGBTQ+ issues (ally) would make the extended contact condition more effective than the direct contact condition, and while it is possible that the ally condition did not work because support for LGBTQ+ issues is not a necessary condition of extended contact, it is also possible that the manipulation was not strong enough to create an effect.

Despite the differences in the results of the two studies, considering both studies as a whole provides a valuable insight on the context in which intergroup contact works to reduce
discrimination. This insight comes from the comparison of two samples on one key difference: participants’ level of previous direct contact, prior to engaging in the studies. Traditionally, high levels of previous direct contact with the outgroup (either high frequency or high quality) has confounded the effects of intergroup contact (both intergroup and extended; Cameron et al., 2011; Compton, 2015). This confounding effect is theorized to occur because individuals with high previous direct contact already have positive attitudes toward the out-group, and already perceive positive in-group norms about intergroup interaction (Bond & Compton, 2015; Cameron et al., 2011). In this dissertation, the sample with no- to low-previous contact experienced an effect of manipulated contact (direct intergroup contact, specifically), and the sample with a large proportion of participants with high previous contact appeared to be unaffected by either contact manipulation. These contrasting results are consistent with the idea that previous experiences of intergroup contact may lead to a floor effect, where the contact experiences in participants’ real lives have already reduced their prejudice to a point where the experimental manipulations could have no additional effect.

Another curious discrepancy between the two studies was in the main effect of target’s support for LGBTQ+ issues. In Study 1, my analysis showed that the educational messaging delivered by a target who self-identified as an ally or advocate for LGBTQ+ issues was associated with higher subtle discrimination than the same messaging delivered by a non-ally/non-advocate target. This finding was novel among the relevant research literature, and, as such, I interpreted the main effect with caution. The results of Study 2 showed no such main effect of target’s support for LGBTQ+ issues. In my earlier discussion of the results of Study 1 (see page 71), I introduced the concepts of cultural backlash and psychological reactance to help explain the phenomenon of allies and advocates inciting more discrimination. However, to
explain why this main effect was found in Study 1 but not in Study 2, I need to again turn to the key factor that makes the two samples different from one another: previous direct contact. Although to the best my knowledge no research has looked at how direct contact with an out-group affects the likelihood of experiencing reactance, there is a possibility that, given the lower level of previous direct contact of the sample in Study 1, this sample was more inclined to respond with reactance. This interpretation is purely speculative, but perhaps having LGBTQ+ friends or family members makes an ally or advocate’s message more relatable, whereas having little to no experience with members of the LGBTQ+ group invokes fears of a “gay agenda” when an ally or advocate pushes a pro-LGBTQ+ message. As it relates to psychological reactance, perhaps individuals with existing relationships with members of the LGBTQ+ group may feel less personally threatened by the notion of an ally or an advocate who’s mission it is to stop the discriminatory behaviours of others. If this hypothesis were to be confirmed in future research, it would provide even more reason to carefully match interventions with their audience, particularly in the area of previous direct contact with the LGBTQ+ group.

Prejudice versus discrimination

In Study 1, subtle discrimination was the criterion which measured discernible main effects; sexual prejudice as a measure picked up on gender effects (in both studies). There are two potential methodological explanations for the lack of main effect on prejudice: socially desirable responding and floor effects. Although much of social psychology research uses explicit prejudice measures (such as the sexual prejudice measure used in the present studies), there must always be awareness around the response bias inherent in asking someone to directly admit to being prejudiced, particularly when the social context is generally disapproving of such prejudice. This social desirability bias in responding to explicit prejudice measures can lead to
reduced detection of actual prejudice levels (Peltier & Walsh, 1990). On the other hand, the subtle discrimination measure used in the present studies was incorporated as a behavioural element to complement the explicit attitude measure. This discrimination measure was presented to participants as part of an ostensibly separate study and was not obviously related to participants’ attitudes or behaviours toward sexual minorities. Thus, I argue that subtle discrimination would have been much less impacted by socially desirable responding, if at all, and could therefore have picked up on more subtle effects than could an explicit prejudice measure.

On a related note, scores from the sexual prejudice scale were positively skewed with the majority of scores falling well below the scale’s center, which could suggest that floor effects prevented the observation of a meaningful reduction in prejudice due to the manipulation. The subtle discrimination measure, on the other hand, was not susceptible to floor effects – all participants cut (or added to) the LGBTQ+ organization’s budget from the same mid-point starting place. The fact that the budget cuts measure was able to pick up on such small effect sizes and not be as obviously susceptible to socially desirable responding makes it a valuable measure to include in any prejudice/discrimination-reduction studies.

Beyond the methodological explanations for the difference in prejudice and discrimination results, it is possible that the experimental manipulation changed participant behaviour, but was not powerful enough to change their attitudes – at least immediately. Attitude change is most often thought of as an antecedent of behaviour change; that is, people act based on pre-existing attitudes (e.g., Theory of Planned Behavior, Ajzen, 1991; MODE model, Fazio, 1990). However, it is possible for behaviour to change first, due to external factors, and for attitude change to follow later (Jhangiani & Tarry, 2014). Without the possibility for behaviour
to change in advance of attitudes, the well-known theory of cognitive dissonance would be impossible. Therefore, it is possible that the manipulation convinced participants to allocate more funding to an LGBTQ+ organization while not completely changing their deep set attitudes. With more positive behaviours toward the LGBTQ+ group, attitudes may begin to change through the process of cognitive dissonance.

Limitations and Recommendations for Future Research

As mentioned, the goal of this research program was to compare competing hypotheses proposed by direct contact and extended contact theories and to test whether certain manipulated conditions enhance the effectiveness of these approaches. Although I did not include a formal control condition in the design – that is, a condition in which participants do not see any information related to prejudice and discrimination of LGBTQ+ individuals – I argue that the goals for this research were better served by using a type of control condition wherein the educational content was delivered by a person who was neither an ally nor an advocate. My research was not designed to determine whether an intervention that includes content-relevant material would be more effective than no intervention since much research has already established that premise (see Bartoş et al., 2014 for a review; and Riggs et al., 2011 for an example). Instead, the intent of the two studies was to determine whether using an ally to deliver the manipulation (extended contact) would have more impact on reducing prejudice and discrimination than having this same information delivered by a member of the LGBTQ+ group (direct contact). However, upon completion of Study 1, I noted that the lack of baseline data limited my ability to interpret the results. Therefore, in Study 2, a baseline measure of sexual prejudice was included to provide a pre-post comparison and determine whether general participation in the experiment resulted in lower prejudice when compared to baseline. The
baseline measure also enabled me to test whether the specific conditions (e.g., control, modest, and strong manipulation) resulted in even stronger reduction of prejudice.

Despite my justification for not including a formal control condition from a methodological standpoint, the fact that all groups received the same educational content likely reduced my ability to find significant differences across the manipulated conditions. Since all participants saw the educational material, its impact was felt across all conditions as demonstrated from the pre- and post-test comparisons that showed lower sexual prejudice post-test when compared to pre-test across all conditions with no difference between conditions. Seeing the same educational content in all conditions surely contributed to the low effect sizes and to the possibility of making a Type II error in failing to observe an effect that exists in the population. Of course, while adding a formal control condition would be of benefit in interpreting the results of the study, it would also produce its own set of methodological challenges. For instance, using a 4-way factorial design instead of a 3-way design would further limit the power of the study and would require a very large sample size, thus making participant selection even more complicated than it was in these studies. But perhaps even more complex is finding a way to explain to participants why, in the formal control condition, an LGBTQ+ advocate or ally would be used for a study that has no relevance to sexual orientation. Doing so may challenge participants’ beliefs in the actual purpose of the study and may reduce the confidence in the study. Hence, this approach may not make intuitive sense despite any advantages it affords.

There were also limitations to the budget cuts tool that was used to measure the outcome variable of subtle discrimination. Recall that participants were asked to cut 20% of the overall funding allocated across 5 social organizations – the Human Rights Campaign (HRC) being the
target organization related to LGBTQ+ issues, and the remaining 4 being decoy organizations covering issues of wildlife preservation, African development, Great Lakes restoration, and homelessness. The organizations used for the task were real organizations with real funding allocations; using accurate information within the task was a failsafe to maintain the deception (for example, if a participant sought out more information on any of the organizations in their decision making process, they would find no reason to doubt the ostensible purpose of the task). However, there was also a limitation regarding the use of real organizations and actual funding allocations. The HRC (target LGBTQ+ organization) had the highest starting budget ($46 million), relative to the homelessness organization with the smallest starting budget ($4 million). A larger cut to the HRC would have been required to see a noticeable loss to the organization, whereas the same size cut to the homelessness organization would have been a much larger loss relative to the organization’s size. Therefore, participants may have been more willing to cut from the HRC as the cut would have been seen as less detrimental to the overall organization. Similarly, the budget cuts task is a zero-sum game, where providing money for (or cutting less money from) one organization necessarily results in cutting more money from another organization. Although the task was framed as cutting budgets overall, some organizations’ funds were increased, and in order to do so, more money was cut from elsewhere than likely would have been if the cuts to each organization were not relative to each other. Regardless of this limitation, the budget cuts task has been used successfully in the past (Haddock et al., 1993; Son Hing et al., 2002). Future research on the psychometric validity of the budget cuts task is needed to contribute to the understanding of how selection of decoy items and starting budget values may impact the tool’s ability to measure subtle discrimination against various outgroups.
Another limitation related to measurement was the occurrence of intergroup anxiety as a baseline (moderator) rather than as a post-manipulation variable (mediator). That is, although the measurement of intergroup anxiety was not useful in the analysis of the present data, the variable was measured as a baseline, prior to priming participants with any sense of interaction with an out-group member. Future studies seeking to compare extended with direct intergroup contact should measure intergroup anxiety immediately following interaction with an in-group or out-group target. Doing so would illuminate whether an extended contact scenario results in less intergroup anxiety toward the LGBTQ+ group, thereby providing an alternative path to prejudice reduction for individuals who are deterred from effective intergroup interaction by their anxiety around such interaction.

