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WHY CONFRONTING SEXISM WORKS:
APPLYING PERSUASION THEORIES TO CONFRONTING SEXISM

by

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University of Nebraska, 2011

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Speaking up about or confronting everyday prejudice creates more positive attitudes towards groups in the short-term. However, the mechanism underlying confrontation's prejudice reducing effect remains unclear. Because one goal of confronting prejudice is attitude change, a persuasion framework provides a theoretical model for research. Based on an integration of the confronting prejudice and persuasion literatures, I developed and tested three hypotheses about the effects of confrontation and elaboration of confrontation messages on observers' attitudes and behavior in the short- and longer-term. I expected that observing a confrontation (vs. no confrontation) reduces prejudice and discrimination; that elaborating on confrontation messages reduces prejudice and discrimination more than confrontation alone; and that elaborating on confrontation messages causes attitude change that lasts longer than confrontation alone. To test these hypotheses, participants were recruited to complete measures of sexism and feelings toward subtypes of women across three time points (i.e., pre-test, lab manipulation, and post-test). During the lab manipulation, participants imagined observing sexist jokes that were either confronted or not confronted. In addition, participants in confrontation conditions then wrote a control essay or an essay elaborating on the confrontation. Across these manipulations, there were three conditions to which 361 participants were randomly

assigned: no confrontation control, confrontation-only, or confrontation+elaboration. 1-14 days after the lab manipulation, 161 participants completed the post-test, which included a measure of discrimination, ostensibly as part of an unrelated study. Results indicated that observing a confrontation (vs. no confrontation) resulted in more positive feelings toward women and less discrimination in the short- and longer-term, but there was no significant reduction in sexism. Contrary to predictions, elaboration of confrontation messages did not reduce prejudice or discrimination more than confrontation alone. In addition, elaborated confrontation did not cause attitude change to last longer over time. Overall, this study suggests that confronting prejudice reduces prejudice and discrimination in observers in the longer-term but that this effect is not enhanced by elaborating on confrontation messages.

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CHAPTER 1

Prejudice Reduction

Prejudice is a concept that has captured the attention of social scientists for decades (Paluck & Green, 2009). Bias and negative attitudes toward social groups (i.e., prejudice) have been the subjects of definitional debates and theorizing, and studies focused on these topics have developed new methodologies in psychology. Most research on prejudice has focused on the nature, measurement, causes, and consequences of prejudice (Oskamp, 2000). For example, contemporary theories have conceptualized prejudice as more covert, subtle, and ambivalent (e.g., Dovidio & Gaertner, 1998; Glick & Fiske, 2001) than older theories of blatant prejudice (Monteith & Mark, 2009). Given the decrease in explicitly endorsed prejudice, social psychologists developed new measures of implicit bias, such as the Implicit Association Test (Greenwald, McGhee, & Schwartz, 1998). Research on causes of prejudice has examined how individual differences, such as those related to personality (e.g., authoritarianism and social dominance orientation; Sidanius & Pratto 1999) and motivation (e.g., internal and external regulation; Plant & Devine, 1998), relate to prejudice. Finally, the consequences of prejudice have been examined for both targets (e.g., anger and depression; Swim, Hyers, Cohen, & Ferguson, 2001) and observers (e.g., stereotyping and discrimination; Ford, Boxer, Armstrong, & Edel, 2008). However, the literature on prejudice has yet to successfully address the more practical question of how to reduce prejudice in real world settings and society more broadly.

Nearly 1,000 studies that have examined prejudice reduction, and most of these studies are either laboratory research lacking external validity or field research lacking

internal validity (Paluck & Green, 2009). As such, still more research is needed to understand the processes that reduce prejudice. Existing approaches to prejudice reduction can be divided into five categories, which range from individual- to group-level interventions: self-regulation, social categorization and identity, intergroup contact, education/training, and social norms and persuasion. Each is reviewed in order to provide background for the present study on prejudice reduction through confronting prejudice.

Self Regulation

Prejudice has an affective component (e.g., negative feelings toward groups), a behavioral component (e.g., discrimination), and a cognitive component (e.g., stereotypes). Unfortunately, cultural stereotypes (i.e., beliefs about an individual's characteristics based on group membership) can be automatically activated in the presence of cues – even among low-prejudice people who do not endorse stereotypes (Devine, 1989). This finding is a problem for the reduction of prejudice because activated stereotypes cause discriminatory behavior (e.g., Greenberg & Pyszczynski, 1985). In order to reduce prejudice and its components, people have to be aware of the potential for bias and then control that bias.