Similarly, inclusion of other in the self is a well-known piece of the contact theory puzzle, but its use as a baseline measure was not valuable in the present study. Although IOS did not moderate the effects of target’s sexual orientation on prejudice and discrimination, future studies may benefit from measuring it as a mediator. That is, closeness with the target can help explain differences in effect between an extended and a direct contact – including a target in the self (or in one’s own identity) has been shown to mediate effectiveness of various contact scenarios (Eller et al., 2011a; Gómez et al., 2011).

There are also some limitations associated with the way the conditions were tested through the use of the video. Most notable is the computer-mediated nature of the contact intervention. Although many studies have successfully reduced prejudice using vicarious forms of contact (e.g., imagined contact, virtual contact, and of course, extended contact), effects may be smaller from these forms of intervention as opposed to traditional, face-to-face contact (see Dovidio et al., 2017, for a review). Similarly, extended contact as a budding theory has not yet
settled on a single operational definition of what constitutes extended contact: some argue that an in-group member’s friendship with an out-group member actually needs to be observed, whereas others argue that this friendship only needs to be known about (Harwood, 2010; Wright et al., 1997). The present studies utilized the less-direct latter option, which may have weakened the effects of the extended contact manipulation.

The concern with vicarious, computer-mediated contact is that it is likely to produce lower quality, or less personally meaningful, contact than in-person contact (Dovidio et al., 2017). A good way to make a contact experience more meaningful is to ensure that the participant has a close relationship with the target, or – in the case of extended contact – that the observed relationship between an in-group target and an out-group member is a close friendship (Gómez et al., 2011). That is because, for both direct and extended contact, effects are generally stronger when the intergroup relationships are friendships as opposed to acquaintances or strangers (Turner et al., 2009, Turner et al., 2007). Even in some experimental settings, a fast friends procedure can be conducted to artificially create the conditions of a friendship (Aron et al., 1997; Lytle & Levy, 2015).

To ensure the best chance for success, future research would provide the most tangible version possible of an extended contact intervention; for example, where possible, actual intergroup friends could be observed interacting, or social network analysis could be used to test real-world networks of in-group connections to the out-group. Given that these approaches are not easily conducive to experimental designs, though, the benefits of such methods may not always outweigh the limitations. In the present studies, in-person friendships (either naturally occurring or manipulated) were not utilized due to the sheer number of participants needed to achieve an acceptable power for the present experimental model. Perhaps more important than
the logistical concerns, however, is that my video content would not have been broadly reaching if it were not for the study’s online nature. Testing the manipulations with a large number of diverse participants makes the results more generalizable and applicable to use in the real world.

Another potential limitation of the experimental design was the focus on a male target only. The decision to use a single target was based on the exploratory nature of the target’s support for LGBTQ+ issues hypothesis and the overall size of the model – I needed to keep a narrow scope in terms of the manipulations and outcomes in order to test all of the variables I was interested in within a reasonable sample size. I chose a male target (as a starting point) because past research has shown that there tends to be more prejudice and discrimination against gay men than against lesbians, and therefore I anticipated more room for improvement (i.e., larger effects) in a manipulation involving a gay man. Furthermore, gender politics played a role in my decision to use a male target to deliver the educational messaging: feminist research posits that men have more power to influence than women in patriarchal society (Carli, 2001).

Ultimately, in the first attempt at testing a new model, I wanted the most persuasive target possible in order to detect effects of unknown magnitude. Of course, research testing various forms, predictors, moderators and mediators of extended contact should continually expand the populations considered as in-groups and out-groups so that various ethnicities, nationalities, sexual orientations, and gender identities are tested for their reactions to extended contact.

Finally, although the experiment did successfully reduce prejudice and subtle discrimination overall, without a longitudinal follow-up design, it is impossible to know if this change was lasting. Prejudice and discrimination reduction research is often limited in this regard because experimental manipulations and interventions tend to be short-lived and follow-ups or longitudinal designs are rare (e.g., only 6.6% of the samples in one meta-analysis of prejudice
reducing initiatives were tested over time; Forscher et al., 2017). Furthermore, when the longevity of such interventions are tested, most do not have lasting effects on attitude change for more than a matter of days (Lai et al., 2016). Thus, even for interventions that show meaningful change in initial data, long-lasting effects cannot be assumed. There is a need for more longitudinal prejudice reduction research, specifically in testing interventions for lasting effects.

**Concluding Statement**

In the United States right now, the presidential administration is creating and maintaining a context of social dividedness where a portion of the population is pushed toward a cultural backlash against minorities, diversity, and inclusiveness. In this social context, work that seeks to reduce prejudice and discrimination needs to consider who its audience is, and tailor content (and content delivery) accordingly. This suggestion applies to traditional social psychological theories of prejudice reduction, including intergroup contact. Rather than expecting intergroup contact to be effective for all Americans, researchers and advocates need to be aware that some demographic characteristics can change how individuals respond to contact.

This dissertation has shown that differences in degree of previous direct contact with a member of the LGBTQ+ group may change how individuals respond to contact-related interventions. In applying these findings to prejudice reduction initiatives, individuals with little to no previous contact with the out-group are more likely to be persuaded by direct contact with a gay person than by extended contact via a fellow member of their in-group.

Although I did not confirm my hypothesis that straight allies would be more effective than members of the LGBTQ+ group at reducing prejudice among a heterosexual audience, I believe there is more research to be done on how best to utilize allies, extended contact, and even direct intergroup contact to achieve maximum efficacy in reduction of prejudice and
discrimination. This dissertation scratched the surface of new and developing ideas in the realm of allyship with the finding that allies and advocates may actually cause prejudice reduction interventions to backfire, with possible psychological reactance leading to increased levels of subtle discrimination. It is my hope that future research will continue to investigate how and why allies can result in such different effects on discrimination depending on the audience. Despite the lack of conclusive evidence in favour of allies, or even in favour of extended contact, I believe that there is great potential in knowing more about how allies work to reduce prejudice and discrimination. Particularly given that allies *could* be detrimental to such initiatives in the wrong set of circumstances, the questions that this dissertation has unearthed about an identity with such sparse research focus beg for further investigation.

In the meantime, the educational messaging put forth in this series of research experiments did benefit the “greater good.” As the pre-post comparison of prejudice in Study 2 revealed, participants did walk away with reduced prejudice toward the LGBTQ+ group. Whether this reduction in prejudice lasted or not cannot be known without follow-up research, but an experimental design that has an effect on sexual prejudice among a diverse population of heterosexual Americans is one worth maintaining.
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anticipated professional behaviors regarding homosexuality and gay and lesbian issues.

*Teaching and Teacher Education, 27, 201-209.*


Appendix A

Pre-Screen Tool for Study 1

1. What is the highest level of education you have completed? *(drop-down menu)*
   a) 8th grade (or less)*
   b) some high school*
   c) high school diploma (12th grade)*
   d) vocational/trade school*
   e) Some college/university
   f) Undergraduate college/university degree
   g) Master’s Degree (graduate school)
   h) Professional Degree (e.g., MD, DVM, DDS, PhD)

*eligibility requirement*

2. To what extent is this true of you: I am a member of the LGBTQ+ community.
   *Note: Membership in the LGBTQ+ community means you identify as lesbian, gay, bisexual, transgender, queer, or any other sexual or gender diverse identity, that is not heterosexual.
   a) untrue of me*
   b) I don’t know
   c) true of me

*eligibility requirement*

3. Do you personally know anyone who identifies as LGBTQ+?
   a) Yes
   b) No*

*Eligibility requirement; OR a to c on the following question*

4. If yes, please indicate how frequently you interact with anyone who is LGBTQ+:
   a) never; *
   b) once a year or less; *
   c) anywhere from once a month to once a year; *
   d) anywhere from once a week to once a month;
   e) more than once a week

*eligibility requirement*
Appendix B
Study 1 Pre-Test

Demographics

1. How old are you? (*open-ended*)

2. What state do you live in currently? (*drop-down menu*)

3. What is your ethnicity? (*drop-down menu*)

4. What is your gender? (*open-ended*)

5. **Think of this ladder as representing where people stand in the United States.**

At the **top** of this ladder are the people who are the best off – those who have the most money, the most education and the most respected jobs. At the **bottom** are the people who are the worst off – who have the least money, least education, and the least respected jobs or no job. The higher up you are on this ladder, the closer you are to the people at the very top; the lower you are, the closer you are to the people at the very bottom.

**Survey utilized a ladder graphic on which participants could click “rungs” that represented where they felt they were located in relation to others.**
Intergroup Anxiety Toward Homosexuals (Muñoz, 2003)

(1) Strongly Disagree;
(2) Disagree;
(3) Neither Agree Nor Disagree;
(4) Agree;
(5) Strongly Agree

1. I would feel nervous if I had to sit alone in a room with a gay person and start a conversation.
2. I just do not know what to expect from gay people.
3. Although I do not consider myself to be homophobic, I do not know how to present myself around gay people.
4. My lack of knowledge about the gay culture prevents me from feeling completely comfortable around gay people.
5. I can interact with gay people without experiencing much anxiety.
6. If I were at a party, I would have no problem with starting a conversation with a gay person.
7. It makes me uncomfortable to bring up the topic of homophobia around gay people.
8. I experience little anxiety when I talk to gay people.
9. The cultural differences between gay and straight people makes interactions between gay and straight people awkward.
10. I would experience some anxiety if I were the only straight person in a room full of gay people.