One way to control bias is through stereotype suppression, in which people avoid thinking about cultural stereotypes. However, thought suppression often has the paradoxical effect of making that thought more accessible (Wegner, 1994). The underlying process is two-fold—people search their minds for the presence of the unwanted thought and, if it is present, replace the stereotype with a distractor thought. Ironically, this increased awareness and monitoring of thoughts makes the thought more accessible. Applied to stereotyping, the process of monitoring for stereotypes decreases

stereotyping on an immediate task but increases stereotyping on later tasks, which has been called stereotype rebound (e.g., Macrae, Bodenhausen, Milne, & Jetten, 1994). Stereotype rebound occurs when the admonishment to avoid stereotypes has been relaxed, but the stereotype remains primed, accessible, and memorable (Macrae, Bodenhausen, Milne, & Wheeler, 1996) because of the previous monitoring process. The result is that the stereotype “floods” the mind and influences later behavior.

The tendency for stereotype rebound means that controlling prejudice through stereotype suppression does not reduce prejudice and discrimination in the long-term; however, people differ in their susceptibility to stereotype rebound. Research has replicated the stereotype rebound conditions while examining whether participants’ level of prejudice moderated the effect. People who have less (vs. more) prejudice should be particularly motivated to avoid bias because of their personal commitment to avoiding bias. Consistent with this notion, high-prejudice participants showed increased stereotype accessibility after stereotype suppression but low-prejudice participants did not (Monteith, Spicer, & Tooman, 1998). In fact, additional research suggests that people can control prejudice given the motivation and ability to do so.

Self-regulation is a construct that involves setting goals and working towards them. An approach to individual prejudice reduction that is consistent with this construct would involve setting and working toward an egalitarian goal, which differs from stereotype suppression. Stereotype suppression involves the avoidance of non-egalitarian thoughts/behavior, whereas self-regulation involves the approach of egalitarian thoughts/behavior. Still, both processes require motivation and ability. Although people are susceptible to automatic activation of stereotypes regardless of their personal

prejudice level, low-prejudice people have more motivation to avoid prejudice and respond in egalitarian ways than high-prejudice people do (e.g., Devine, 1989). However, high-prejudice people might also be motivated to avoid appearing prejudiced if they are high in external motivation to control prejudice (i.e., respond to social pressure to avoid prejudice; Plant & Devine, 1998).

In some ways, self-regulation of prejudice is similar to Festinger's cognitive dissonance theory (1957). Cognitive dissonance occurs when one recognizes a discrepancy between an attitude and behavior, which causes discomfort. As a result, the individual changes either the attitude or behavior in order to gain consistency between attitude and behavior. Similarly, when a low-prejudice person recognizes discriminatory behavior that is in conflict with his/her egalitarian attitudes, it causes discomfort. Discomfort then causes the person to change later behavior to be consistent with his/her egalitarian attitudes, thus self-regulating prejudice.

Similarly, Monteith and Mark (2005) suggest that self-regulation of prejudice begins when an individual becomes aware of the discrepancy between a prejudiced response (e.g., the use of an automatically activated stereotype) and one's personal egalitarian standards. This discrepancy causes behavioral inhibition (e.g., interruption of the response), negative self-directed affect (e.g., guilt), and reflection on the factors leading to the discrepant response, which leads to the development of cues for control of future behavior. Once an individual has identified cues related to their discrepant response, future situations in which those cues are present should cause behavioral inhibition as well as prospective reflection (e.g., careful consideration of response options) should occur in future situation in which those cues are present. These processes

then inhibit discrimination and generate non-prejudiced response options (Monteith & Mark, 2009).