I worry about coming across as homophobic when I talk with gay people.
RWA (Altemeyer, 2006)

This survey is part of an investigation of general public opinion concerning a variety of social issues. You will probably find that you agree with some of the statements, and disagree with others, to varying extents. Please indicate your reaction to each statement according to the following scale:

(-4) You very strongly disagree with the statement.
(-3) You strongly disagree with the statement.
(-2) You moderately disagree with the statement.
(-1) You slightly disagree with the statement.
(0) You feel exactly and precisely neutral about the statement.
(1) You slightly agree with the statement.
(2) You moderately agree with the statement.
(3) You strongly agree with the statement.
(4) You very strongly agree with the statement.

Important: You may find that you sometimes have different reactions to different parts of a statement. For example, you might very strongly disagree (“-4”) with one idea in a statement, but slightly agree (“+1”) with another idea in the same item. When this happens, please combine your reactions, and [record] how you feel on balance (a “-3” in this case).

1. The established authorities generally turn out to be right about things, while the radicals and protestors are usually just “loud mouths” showing off their ignorance.
2. Women should have to promise to obey their husbands when they get married.
3. Our country desperately needs a mighty leader who will do what has to be done to destroy the radical new ways and sinfulness that are ruining us.
4. Gays and lesbians are just as healthy and moral as anybody else.
5. It is always better to trust the judgment of the proper authorities in government and religion than to listen to the noisy rabblerousers in our society who are trying to create doubt in people’s minds.
6. Atheists and others who have rebelled against the established religions are no doubt every bit as good and virtuous as those who attend church regularly.
7. The only way our country can get through the crisis ahead is to get back to our traditional values, put some tough leaders in power, and silence the troublemakers spreading bad ideas.

8. There is absolutely nothing wrong with nudist camps.

9. Our country needs free thinkers who have the courage to defy traditional ways, even if this upsets many people.

10. Our country will be destroyed someday if we do not smash the perversions eating away at our moral fiber and traditional beliefs.

11. Everyone should have their own lifestyle, religious beliefs, and sexual preferences, even if it makes them different from everyone else.

12. The “old-fashioned ways” and the “old-fashioned values” still show the best way to live.

13. You have to admire those who challenged the law and the majority’s view by protesting for women’s abortion rights, for animal rights, or to abolish school prayer.

14. What our country really needs is a strong, determined leader who will crush evil, and take us back to our true path.

15. Some of the best people in our country are those who are challenging our government, criticizing religion, and ignoring the “normal way things are supposed to be done.”

16. God’s laws about abortion, pornography and marriage must be strictly followed before it is too late, and those who break them must be strongly punished.

17. There are many radical, immoral people in our country today, who are trying to ruin it for their own godless purposes, whom the authorities should put out of action.

18. A “woman’s place” should be wherever she wants to be. The days when women are submissive to their husbands and social conventions belong strictly in the past.

19. Our country will be great if we honor the ways of our forefathers, do what the authorities tell us to do, and get rid of the “rotten apples” who are ruining everything.

20. There is no “one right way” to live life; everybody has to create their own way.

21. Homosexuals and feminists should be praised for being brave enough to defy “traditional family values.”

22. This country would work a lot better if certain groups of troublemakers would just shut up and accept their group’s traditional place in society.
Appendix C

Video Script for Experimental Manipulation

Table C1

*Educational Entertainment Content of the Experimental Manipulation Script*

<table>
<thead>
<tr>
<th>Component</th>
<th>Script</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brief description of sexual orientation</td>
<td>“Sexual orientation is about who you’re attracted to – at the most basic level, you can be attracted to the same sex <em>[the words “lesbian or gay” shows up on the screen]</em>, both sexes [bisexual], or the opposite sex [heterosexual]. That’s right, heterosexual is a sexual orientation, too.”</td>
</tr>
<tr>
<td>Population statistics</td>
<td>Although most people identify as heterosexual, there are actually more lesbian, gay, and bisexual [“LGB”] people in America than you might think. A recent Gallup poll found that 4.1% of Americans identified as LGB – that’s over 10 million people [4.1% identify as LGB (Gates, 2017)]. When researchers account for things like hiding one’s sexual orientation, that number goes up. Using advanced survey methods, recent estimates from Harvard researchers are as high as 19% of the population identifying as not heterosexual [up to 19% of Americans are not heterosexual (Coffman et al., 2017)]. So it turns out, we all probably know LGBT people, whether we’re aware of it or not.</td>
</tr>
<tr>
<td>Bridge</td>
<td>LGBT people are a marginalized group – that means they’re kind of social outsiders, and don’t always get to enjoy the same rights and privileges as the rest of society. In order to help understand why we as a society treat marginalized groups differently, let’s turn to some psychology.</td>
</tr>
</tbody>
</table>
Table C1

Continued

| Psychological phenomenon of stereotypes | Did you know that the human brain has ways to take mental short cuts when we approach someone or something new? Your brain acts fast to categorize new people so you can know how you should interact with them. It’s because of our super speedy brains that we end up stereotyping people [Stereotypes]. For example, if a person in a balaclava approached you in the middle of summer, your brain would rely on stereotypes to quickly categorize that person as a criminal and tell you to stay away! The only problem with this system is, stereotypes, like first impressions, can be turn out to be inaccurate once you get to know the person. |
| Example of a debunked negative gay stereotype | Fact or fiction: All gay men are effeminate and all lesbians are tomboys [Fact or Fiction: All gay men are effeminate, all lesbians are masculine]. It’s fiction! Actually, masculinity and femininity are unrelated to sexual orientation [UNRELATED to sexual orientation]. Gay men and lesbians, just like straight people, usually have a mix of stereotypically feminine traits and stereotypical masculine traits. But because of this stereotype, when we see an effeminate man or a woman who has masculine traits, our brains take a short cut and categorize them as gay or lesbian. Sometimes that’s an accurate assumption, but sometimes it’s not. And chances are, we’ve met masculine gay men and feminine lesbians without even knowing it – because we didn’t have that stereotype to rely on, we didn’t categorize them as gay or lesbian. |
Facts about the current status of LGBT legal rights

The Supreme Court has gotten involved in gay and lesbian rights recently with regards to the Defense of Marriage act. In Obergefell v. Hodges, the Supreme Court ruled that the right to marry was a fundamental right and applied to same-sex couples, which required that all states issue marriage certificates to, and recognize the marriages of, same-sex couples. Despite the very few gay and lesbian rights cases that the Supreme Court has taken on, the rights of the gay and lesbian community are mostly dictated by jurisdiction, so different states have different laws regarding their rights and discrimination.

Examples of how prejudice and discrimination affects LGBT people negatively

Last year alone, more than 100 anti-LGBT bills were introduced in 29 states. Many were religious exemption bills which allow people to disregard the law on the basis of their religion. Another common bill that received a lot of air time on the news was the “bathroom bill” [Bathroom bills prevent transgender people from using the restroom that matches their gender identity]. In 2017, 21 states [21 states] introduced bills to prevent transgender people from using restrooms that match the gender they identify as.

LGBT rights issues are about more than just being able to get married or use public restrooms, though. All around the world, gay, lesbian, bisexual and transgender people are beaten, tortured, and murdered because of their sexual orientation or gender identity. This happens in America, too. In 2016, over one thousand anti-gay violent attacks were documented by the National Coalition of Anti-Violence Programs. The NCAVP reported that 2016 was the deadliest year for LGBT people in the United States.
If you were surprised or upset to hear these kinds of statistics, you’re not alone. There are many straight people out there who believe that LGBT people should have equal rights and should be free from discrimination and violence. I thought I’d leave you with some ideas of how to respond when you hear someone using anti-gay slurs, discriminating against gays or lesbians, or otherwise perpetuating these challenges that LGBT people face. You could try to get the person to relate to you by reflecting on how your own attitudes have changed; I used to think that way too, but then I learned that... [I used to think that way too, but then I learned that…]. If you hear someone saying that’s so gay, you could suggest a different term for them to use, like Did you mean that’s not cool? [Did you mean that’s not cool?]. If someone in your workplace is discriminating against an LGBT employee, you could stand up for your colleague, tell them that you see the discrimination they are facing, and offer to back them up if they want to launch a formal complaint [stand up for them, believe them, offer to help them in their fight for equal rights].

<table>
<thead>
<tr>
<th>Ideas of how to respond to sexual prejudice and discrimination when it is observed</th>
<th>If you were surprised or upset to hear these kinds of statistics, you’re not alone. There are many straight people out there who believe that LGBT people should have equal rights and should be free from discrimination and violence. I thought I’d leave you with some ideas of how to respond when you hear someone using anti-gay slurs, discriminating against gays or lesbians, or otherwise perpetuating these challenges that LGBT people face. You could try to get the person to relate to you by reflecting on how your own attitudes have changed; I used to think that way too, but then I learned that... [I used to think that way too, but then I learned that…]. If you hear someone saying that’s so gay, you could suggest a different term for them to use, like Did you mean that’s not cool? [Did you mean that’s not cool?]. If someone in your workplace is discriminating against an LGBT employee, you could stand up for your colleague, tell them that you see the discrimination they are facing, and offer to back them up if they want to launch a formal complaint [stand up for them, believe them, offer to help them in their fight for equal rights].</th>
</tr>
</thead>
<tbody>
<tr>
<td>Closing Statement</td>
<td>In making this video, my classmates and I have learned a lot about a group of people who are still fighting for legal rights to live their lives with equality and fairness. I hope you’ve found our video interesting and informative, and I hope you’ve enjoyed watching!</td>
</tr>
</tbody>
</table>

When the video ended, participants were shown a list of the references for the data included in the video.
References


Appendix D
Post-Manipulation Survey
(Studies 1 and 2)

Instructions: We are interested in your assessment of the video that you just viewed. In the following survey, some questions will ask you to directly assess the video, and others are included to help us get a better idea of who is watching the video, and what perspective your assessment is coming from.

Reflecting on the Video

1. Please take a moment to tell us something you liked and something you didn’t like about the video you just watched. [open-ended]
   a. Liked:
   b. Didn’t like:

2. We are curious what information in the video was the most memorable. Please list up to three piece of information that you remember from the video:

3. This video may be applied to an actual anti-prejudice or diversity training workshop in the future. What, if anything, would you change about it to make it more likely to influence people’s perceptions or attitudes?
Likability Scale

Please rate the person in the video on the following statements, using the scale from 1 (very strongly disagree) to 7 (very strongly agree). He will not see individual people’s responses – only an aggregated total from everyone who completed this survey.

(1) Very strongly disagree
(2) Strongly disagree
(3) Disagree
(4) Neither Agree Nor Disagree
(5) Agree
(6) Strongly agree
(7) Very Strongly Disagree

1. This person is friendly.
2. This person is likeable.
3. This person is warm.
4. This person is approachable.
5. I would ask this person for advice.
6. I would like this person as a coworker.
7. To answer this question, please choose the fourth, “neither agree nor disagree.”
8. I would like this person as a roommate.
9. I would like to be friends with this person.
10. This person is physically attractive.
11. This person is similar to me.
12. This person is knowledgeable.
Sexual Prejudice Scale (repeated from pre-test)

Instructions: We’d now like to know your views on matters related to sexual preferences. Please read the following questions and answer them using the following scale:

(1) Strongly disagree
(2) Disagree
(3) Neither Agree Nor Disagree
(4) Agree
(5) Strongly agree

1. It’s wrong for men to have sex with men.
2. Lesbians are confused about their sexuality.
3. Marriage between two men should be kept illegal.
4. It’s morally wrong to be a lesbian.
5. A sexual relationship between two men is unnatural.
6. Lesbians are harming the traditional family.
7. Gay men are immoral.
8. I disapprove of lesbians.
9. I think it’s gross when I see two men who are clearly “together.”
10. Marriage between two women should be legal.
11. There’s nothing wrong with being a gay man.
12. Being a lesbian is a normal expression of sexuality.
13. Health care benefits should include partners of gay male employees.
14. Lesbians want too many rights.
15. Hospitals should allow gay men to be involved in their partners’ medical care.
16. Lesbians should have the same civil rights as straight women.
17. Retirement benefits should include the partners of gay men.
18. Employers should provide retirement benefits for lesbian partners.
19. Family medical leave rules should include the domestic partners of gay men.
20. Employers should provide health care benefits to the partners of their lesbian employees.
Budget Cuts Task

[*Note: items within this scale were counterbalanced so that the LGBT organization appeared in random order within the list for each participant.]

Instructions: The law class that produced the video you just watched is also involved in advocating for public policy changes. Please take a few minutes to complete the following exercise to help inform the students as they gain experience advocating for social organizations.

The current federal government administration is making changes to the funding allocations of social services, and intends to cut 20% of funding for social services across the board. As a class, we are interested in hearing the public’s views on how the limited funds should be allocated. In addition to our academic interest in public opinions regarding funding to social services, we intend to present our findings to policy makers in an effort to have your opinions inform future budget decisions.

Presented here is a list of social services that are currently under scrutiny for budget cuts. The federal funding allocated to each organization in 2017 is provided for you as a starting point. Your task is to reallocate funds across these organizations until the overall funds allocated are 20% less than they were in 2017. Use the slider tool for each organization/program to either degrease or increase the proposed budget until the indicated below shows an overall 20% cut.
Table D1.

*Items and starting budgets in budget cuts task*

<table>
<thead>
<tr>
<th>Organization/Program Name</th>
<th>Description</th>
<th>Current Funding</th>
</tr>
</thead>
<tbody>
<tr>
<td>National Wildlife Refuge fund</td>
<td>This is a revenue-sharing fund that makes payments to counties where wildlife refuges are located from fees the Fish and Wildlife Service receives</td>
<td>$13 million</td>
</tr>
<tr>
<td>Great Lakes Restoration Initiative</td>
<td>Endeavors to protect and restore the Great Lakes by strategically targeting the biggest threats to the ecosystem.</td>
<td>$40 million</td>
</tr>
<tr>
<td>African Development Foundation</td>
<td>An independent foreign aid agency focusing on economic development in Africa.</td>
<td>$26 million</td>
</tr>
<tr>
<td>U.S. Interagency Council on Homelessness</td>
<td>An independent agency coordinating the federal government's efforts to reduce homelessness.</td>
<td>$4 million</td>
</tr>
<tr>
<td>Human Rights Campaign</td>
<td>The largest national lesbian, gay, bisexual, transgender and queer civil rights organization.</td>
<td>$46 million</td>
</tr>
</tbody>
</table>

**Open-Ended Concluding Question**

Is there anything else you’d like to say in response to the video you viewed, or the survey items you completed?
Appendix E

Pilot Study Survey

*These questions were seen by pilot participants following the debriefing information*

Thank you for participating in this research on ways to reduce prejudice and discrimination against LGBT people. Your role as a participant was to pilot test the materials before we disseminate it to a larger population. As such, your feedback on the study is extremely important to us.

Note that *none* of your responses will be shared with the actor in the video.

1. Did you believe that the actor in the video was a law student? If not, why?
2. Did you believe that the video was made for a class project by the actor and his classmates? If not, why?
3. At one point in the video, the actor disclosed his sexual orientation. Did you believe him?
4. Did you believe that the actor was American?
5. Were the survey materials easy to understand and fill out?
   a. Yes
   b. No
   i. Can you describe the problems with the materials?
6. Please rate the physical attractiveness of the actor on a scale from (1) not at all attractive to (10) extremely attractive.
7. Please rate the masculinity of the actor on a scale from (1) feminine to (10) masculine.
8. We would like to know what you liked and didn’t like about the video overall. You may reflect on the actor, the dialogue, the video quality, or any other component.
   a. What I liked:
   b. What I didn’t like:
Appendix F

Internal Reliability of Scales for Study 1

Table F1

<table>
<thead>
<tr>
<th>Scale Reliabilities</th>
<th>Actual</th>
<th>Expected</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>$\alpha = .95$</td>
<td>$\alpha = .90$</td>
<td>Altemeyer, 2006</td>
</tr>
<tr>
<td>Sex Prej</td>
<td>$\alpha = .98$</td>
<td>$\alpha = .90$</td>
<td>Chonody, 2013</td>
</tr>
<tr>
<td>Likeability</td>
<td>$\alpha = .91$</td>
<td>$\alpha = .91$</td>
<td>Reysen, 2005</td>
</tr>
</tbody>
</table>
Appendix G

Testing of Statistical Assumptions for Study 1

Normality

Figure G1. Distribution of RWA scores.
Figure G2. Distribution of RWA scores.
Figure G3. Distribution of RWA scores.

Table G1

*Skewness coefficients of main variables*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Skewness</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>0.21</td>
</tr>
<tr>
<td>Sexual Prejudice</td>
<td>0.72</td>
</tr>
<tr>
<td>Subtle Discrimination</td>
<td>0.63</td>
</tr>
</tbody>
</table>
Figure G4. Linearity of RWA as shown through P-P plot of expected by observed scores.
Figure G5. Linearity of Sexual Prejudice as shown through P-P plot of expected by observed scores.
Figure G6. Linearity of Subtle Discrimination as shown through P-P plot of expected by observed scores.
**Homoscedacity**

*Figure G7.* Simple Scatter Plot of Sexual Prejudice by Right Wing Authoritarianism.

*Figure G8.* Simple Scatter Plot of Subtle Discrimination by Right Wing Authoritarianism.
### Equality of Variances between Condition

**Table G2.**

*Right Wing Authoritarianism Means by Condition*

<table>
<thead>
<tr>
<th>Condition</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>88.68</td>
<td>36.09</td>
</tr>
<tr>
<td>Straight Non-Ally</td>
<td>84.91</td>
<td>35.99</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>79.81</td>
<td>39.52</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>81.85</td>
<td>36.64</td>
</tr>
<tr>
<td>Male</td>
<td>85.99</td>
<td>36.11</td>
</tr>
<tr>
<td>Female</td>
<td>83.28</td>
<td>37.70</td>
</tr>
</tbody>
</table>

**Table G3.**

*Sexual Prejudice Means by Condition*

<table>
<thead>
<tr>
<th>Condition</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>2.32</td>
<td>1.13</td>
</tr>
<tr>
<td>Straight Non-Ally</td>
<td>2.43</td>
<td>1.27</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>2.24</td>
<td>1.12</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>2.21</td>
<td>1.07</td>
</tr>
<tr>
<td>Male</td>
<td>2.30</td>
<td>1.13</td>
</tr>
<tr>
<td>Female</td>
<td>2.28</td>
<td>1.14</td>
</tr>
</tbody>
</table>