This Self-Regulation of Prejudice Model (Monteith & Mark, 2005) has received empirical support. For example, participants who reported greater discrepancies between how they *would* and *should* (i.e., based on personal standards) respond to prejudiced jokes experienced more negative self-directed (Monteith & Voils, 1998). Behavioral inhibition has also been demonstrated following discrepancies. For example, participants in one study were connected to physiological equipment, shown pictures, and asked to press a key to continue to the next picture. Participants then received bogus feedback that indicated they had a negative reaction to pictures of racial minorities. Consistent with the notion that awareness of prejudiced responses that are discrepant with one's beliefs causes behavioral inhibition, participants given this feedback subsequently took longer to press the key to go to the next picture for pictures of minorities (Monteith, Ashburn-Nardo, Voils, & Czopp, 2002). Thought listing has been used to study whether prejudiced behavior-belief discrepancy also leads to retrospective reflection. Participants who believed they had negative or discriminatory responses to disadvantaged groups showed a preoccupation with their responses (e.g., listing more thoughts relevant to the racial minority pictures; Monteith et al., 2002; see also Monteith, 1993). Finally, there also is support for the notion that awareness of discrepancy and the resulting processes (i.e., behavioral inhibition, negative self-directed affect, and retrospective reflection) improves future self-regulation (i.e., through behavioral inhibition and prospective reflection). For example, participants who received bogus feedback indicating prejudice later took longer to evaluate prejudiced jokes and evaluated prejudiced jokes more negatively in an

ostensibly unconnected study compared to participants not given this feedback (Monteith, 1993; Study 2).

However, the problem still remains that many people are not motivated or lack the ability to control prejudice. Self-regulatory processes are not likely to be effective for high-prejudice individuals because they lack the motivation to behave in egalitarian ways. Additionally, self-regulation requires the ability to avoid prejudiced responding, which can be limited in certain situations. For example, when people lack cognitive resources (i.e., under conditions of high cognitive load), even low-prejudice people are unable to regulate their prejudiced responses (Monteith & Voils, 1998). This limitation of the self-regulation approach to prejudice reduction suggests that other means of reducing prejudice are necessary.

Intergroup Contact

Intergroup contact or interaction between different social groups is another means through which prejudice reduction is achieved. However, Allport (1954) noted that intergroup contact might either increase or reduce prejudice. According to his contact hypothesis/theory, certain features of intergroup contact are necessary to reduce prejudice and conflict. Specifically, the contact situation must involve equal status, cooperation, common goals, and institutional and social support. For example, researchers in the classic Robbers Cave field study (Sherif, Harvey, White, Hood, & Sherif, 1961) constructed situations in which there was competition between young campers, which led to prejudice and hostility between the two camps. The researchers were able to reduce prejudice, however, through situations in which the camps worked cooperatively toward common goals. Further, the campers were of equal status, and there was institutional

support for the campers' interactions. Thus, the Robbers Cave study illustrates each of the features of intergroup contact that may successfully reduce prejudice.

In another compelling study that combines the benefits of laboratory and field research, White participants in the American South in the 1960s were hired to work on a railroad supervising a Black confederate and a White confederate (Cook, 1971, 1978). The working conditions over the month-long experiment had each of the features of intergroup contact that Allport (1954) specified. Consistent with the contact hypothesis, results showed that participants who had worked with a Black confederate were less prejudiced than control participants were several months later.

Other research also has supported Allport's (1954) features for successful intergroup contact. For example, the Jigsaw classroom technique has been well-studied and involves cooperative learning (Aronson, Blaney, Stephan, Sikes, & Snapp, 1978; see also Slavin, 1990). Specifically, this teaching technique requires each student to be responsible for a subtopic. The student is then the subtopic expert and must share this information with their group to meet the learning objective. This technique involves equal status students (i.e., who each have a piece of information to share) working cooperatively toward a common goal with institutional support. As such, it is not surprising that cooperative learning techniques have been shown to reduce prejudice (Pettigrew & Tropp, 2000; Walker & Croghan, 1998) as well as increase cross-group friendships (i.e., across race, gender, and achievement; Singh, 1991). More broadly, a meta-analysis of 515 studies found that the intergroup contact reduces prejudice (mean $r = -.22$); however, intergroup contact reduces prejudice to a greater degree when the

features Allport (1954) specified were present (mean $r = -.29$) than when they were absent (mean $r = -.20$; Pettigrew & Tropp, 2006).

Contemporary research has added more features of intergroup contact that reduce prejudice, including potential for acquaintance and friendship (Pettigrew, 1997, 1998). For example, a longitudinal study of college students' friendship showed that having more outgroup friends decreased prejudice years later (Levin, Van Laar, & Sidanius, 2003). In fact, knowing that another ingroup member has an outgroup friend reduces prejudice toward that outgroup (i.e., extended contact hypothesis; Wright, Aron, McLaughlin-Volpe, & Rop, 1997; see also Turner, Hewstone, Voci, Paolini, & Christ, 2007). Another longitudinal study of intergroup friendship indicated that contact reduces prejudice but also that prejudice reduces contact (Binder, Zagefka, Brown, Funke, Kessler, Mummendey, Muquill, et al., 2009). This study also supported Allport's (1954) features of intergroup contact, because the quality of intergroup friendships influenced prejudice more than the quantity of intergroup friendships. Specifically, high quality friendships featuring equal status and cooperation were associated with less prejudice.