**Table G4.**

*Subtle Discrimination (Budget Cuts) Means by Condition*

<table>
<thead>
<tr>
<th>Condition</th>
<th>Mean (in %)</th>
<th>SD (in %)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>22.9%</td>
<td>30.6%</td>
</tr>
<tr>
<td>Straight Non-Ally</td>
<td>36.7%</td>
<td>33.1%</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>33.1%</td>
<td>30.4%</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>37.7%</td>
<td>29.4%</td>
</tr>
<tr>
<td>Male</td>
<td>32.7%</td>
<td>30.8%</td>
</tr>
<tr>
<td>Female</td>
<td>32.6%</td>
<td>31.3%</td>
</tr>
</tbody>
</table>
Table G5

*Correlations Table*

<table>
<thead>
<tr>
<th></th>
<th>RWA</th>
<th>Sexual Prejudice</th>
<th>% Cut from HRC</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>Pearson Correlation</td>
<td>1</td>
<td>.461**</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>.000</td>
<td>.000</td>
</tr>
<tr>
<td>Sexual Prejudice</td>
<td>Pearson Correlation</td>
<td>.461**</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>.000</td>
<td>.000</td>
</tr>
<tr>
<td>% Cut from HRC</td>
<td>Pearson Correlation</td>
<td>.245**</td>
<td>.570**</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>.000</td>
<td>.000</td>
</tr>
</tbody>
</table>

**. Correlation is significant at the 0.01 level (2-tailed). N=382.
### Table G6

**Subtle Discrimination Measure Item Correlations Table**

<table>
<thead>
<tr>
<th></th>
<th>% Cut from HRC</th>
<th>% Cut from GLRI</th>
<th>% Cut from NWRF</th>
<th>% Cut from ADF</th>
<th>% Cut from USICH</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>% Cut from HRC</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pearson Correlation</td>
<td>1</td>
<td>-.169**</td>
<td>-.502**</td>
<td>-.079</td>
<td>-.552**</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td></td>
<td>.001</td>
<td>.000</td>
<td>.125</td>
<td>.000</td>
</tr>
<tr>
<td><strong>% Cut from GLRI</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pearson Correlation</td>
<td>-.169**</td>
<td>1</td>
<td>.021</td>
<td>-.254**</td>
<td>-.423**</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td></td>
<td>.001</td>
<td>.679</td>
<td>.000</td>
<td>.000</td>
</tr>
<tr>
<td><strong>% Cut from NWRF</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pearson Correlation</td>
<td>-.502**</td>
<td>.021</td>
<td>1</td>
<td>-.261**</td>
<td>.065</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td></td>
<td>.000</td>
<td>.679</td>
<td>.000</td>
<td>.204</td>
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<tr>
<td><strong>% Cut from ADF</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Pearson Correlation</td>
<td>-.079</td>
<td>-.254**</td>
<td>-.261**</td>
<td>1</td>
<td>-.226**</td>
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<tr>
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<td>.125</td>
<td>.000</td>
<td>.000</td>
<td>.000</td>
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<tr>
<td>N</td>
<td>382</td>
<td>382</td>
<td>382</td>
<td>382</td>
<td>382</td>
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<tr>
<td><strong>% Cut from USICH</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Pearson Correlation</td>
<td>-.552**</td>
<td>-.423**</td>
<td>.065</td>
<td>-.226**</td>
<td>1</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td></td>
<td>.000</td>
<td>.000</td>
<td>.204</td>
<td>.000</td>
</tr>
</tbody>
</table>

**Note.** Human Rights Campaign (HRC) is the LGBTQ+ organization on which budget cuts represent subtle discrimination. National Wildlife Refuge Fund (NWRF), African Development Fun (ADF), and US Interagency Council on Homelessness (USICH) are decoy organizations.

**Multicollinearity**

When all variables are entered into a regression model together, all VIF values are below 10.
### Homogeneity of Regression Slopes

Table G7

**Homogeneity of RWA and Sexual Prejudice Regression Slopes**

<table>
<thead>
<tr>
<th>Source</th>
<th>Type III Sum of Squares</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
<th>Sig.</th>
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</thead>
<tbody>
<tr>
<td>Corrected Model</td>
<td>107.402(^a)</td>
<td>7</td>
<td>15.343</td>
<td>14.640</td>
<td>.000</td>
</tr>
<tr>
<td>Intercept</td>
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<td>72.756</td>
<td>69.424</td>
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<tr>
<td>RWA_Score</td>
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<td>98.782</td>
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<tr>
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<td>.502</td>
<td>.479</td>
<td>.489</td>
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<td>Condtn_SexualOrientation</td>
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<td>.030</td>
<td>.029</td>
<td>.865</td>
</tr>
<tr>
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<td>.174</td>
<td>.166</td>
<td>.684</td>
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<td>Gender * RWA_Score</td>
<td>.204</td>
<td>1</td>
<td>.204</td>
<td>.195</td>
<td>.659</td>
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<tr>
<td>Condtn_SexualOrientation * RWA_Score</td>
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<td>1</td>
<td>.000</td>
<td>.000</td>
<td>.985</td>
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<td>1</td>
<td>.276</td>
<td>.263</td>
<td>.608</td>
</tr>
<tr>
<td>Error</td>
<td>390.906</td>
<td>373</td>
<td>1.048</td>
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<td>Total</td>
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<tr>
<td>Corrected Total</td>
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</tbody>
</table>

*Note.* IV interactions with RWA are non-significant, suggesting homogeneity of regression slopes.
Table G8

*Homogeneity of RWA and Subtle Discrimination Regression Slopes*

<table>
<thead>
<tr>
<th>Source</th>
<th>Type III Sum of Squares</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
<th>Sig.</th>
</tr>
</thead>
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<tr>
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<td>7</td>
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<tr>
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<td>.109</td>
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<td>Error</td>
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</table>

*Note.* IV interactions with RWA are non-significant, suggesting homogeneity of regression slopes.
Appendix H

Results of MANOVA to Rule Out Significance of Budget Cuts Decoy Items: Study 1

Table H1
Effects of Conditions on Cuts to Human Rights Campaign

<table>
<thead>
<tr>
<th>Condition</th>
<th>F</th>
<th>p</th>
<th>Partial η²</th>
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<tbody>
<tr>
<td>Participant Gender</td>
<td>.004</td>
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<td>.000</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
<td>8.14</td>
<td>.00</td>
<td>.021</td>
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<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
<td>2.71</td>
<td>.10</td>
<td>.007</td>
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</table>

Table H2
Effects of Conditions on Cuts to Great Lakes Restoration Initiative (Decoy Item)

<table>
<thead>
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<th>Condition</th>
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<th>p</th>
<th>Partial η²</th>
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</thead>
<tbody>
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<td>.18</td>
<td>.005</td>
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<td>Target’s Support for LGBTQ+ Issues</td>
<td>.425</td>
<td>.51</td>
<td>.001</td>
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Table H3
Effects of Conditions on Cuts to African Development Foundation (Decoy Item)

<table>
<thead>
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</thead>
<tbody>
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<td>Target’s Sexual Orientation</td>
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<td>.002</td>
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<td>Target’s Support for LGBTQ+ Issues</td>
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<td>.78</td>
<td>.000</td>
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Table H4
Effects of Conditions on Cuts to National Wildlife Refuge Fund (Decoy Item)

<table>
<thead>
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<th>Condition</th>
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<th>p</th>
<th>Partial η²</th>
</tr>
</thead>
<tbody>
<tr>
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<td>.001</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
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<td>.14</td>
<td>.006</td>
</tr>
<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
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<td>.11</td>
<td>.007</td>
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</table>
Table H5

Effects of Conditions on Cuts to US Interagency Council on Homelessness (Decoy Item)

<table>
<thead>
<tr>
<th>Condition</th>
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<th>p</th>
<th>Partial $\eta^2$</th>
</tr>
</thead>
<tbody>
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<td>.000</td>
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<tr>
<td>Target’s Sexual Orientation</td>
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<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
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<td>.95</td>
<td>.000</td>
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</table>
Appendix I

Analysis of Intergroup Anxiety as a Moderator: Study 1

Intergroup Anxiety was tested as a moderator of the relationship between participant gender and prejudice/discrimination, between target’s sexual orientation and prejudice/discrimination, as well as between target’s support for LGBTQ+ issues and prejudice/discrimination.

In the first analysis, intergroup anxiety score, participant gender, and an interaction term between anxiety and gender were entered as independent variables in two separate regression analyses (one for each dependent variable). This process was repeated for each independent variable and its relevant interaction term. Results showed that anxiety did not moderate the effect of any independent variable on prejudice or discrimination.

Anxiety X Gender on Sexual Prejudice, $B = .237$, $t(381) = 1.49$, $p = .135$

Anxiety X Gender on Subtle Discrimination, $B = .045$, $t(381) = 1.00$, $p = .316$

Anxiety X Sexual Orientation on Sexual Prejudice, $B = -.197$, $t(380) = -1.28$, $p = .20$

Anxiety X Sexual Orientation on Subtle Discrimination, $B = -.007$, $t(380) = -.16$, $p = .87$

Anxiety X Support on Sexual Prejudice, $B = .105$, $t(380) = .67$, $p = .50$

Anxiety X Support on Subtle Discrimination, $B = .022$, $t(380) = .49$, $p = .62$
Appendix J

Analysis of RWA as a Moderator: Study 1

RWA was tested as a moderator of the relationship between participant gender and prejudice/discrimination, between target’s sexual orientation and prejudice/discrimination, as well as between target’s support for LGBTQ+ issues and prejudice/discrimination.

In the first analysis, RWA, participant gender, and an interaction term between RWA and gender were entered as independent variables in two separate regression analyses (one for each dependent variable). This process was repeated for each independent variable and its relevant interaction term. Results showed that anxiety did not moderate the effect of any independent variable on prejudice or discrimination.

RWA X Gender on Sexual Prejudice, $B = .001$, $t(381) = .45$, $p = .64$

RWA X Gender on Subtle Discrimination, $B = .000$, $t(381) = .15$, $p = .87$

RWA X Sexual Orientation on Sexual Prejudice, $B = -1.3$, $t(380) = .000$, $p = 1.0$

RWA X Sexual Orientation on Subtle Discrimination, $B = .000$, $t(380) = .26$, $p = .75$

RWA X Support on Sexual Prejudice, $B = .001$, $t(380) = .52$, $p = .59$

RWA X Support on Subtle Discrimination, $B = .001$, $t(380) = 1.17$, $p = .24$
### Summary of Materials

Table K1

*Summary Comparison of Survey Materials in Study 1 Vs. Study 2*

<table>
<thead>
<tr>
<th>Stage of Study</th>
<th>Survey Items and Scales</th>
<th>Study 1</th>
<th>Study 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pre-screening survey</td>
<td>Education</td>
<td>Demographics (e.g., age, state, gender)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Sexual orientation/gender identity</td>
<td>Sexual orientation/gender identity</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Previous contact</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-test</td>
<td>Demographics</td>
<td>Previous direct contact, IOS</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Intergroup Anxiety</td>
<td>Previous extended contact, IOS</td>
<td></td>
</tr>
<tr>
<td></td>
<td>RWA scale</td>
<td>RWA scale</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>Sexual prejudice scale</td>
<td></td>
</tr>
<tr>
<td>Post-manipulation survey</td>
<td>Open-ended reflection</td>
<td>Open-ended reflection</td>
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<tr>
<td></td>
<td>Likeability</td>
<td>Likeability</td>
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</tr>
<tr>
<td></td>
<td>Sexual prejudice</td>
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</tr>
<tr>
<td></td>
<td>Budget cuts task</td>
<td>Budget cuts task</td>
<td></td>
</tr>
</tbody>
</table>
Appendix L

Pre-Screen Tool for Study 2

Demographics

1. How old are you? (open-ended)

2. What state do you live in currently? (drop-down menu)

3. What is your ethnicity? (drop-down menu)

4. What is the highest level of education you have completed?
   a) 8th grade (or less)
   b) some high school
   c) high school diploma (12th grade)
   d) vocational/trade school
   e) Some college/university
   f) Undergraduate college/university degree
   g) Master’s Degree (graduate school)
   h) Professional Degree (e.g., MD, DVM, DDS, PhD)

5. What is your gender? (open-ended)

6. To what extent is this true of you: I am a member of the LGBTQ+ community.
   *Note: Membership in the LGBTQ+ community means you identify as lesbian, gay, bisexual, transgender, queer, or any other sexual or gender diverse identity, that is not heterosexual.
   d) untrue of me*
   e) I don’t know
   f) true of me

*eligibility requirement
Appendix M
Study 2 Pre-Test

1. **Think of this ladder as representing where people stand in the United States.**
   At the top of this ladder are the people who are the best off – those who have the most money, the most education and the most respected jobs. At the bottom are the people who are the worst off – who have the least money, least education, and the least respected jobs or no job. The higher up you are on this ladder, the closer you are to the people at the very top; the lower you are, the closer you are to the people at the very bottom.

   **Survey utilized a ladder graphic on which participants could click “rungs” that represented where they felt they were located in relation to others.**

   **Religion Index**

2. How often do you attend church or other religious meetings?
   1=never; 2=once a year or less; 3=a few times a year; 4=a few times a month; 5=once a week; 6=more than once/week

3. How often do you spend time in private religious activities, such as prayer, meditation, or Bible study?
   1=never; 2=once a year or less; 3=a few times a year; 4=a few times a month; 5=once a week; 6=more than once/week

4. In my life, I experience the presence of the Divine (i.e., God).
   1=Definitely not true; 2=tends not to be true; 3=unsure; 4=tends to be true; 5=definite true of me

5. My religious beliefs are what really lie behind my whole approach to life.
   1=Definitely not true; 2=tends not to be true; 3=unsure; 4=tends to be true; 5=definite true of me

6. I try hard to carry my religion over into all other dealings in life.
   1=Definitely not true; 2=tends not to be true; 3=unsure; 4=tends to be true; 5=definite true of me
Previous Contact Questions

7. Do you **personally know anyone** who identifies as LGBTQ+?

*Note: An LGBTQ+ person is someone who identifies as lesbian, gay, bisexual, transgender, queer, or any other sexual or gender diverse identity, that is not heterosexual.

   i) Yes
   ii) No [if no, skip to Q 14]

8. a. Do you have any family members who identify as LGBTQ+, and what is their relationship to you? Check as many as are applicable.

   o I don’t have any family members who identify as LGBTQ+
   o Mother
   o Father
   o Sister
   o Brother
   o Uncle
   o Aunt
   o Cousin
   o Other extended family member

b. Do you have any friends who identify as LGBTQ+?

   i) Yes
   ii) No

c. If yes, how many friends do you have who identify as LGBTQ+?

d. Please consider the LGBTQ+ person you are closest with when answering this question.

   Please circle the picture below which best describes your relationship with that person. The more the circles overlap, the closer you are with that person.
e. Please indicate which person you are referring to in the diagram above:
(e.g., brother, best friend, colleague)__________.

9. Please indicate how frequently you interact with anyone who is LGBTQ+:
   i. never;
   ii. once a year or less;
   iii. 1 to 5 times a year;
   iv. anywhere from 6 times a year to once a month;
   v. anywhere from once a month to once a week;
   vi. more than once a week

10. We would like to know more about your social network; specifically, we want to know how many straight (or heterosexual) people you know who are friends with someone who is LGBTQ+.

   Scale for the following 5 questions:
   1=none;
   2=a few;
   3=about half;
   4=more than half;
   5=most or all

   a) How many straight people do you know who have friends who are LGBTQ+?
   b) How many of your straight friends have friends who are LGBTQ+?
   c) How many of your very best straight friends have friends who are LGBTQ+?
   d) How many straight members of your family (including parents, brothers and sisters, cousins, etc.) have friends who are LGBTQ+?
11. a) Of the straight people you recalled in the previous 5 questions, consider the person you’re closest with when answering this question. If you don’t know any straight people who have friends who are LGBTQ+, skip this question.

Please circle the picture below which best describes your relationship with that person. The more the circles overlap, the closer you are with that person.

b) What is your relationship to the person you are referring to in the diagram above? (e.g., brother, best friend, colleague)_________
RWA

This survey is part of an investigation of general public opinion concerning a variety of social issues. You will probably find that you agree with some of the statements, and disagree with others, to varying extents. Please indicate your reaction to each statement according to the following scale:

(-4) You very strongly disagree with the statement.
(-3) You strongly disagree with the statement.
(-2) You moderately disagree with the statement.
(-1) You slightly disagree with the statement.
(0) You feel exactly and precisely neutral about the statement.
(1) You slightly agree with the statement.
(2) You moderately agree with the statement.
(3) You strongly agree with the statement.
(4) You very strongly agree with the statement.

Important: You may find that you sometimes have different reactions to different parts of a statement. For example, you might very strongly disagree (“-4”) with one idea in a statement, but slightly agree (“+1”) with another idea in the same item. When this happens, please combine your reactions, and [record] how you feel on balance (a “-3” in this case).

1. The established authorities generally turn out to be right about things, while the radicals and protestors are usually just “loud mouths” showing off their ignorance.
2. Women should have to promise to obey their husbands when they get married.
3. Our country desperately needs a mighty leader who will do what has to be done to destroy the radical new ways and sinfulness that are ruining us.
4. Gays and lesbians are just as healthy and moral as anybody else.
5. It is always better to trust the judgment of the proper authorities in government and religion than to listen to the noisy rabble-rousers in our society who are trying to create doubt in people’s minds.
6. Atheists and others who have rebelled against the established religions are no doubt every bit as good and virtuous as those who attend church regularly.
7. The only way our country can get through the crisis ahead is to get back to our traditional values, put some tough leaders in power, and silence the troublemakers spreading bad ideas.

8. There is absolutely nothing wrong with nudist camps.

9. Our country needs free thinkers who have the courage to defy traditional ways, even if this upsets many people.

10. Our country will be destroyed someday if we do not smash the perversions eating away at our moral fiber and traditional beliefs.

11. Everyone should have their own lifestyle, religious beliefs, and sexual preferences, even if it makes them different from everyone else.

12. The “old-fashioned ways” and the “old-fashioned values” still show the best way to live.

13. You have to admire those who challenged the law and the majority’s view by protesting for women’s abortion rights, for animal rights, or to abolish school prayer.

14. What our country really needs is a strong, determined leader who will crush evil, and take us back to our true path.

15. Some of the best people in our country are those who are challenging our government, criticizing religion, and ignoring the “normal way things are supposed to be done.”

16. God’s laws about abortion, pornography and marriage must be strictly followed before it is too late, and those who break them must be strongly punished.

17. There are many radical, immoral people in our country today, who are trying to ruin it for their own godless purposes, whom the authorities should put out of action.

18. A “woman’s place” should be wherever she wants to be. The days when women are submissive to their husbands and social conventions belong strictly in the past.

19. Our country will be great if we honor the ways of our forefathers, do what the authorities tell us to do, and get rid of the “rotten apples” who are ruining everything.