Although evidence across experimental, field, and longitudinal studies indicates that intergroup contact reduces prejudice (e.g., Pettigrew & Tropp, 2006), people avoid intergroup interactions if given the opportunity (Plant & Devine, 2003). Intergroup interactions cause anxiety and discomfort because people have negative expectations about the outcome of these interactions (Dovidio, Kawakami, & Gaertner, 2002). An additional problem is that intergroup contact is not always possible or easily achieved (e.g., concealable stigma or lack of a member of a specific minority group in the area). Finally, intergroup contact cannot explain the continuation of sexism, because most

people have contact with both men and women. For these reasons and others, researchers have examined the processes underlying intergroup contact that cause prejudice reduction.

Social Categorization and Identity

People have a tendency to categorize others based on their group memberships. Ingroup members share a characteristic with an individual (i.e., leading one to share a social identity with ingroup members) whereas outgroup members differ on that characteristic. However, categorization exaggerates differences between in- and outgroups as well as the similarity within the outgroup (i.e., outgroup homogeneity effect; Judd & Park, 1988; Judd, Ryan, & Park, 1991). People also favor their ingroup and derogate the outgroup (e.g., Brewer & Brown, 1998). In fact, ingroup favoritism and outgroup derogation can be activated even for arbitrary, meaningless categories randomly assigned to participants in the lab (i.e., minimal groups paradigm; Tajfel & Turner, 1986). Allport was the first to argue that the process underlying prejudice involved simple group categorization (1954), and contemporary research on prejudice reduction has examined ways to combat categorization in order to reduce prejudice.

There are a variety of ways to change how people categorize themselves and others, and changes to categorization can reduce prejudice. The most commonly examined category-based approaches to reduce prejudice include decategorization, group differentiation, and recategorization (Dovidio & Gaertner, 2010). Decategorization decreases the salience of social categories by increasing the focus on individuals rather than groups. For example, the exchange of individuating information undermines outgroup stereotypes (Brewer & Miller, 1984), and personalized intergroup contact

focused on persons rather than tasks reduces prejudice (Miller, Brewer, & Edwards, 1985).

However, eliminating group boundaries can sometimes cause resistance and increased prejudice (Dixon, Tropp, Durrheim, & Tredoux, 2010; Jetten, Spears, & Postmes, 2004). The Mutual Intergroup Differentiation Model (Hewstone & Brown, 1986) instead posits that prejudice could be reduced by maintaining group categorization in a cooperative, intergroup setting. Similar to multicultural approaches to intergroup relations, differences between groups are recognized and valued in group differentiation approaches to categorization. The goal is to have both groups recognize differences and how these differences might benefit both groups through collaborative work toward a common goal. This process of maintaining group distinctiveness while highlighting interdependence decreases intergroup threat as well as prejudice (e.g., Brown & Wade, 1987; see also Brown & Hewstone, 2005). Consistent with the Mutual Intergroup Differentiation Model, meta-analysis indicates that intergroup contact has been shown to reduce prejudice through increased outgroup knowledge and reduced intergroup anxiety (Pettigrew & Tropp, 2008). However, a longitudinal study examining whether intergroup anxiety mediates intergroup friendships and prejudice reduction found only partial mediation, which suggests that intergroup anxiety as well as other factors (e.g., empathy; Pettigrew & Tropp, 2008) influence prejudice reduction (Binder et al., 2009).

Recategorization differs from decategorization, (i.e., eliminating category boundaries) and mutual group differentiation (i.e., maintain category boundaries in a cooperative setting) because it creates a superordinate category or identity that overrides the distinctions between ingroups and outgroups. The Common Ingroup Identity Model

(Gaertner & Dovidio, 2000) posits that prejudice is reduced if ingroups and outgroups are recategorized into a larger, more inclusive group (e.g., recategorizing “Jewish” to “human” identity; Wohl & Branscombe, 2005). This process harnesses the power of ingroup favoritism in order to reduce prejudice. The positive evaluations and feelings normally reserved for a narrowly defined ingroup can be applied to a broader, superordinate group that is inclusive of both groups. In other words, what was “us” and “them” becomes “we” through recategorization (Gaertner, Dovidio, & Houlette, 2010). Consistent with this model, common ingroup identity has been shown to reduce prejudice for majority members (Smith & Tyler, 1996) as well as students in a multiethnic high school (Gaertner, Rust, Dovidio, Bachman, & Anastasio, 1996) and executives in a corporate merger (Bachman, 1993). Similarly, students with dual identities including a superordinate category (e.g., Korean and American) had less prejudice than students with a single identity did (e.g., Korean; Gaertner et al., 1996). Given the success of these category- and identity-based approaches, they would be a welcome addition to education and training that seeks to reduce prejudice, which I turn to next.

Education and Training

Several educational approaches may reduce prejudice, including multicultural and anti-bias education, social-cognitive skills training, and perspective taking interventions. According to a survey of American school districts, nearly half use a multicultural education program (Washburn, 1996). Multicultural education in schools emphasizes tolerance and/or contributions of disadvantaged groups (Aboud & Levy, 2000). For example, reading about African American historical figures’ experiences and contributions reduced majority students’ prejudice (Hughes, Bigler, & Levy, 2007).

Multicultural programs featuring positive stories and media about minorities are not always successful (Lessing & Clarke, 1976; Litcher & Johnson, 1969; Litcher, Johnson & Ryan, 1973; Yawkey, 1973). However, longer interventions that involve reading about cross-group friendships followed by discussion seem to reduce prejudice more effectively than multicultural literature alone (Cameron & Rutland, 2006; Paluck & Green, 2009).

Anti-bias education with older students and adults often involves discussion, which may be more effective than simple exposure to readings or media (Aboud & Levy, 2000; Fisher, 1968; Slavin & Madden, 1979). Similar to research on self-regulation, anti-bias education draws on self-insight. That is, it encourages students to recognize the potential for bias, which may lead to feelings of guilt and increased vigilance to control prejudice. Across several studies, anti-racism education has been shown to reduce prejudice in students (McGregor, 1993), but it can backfire and increase prejudice in adults (Kehoe & Mansfield, 1993). Diversity training is the umbrella term for educational, anti-bias programs and/or activities that are conducted in the workplace. Although most American employers use diversity training, most diversity training is not informed by social science theories or research and has not been evaluated using social science methods (Paluck, 2006).

Other interventions to reduce prejudice focus on social-cognitive skills, with the goal of changing schemas and categorization processes that underlie prejudice. For example, some interventions combat outgroup homogeneity by having students learn individuating information about fictional students from outgroups (e.g., Katz & Zalk, 1978), which reduced prejudice amongst formerly high-prejudice students (Aboud & Fenwick, 1999). Similarly, interventions that highlight the malleability of personality also

reduce outgroup homogeneity and stereotyping (e.g., Levy & Dweck, 1999; Levy, Strossner, & Dweck, 1998). Finally, a week-long intervention featuring a task in which students practiced thinking in counter-stereotypic ways by classifying pictures according to gender and occupation reduced stereotyping (Bigler & Liben, 1992, 1993).

Perspective taking interventions that increase outgroup empathy can also reduce prejudice. These interventions typically involve role-playing to gain understanding of minority experiences, which encourages empathy and reduces prejudice toward the outgroup. The most well known example of a perspective taking intervention is the Blue-Eyes/Brown-Eyes exercise, which was devised by a third-grade teacher named Jane Elliot in the 1960s. Elliot assigned students to two groups based on eye color and showed preferential treatment to one group on the first day and the other group on the second day. Thus, all students temporarily were members of a devalued group. Variations of this exercise have been examined in the laboratory and field, and results indicate that it increases intentions for outgroup interactions (Weiner & Wright, 1973; Breckheimer & Nelson, 1976).

Even less-intensive perspective taking interventions can reduce prejudice, however. For example, participants asked to take the point of view of the stigmatized person by imagining the person's feelings (vs. taking an objective point of view) had less prejudice toward the stigmatized group (Batson, Polycarpou, Harmon-Jones, Imhoff, Mitchener, & Bednar, 1997). Similarly, participants who wrote an essay from the perspective of a disadvantaged group member showed greater empathy and decreased stereotyping and prejudice (Galinsky & Moskowitz, 2000; Vescio, Sechrist, & Paolucci, 2003; see also Stephan & Finlay, 1999). However, perspective taking can interrupt

intergroup interactions, leading to more negative evaluations of majority group members by minority group members after an interaction (Vorauer, Martens, & Sasaki, 2009).