20. There is no “one right way” to live life; everybody has to create their own way.

21. Homosexuals and feminists should be praised for being brave enough to defy “traditional family values.”

22. This country would work a lot better if certain groups of troublemakers would just shut up and accept their group’s traditional place in society.
Intergroup Anxiety Toward Homosexuals Scale

(1) Strongly Disagree;
(2) Disagree;
(3) Neither Agree Nor Disagree;
(4) Agree;
(5) Strongly Agree

1. I would feel nervous if I had to sit alone in a room with a gay person and start a conversation.
2. I just do not know what to expect from gay people.
3. Although I do not consider myself to be homophobic, I do not know how to present myself around gay people.
4. My lack of knowledge about the gay culture prevents me from feeling completely comfortable around gay people.
5. I can interact with gay people without experiencing much anxiety.
6. If I were at a party, I would have no problem with starting a conversation with a gay person.
7. It makes me uncomfortable to bring up the topic of homophobia around gay people.
8. I experience little anxiety when I talk to gay people.
9. The cultural differences between gay and straight people makes interactions between gay and straight people awkward.
10. I would experience some anxiety if I were the only straight person in a room full of gay people.
11. I worry about coming across as homophobic when I talk with gay people.
Sexual Prejudice Scale

Instructions: We’d now like to know your views on matters related to sexual preferences. Please read the following questions and answer them using the following scale.

(6) Strongly disagree  
(7) Disagree  
(8) Neither Agree Nor Disagree  
(9) Agree  
(10) Strongly agree

1. It’s wrong for men to have sex with men.  
2. Lesbians are confused about their sexuality.  
3. Marriage between two men should be kept illegal.  
4. It’s morally wrong to be a lesbian.  
5. A sexual relationship between two men is unnatural.  
6. Lesbians are harming the traditional family.  
7. Gay men are immoral.  
8. I disapprove of lesbians.  
9. I think it’s gross when I see two men who are clearly “together.”  
10. Marriage between two women should be legal.  
11. There’s nothing wrong with being a gay man.  
12. Being a lesbian is a normal expression of sexuality.  
13. Health care benefits should include partners of gay male employees.  
14. Lesbians want too many rights.  
15. Hospitals should allow gay men to be involved in their partners’ medical care.  
16. Lesbians should have the same civil rights as straight women.  
17. Retirement benefits should include the partners of gay men.  
18. Employers should provide retirement benefits for lesbian partners.  
19. Family medical leave rules should include the domestic partners of gay men.  
20. Employers should provide health care benefits to the partners of their lesbian employees.
Appendix N
Participant Religiosity

Figure N1. Frequency of attendance at religious services (e.g., Church)

Figure N2. Frequency of private worship (e.g., Prayer)
Appendix O
Internal Reliability of Scales for Study 2

Table O1

*Scale Reliabilities*

<table>
<thead>
<tr>
<th></th>
<th>Actual</th>
<th>Expected</th>
<th>Source</th>
</tr>
</thead>
<tbody>
<tr>
<td>RWA</td>
<td>$\alpha = .96$</td>
<td>$\alpha = .90$</td>
<td>Altemeyer, 2006</td>
</tr>
<tr>
<td>Sex Prej post</td>
<td>$\alpha = .97$</td>
<td>$\alpha = .90$</td>
<td>Chonody, 2013</td>
</tr>
<tr>
<td>Likeability</td>
<td>$\alpha = .92$</td>
<td>$\alpha = .91$</td>
<td>Reysen, 2005</td>
</tr>
</tbody>
</table>
Appendix P

Testing of statistical assumptions for study 2

Normality

Figure P1. Distribution of RWA scores.

Right Wing Authoritarianism Histogram

Mean = 69.18
Std. Dev. = 34.325
N = 806
Figure P2. Distribution of Sexual Prejudice Scores.

Figure P3. Distribution of Subtle Discrimination Scores.
### Skewness

**Table P1**

*Skewness coefficients for Variables of Interest*

<table>
<thead>
<tr>
<th>Variable</th>
<th>Skewness</th>
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<tbody>
<tr>
<td>RWA</td>
<td>0.41</td>
</tr>
<tr>
<td>Sexual Prejudice (Post)</td>
<td>1.15</td>
</tr>
<tr>
<td>Subtle Discrimination</td>
<td>0.72</td>
</tr>
</tbody>
</table>
Linearity

**Figure P4.** Linearity of RWA as shown through P-P plot of expected by observed scores.

**Figure P5.** Linearity of Sexual Prejudice as shown through P-P plot of expected by observed scores.
Figure P6. Linearity of Subtle Discrimination as shown through P-P plot of expected by observed scores.
**Homoscedasity**

*Figure P7.* Simple Scatter Plot of Subtle Discrimination by Right Wing Authoritarianism.

*Figure P8.* Simple Scatter Plot of Subtle Discrimination by Sexual Prejudice.
Figure P9. Simple Scatter Plot of RWA by Sexual Prejudice.
### Equality of Variances between Condition

Table P2

*Right Wing Authoritarianism Means by Condition*

<table>
<thead>
<tr>
<th>Condition</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>69.15</td>
<td>32.92</td>
</tr>
<tr>
<td>Straight Non-Allies</td>
<td>68.92</td>
<td>35.23</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>69.20</td>
<td>34.36</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>72.24</td>
<td>35.21</td>
</tr>
<tr>
<td>Gay Advocate Strong</td>
<td>72.00</td>
<td>33.59</td>
</tr>
<tr>
<td>Straight Ally Strong</td>
<td>63.66</td>
<td>34.52</td>
</tr>
<tr>
<td>Male</td>
<td>67.15</td>
<td>31.20</td>
</tr>
<tr>
<td>Female</td>
<td>70.70</td>
<td>36.45</td>
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</table>

Table P3

*Sexual Prejudice (pre-test) Means by Condition*

<table>
<thead>
<tr>
<th>Condition</th>
<th>Mean</th>
<th>SD</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>1.99</td>
<td>0.97</td>
</tr>
<tr>
<td>Straight Non-Allies</td>
<td>2.06</td>
<td>1.07</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>1.99</td>
<td>0.99</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>2.11</td>
<td>1.07</td>
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<tr>
<td>Gay Advocate Strong</td>
<td>2.12</td>
<td>1.10</td>
</tr>
<tr>
<td>Straight Ally Strong</td>
<td>1.99</td>
<td>1.00</td>
</tr>
<tr>
<td>Male</td>
<td>2.03</td>
<td>.93</td>
</tr>
<tr>
<td>Female</td>
<td>2.04</td>
<td>1.10</td>
</tr>
</tbody>
</table>
### Table P4

*Sexual Prejudice (post-test) Means by Condition*

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<th>SD</th>
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</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>1.79</td>
<td>0.88</td>
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<tr>
<td>Straight Non- Ally</td>
<td>1.90</td>
<td>0.99</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>1.83</td>
<td>0.92</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>1.88</td>
<td>0.95</td>
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<tr>
<td>Gay Advocate Strong</td>
<td>1.90</td>
<td>0.96</td>
</tr>
<tr>
<td>Straight Ally Strong</td>
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<td>0.89</td>
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<tr>
<td>Male</td>
<td>1.87</td>
<td>0.87</td>
</tr>
<tr>
<td>Female</td>
<td>1.83</td>
<td>0.97</td>
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</table>

### Table P5

*Subtle Discrimination (% Cut from HRC) Means by Condition*

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<thead>
<tr>
<th>Condition</th>
<th>Mean (in %)</th>
<th>SD (in %)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Gay Non-Advocate</td>
<td>25.38</td>
<td>29.16</td>
</tr>
<tr>
<td>Straight Non- Ally</td>
<td>24.04</td>
<td>27.21</td>
</tr>
<tr>
<td>Gay Advocate</td>
<td>24.01</td>
<td>28.61</td>
</tr>
<tr>
<td>Straight Ally</td>
<td>20.92</td>
<td>27.76</td>
</tr>
<tr>
<td>Gay Advocate Strong</td>
<td>29.79</td>
<td>30.78</td>
</tr>
<tr>
<td>Straight Ally Strong</td>
<td>24.06</td>
<td>24.98</td>
</tr>
<tr>
<td>Male</td>
<td>28.57</td>
<td>28.53</td>
</tr>
<tr>
<td>Female</td>
<td>21.81</td>
<td>27.57</td>
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</table>
### Correlations

**Table P6**

*Correlations Table*

<table>
<thead>
<tr>
<th></th>
<th>RWA</th>
<th>SexPrej(post)</th>
<th>% Cut from HRC</th>
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</thead>
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<tr>
<td><strong>RWA</strong></td>
<td>Pearson Correlation</td>
<td>1</td>
<td>.761**</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td>.000</td>
<td>.000</td>
<td></td>
</tr>
<tr>
<td><strong>SexPrej(post)</strong></td>
<td>Pearson Correlation</td>
<td>.761**</td>
<td>1</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td>.000</td>
<td>.000</td>
<td></td>
</tr>
<tr>
<td><strong>% Cut from HRC</strong></td>
<td>Pearson Correlation</td>
<td>.427**</td>
<td>.501**</td>
</tr>
<tr>
<td>Sig. (2-tailed)</td>
<td>.000</td>
<td>.000</td>
<td></td>
</tr>
</tbody>
</table>

**. Correlation is significant at the 0.01 level (2-tailed). N=806.**
<table>
<thead>
<tr>
<th></th>
<th>% Cut from HRC</th>
<th>% Cut from GLRI</th>
<th>% Cut from NWRF</th>
<th>% Cut from ADF</th>
<th>% Cut from USICH</th>
</tr>
</thead>
<tbody>
<tr>
<td>% Cut from HRC</td>
<td>Pearson</td>
<td>1</td>
<td>-0.296**</td>
<td>-0.458**</td>
<td>-0.140**</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>% Cut from GLRI</td>
<td>Pearson</td>
<td>-0.296**</td>
<td>1</td>
<td>-0.060</td>
<td>-0.123**</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>0.00</td>
<td>0.091</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>% Cut from NWRF</td>
<td>Pearson</td>
<td>-0.458**</td>
<td>-0.060</td>
<td>1</td>
<td>-0.265**</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>0.00</td>
<td>0.091</td>
<td>0.00</td>
<td>0.040</td>
</tr>
<tr>
<td>% Cut from ADF</td>
<td>Pearson</td>
<td>-0.140**</td>
<td>-0.123**</td>
<td>-0.265**</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>% Cut from USICH</td>
<td>Pearson</td>
<td>-0.405**</td>
<td>-0.433**</td>
<td>0.072</td>
<td>-0.307**</td>
</tr>
<tr>
<td></td>
<td>Sig. (2-tailed)</td>
<td>0.00</td>
<td>0.00</td>
<td>0.040</td>
<td>0.00</td>
</tr>
</tbody>
</table>

**. Correlation is significant at the 0.01 level (2-tailed). N=806.

Note. Human Rights Campaign (HRC) is the LGBTQ+ organization on which budget cuts represent subtle discrimination. National Wildlife Refuge Fund (NWRF), African Development Fun (ADF), and US Interagency Council on Homelessness (USICH) are decoy organizations.

**Multicollinearity**

When all variables are entered into a regression model together, all VIF values are below 10.
### Homogeneity of Regression Slopes

**Table P8**

*Homogeneity of RWA and Sexual Prejudice Regression Slopes*

<table>
<thead>
<tr>
<th>Source</th>
<th>Type III Sum of Squares</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
<th>Sig.</th>
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</thead>
<tbody>
<tr>
<td>Corrected Model</td>
<td>409.177a</td>
<td>9</td>
<td>45.464</td>
<td>124.619</td>
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</tr>
<tr>
<td>Intercept</td>
<td>27.566</td>
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<td>27.566</td>
<td>75.558</td>
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<tr>
<td>Gender</td>
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<td>1.211</td>
<td>3.321</td>
<td>.069</td>
</tr>
<tr>
<td>Condtn_SexualOrientation</td>
<td>.013</td>
<td>1</td>
<td>.013</td>
<td>.036</td>
<td>.850</td>
</tr>
<tr>
<td>Condtn_SupportLGBTrights</td>
<td>.020</td>
<td>2</td>
<td>.010</td>
<td>.028</td>
<td>.972</td>
</tr>
<tr>
<td>RWA_Score</td>
<td>366.880</td>
<td>1</td>
<td>366.880</td>
<td>1005.633</td>
<td>.000</td>
</tr>
<tr>
<td>Gender * RWA_Score</td>
<td>.264</td>
<td>1</td>
<td>.264</td>
<td>.724</td>
<td>.395</td>
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<tr>
<td>Condtn_SexualOrientation * RWA_Score</td>
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<td>.086</td>
<td>.236</td>
<td>.627</td>
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<tr>
<td>Condtn_SupportLGBTRights * RWA_Score</td>
<td>.152</td>
<td>2</td>
<td>.076</td>
<td>.208</td>
<td>.812</td>
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<tr>
<td>Error</td>
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<td>796</td>
<td>.365</td>
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<td></td>
</tr>
<tr>
<td>Total</td>
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<td>806</td>
<td></td>
<td></td>
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<tr>
<td>Corrected Total</td>
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<td>805</td>
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*Note.* IV interactions with RWA are non-significant, suggesting homogeneity of regression slopes.
Table P9

*Homogeneity of RWA and Subtle Discrimination Regression Slopes*

<table>
<thead>
<tr>
<th>Source</th>
<th>Type III Sum of Squares</th>
<th>df</th>
<th>Mean Square</th>
<th>F</th>
<th>Sig.</th>
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</thead>
<tbody>
<tr>
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<td>24.351</td>
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<tr>
<td>Intercept</td>
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<td>.004</td>
<td>.063</td>
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<td>Gender</td>
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<td>.003</td>
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<td>.830</td>
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<td>.035</td>
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<td>.035</td>
<td>.560</td>
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<td>.012</td>
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<td>.821</td>
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<td>RWA_Score</td>
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<td>.009</td>
<td>.137</td>
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<tr>
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<td>Total</td>
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<td>Corrected Total</td>
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</tbody>
</table>

*Note.* IV interactions with RWA are non-significant, suggesting homogeneity of regression slopes.
Appendix Q

Results of MANOVA to Rule Out Significance of Budget Cuts Decoy Items: Study 2

Table Q1

**Effects of Conditions on Cuts to Human Rights Campaign**

<table>
<thead>
<tr>
<th>Condition</th>
<th>F</th>
<th>p</th>
<th>Partial η²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participant Gender</td>
<td>10.8</td>
<td>.00*</td>
<td>.013</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
<td>3.82</td>
<td>.51</td>
<td>.005</td>
</tr>
<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
<td>1.24</td>
<td>.28</td>
<td>.003</td>
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</tbody>
</table>

Table Q2

**Effects of Conditions on Cuts to Great Lakes Restoration Initiative (Decoy Item)**

<table>
<thead>
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<th>Condition</th>
<th>F</th>
<th>p</th>
<th>Partial η²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participant Gender</td>
<td>5.99</td>
<td>.01*</td>
<td>.007</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
<td>.069</td>
<td>.79</td>
<td>.000</td>
</tr>
<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
<td>.247</td>
<td>.78</td>
<td>.001</td>
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</table>

Table Q3

**Effects of Conditions on Cuts to African Development Foundation (Decoy Item)**

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<th>F</th>
<th>p</th>
<th>Partial η²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participant Gender</td>
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<td>.08</td>
<td>.004</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
<td>.220</td>
<td>.63</td>
<td>.000</td>
</tr>
<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
<td>.596</td>
<td>.55</td>
<td>.001</td>
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Table Q4

**Effects of Conditions on Cuts to National Wildlife Refuge Fund (Decoy Item)**

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<th>Condition</th>
<th>F</th>
<th>p</th>
<th>Partial η²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participant Gender</td>
<td>2.28</td>
<td>.13</td>
<td>.003</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
<td>9.02</td>
<td>.00*</td>
<td>.001</td>
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<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
<td>2.09</td>
<td>.12</td>
<td>.005</td>
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</table>
Table Q5

*Effects of Conditions on Cuts to US Interagency Council on Homelessness (Decoy Item)*

<table>
<thead>
<tr>
<th>Condition</th>
<th>F</th>
<th>p</th>
<th>Partial $\eta^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participant Gender</td>
<td>.892</td>
<td>.34</td>
<td>.001</td>
</tr>
<tr>
<td>Target’s Sexual Orientation</td>
<td>.446</td>
<td>.50</td>
<td>.001</td>
</tr>
<tr>
<td>Target’s Support for LGBTQ+ Issues</td>
<td>.515</td>
<td>.59</td>
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Appendix R
Analysis of Intergroup Anxiety as a Moderator: Study 2

Intergroup Anxiety was tested as a moderator of the relationship between participant gender and prejudice/discrimination, between target’s sexual orientation and prejudice/discrimination, as well as between target’s support for LGBTQ+ issues and prejudice/discrimination.

In the first analysis, intergroup anxiety score, participant gender, and an interaction term between anxiety and gender were entered as independent variables in two separate regression analyses (one for each dependent variable). This process was repeated for each independent variable and its relevant interaction term. Results showed that anxiety did not moderate the effect of any independent variable on prejudice or discrimination.

Anxiety X Gender on Sexual Prejudice, $B = .251$, $t(805) = 3.10$, $p = .002$

Anxiety X Gender on Subtle Discrimination, $B = .05$, $t(805) = 1.86$, $p = .06$

Anxiety X Sexual Orientation on Sexual Prejudice, $B = -.10$, $t(805) = -1.3$, $p = .19$

Anxiety X Sexual Orientation on Subtle Discrimination, $B = .011$, $t(805) = .40$, $p = .68$

Anxiety X Support on Sexual Prejudice, $B = -.066$, $t(805) = -1.39$, $p = .16$

Anxiety X Support on Subtle Discrimination, $B = .005$, $t(805) = .29$, $p = .77$
Appendix S
Analysis of RWA as a Moderator: Study 2

RWA was tested as a moderator of the relationship between participant gender and prejudice/discrimination, between target’s sexual orientation and prejudice/discrimination, as well as between target’s support for LGBTQ+ issues and prejudice/discrimination.

In the first analysis, RWA, participant gender, and an interaction term between RWA and gender were entered as independent variables in two separate regression analyses (one for each dependent variable). This process was repeated for each independent variable and its relevant interaction term. Results showed that anxiety did not moderate the effect of any independent variable on prejudice or discrimination.

RWA X Gender on Sexual Prejudice, $B = .001, t(805) = .73, p = .46$
RWA X Gender on Subtle Discrimination, $B = -.001, t(805) = -1.98, p = .05$
RWA X Sexual Orientation on Sexual Prejudice, $B = -.001, t(805) = -.47, p = .63$
RWA X Sexual Orientation on Subtle Discrimination, $B = .001, t(805) = 1.67, p = .09$
RWA X Support on Sexual Prejudice, $B = .000, t(805) = .209, p = .83$
RWA X Support on Subtle Discrimination, $B = .000, t(805) = .60, p = .54$