Next, I turn to more broad interventions that may reduce prejudice.

Social Norms and Influence

Finally, social norms can be communicated and reduce prejudice through a variety of mediums (e.g., media and peer influence). Media includes readings, advertisements, television, and movies, and each can convey and influence social norms. For example, *Sesame Street* and other high quality, multicultural children's programming increases tolerance in children (Browne Graves, 1999; Mays, Henderson, Seidman, & Steiner, 1975). However, social scientists have rarely examined media's large-scale impact over time or audiences. (For a review, see Paluck & Green, 2009.)

One exception is a randomized field experiment that examined beliefs, behavior, and perceived norms one year after the introduction of an educational Rwandan radio soap opera (Paluck, 2009). The radio program featured two communities' struggle with conflict, and it was designed to facilitate reconciliation between the Tutsi and Hutu peoples following the Rwandan genocide in the 1990s. Although personal beliefs were not affected, perceived norms and behavior became more egalitarian for participants who listened to the reconciliation broadcast compared to a control broadcast. As Paluck argues, it may be that educational media has a larger influence on perceived norms than beliefs; still, perceived norms affect behavior. In this case, an educational campaign created more egalitarian behavior by changing perceived norms.

Friends, peers, and others in our social world can also influence the expression of and belief in prejudice. For example, participants who are told that stereotyping is rare

among their peers (vs. control participants) have less prejudice (Stangor, Sechrist, & Jost, 2001). Witnessing a peer give a low prejudice response also reduces prejudice—most likely by establishing an egalitarian norm (Blanchard, Crandall, Brigham, & Vaughan, 1994; Blanchard, Lilly, & Vaughn, 1991; Monteith, Deneen, & Tooman, 1996). Finally, friends and acquaintances of students trained to speak out against instances of everyday prejudice (vs. wait list controls) engaged in more anti-prejudice behavior (Paluck, 2011).

Implications and Conclusion

Clearly, this review suggests that there is no antidote for prejudice. Each approach to reducing prejudice has advantages and disadvantages. As such, a multi-pronged approach involving a variety of the five approaches is likely to be most effective at reducing prejudice. However, research on prejudice reduction through social influence highlights that there are simple things that everyone can do to reduce prejudice in society.

As indicated in the literature review that follows, speaking up about everyday prejudice (i.e., confronting prejudice) can reduce prejudice. Although the mechanisms behind this effect are not well understood, there are a variety of ways that confronting prejudice might involve the broader processes of prejudice reduction. Confronting prejudice might trigger self-regulation of prejudice, for example. Confronting prejudice brings awareness to prejudice, which may encourage behavioral inhibition, negative self-directed affect, and reflection. According to the Self-Regulation of Prejudice Model (Monteith & Mark, 2005), these processes serve to develop cues for control to avoid future prejudiced behavior.

Confronting prejudice might also be related to research on the effects of intergroup contact. Research on the contact hypothesis has highlighted the importance of

high quality interactions. The features of contact that Allport (1954) specified as related to prejudice reduction might also influence the outcomes of confronting prejudice. For example, confrontation that occurs between people of equal status might reduce prejudice more than confrontations within hierarchical relationships. Further, perhaps confrontation messages highlighting cooperation, common goals, and institutional and social support may increase confrontation's ability to reduce prejudice.

Similarly, confronting prejudice may be related to research on social categorization interventions through the appeals that confronters present to perpetrators. For example, a confronter might attempt to reduce prejudice by arguing that individuals vary more than groups (i.e., a decategorization message), that differences between groups are mutually beneficial (i.e., a group differentiation message based on the Mutual Intergroup Differentiation Model; Brown & Hewstone, 2005), or that there is a superordinate category inclusive of both in- and outgroups (i.e., a recategorization message based on the Common Ingroup Identity Model; Gaertner & Dovidio, 2000). Thus, confronting prejudice builds on many of the broader approaches taken to reduce prejudice, and understanding the effect of confronting prejudice may bolster understanding of a variety of prejudice reduction techniques.

CHAPTER 2

Literature Review on Confronting Prejudice

Prejudiced statements are frequent and have negative effects on both targets and observers. For example, sexist interpersonal encounters (e.g., gender stereotyping, derogatory comments, and sexual objectification) occur as much as once a day (Swim et al., 2001). These experiences have cumulative negative effects for targets, including anger, depression, and decreased self-esteem, even when targets are uncertain whether prejudice motivated the event (Swim et al.). For observers, hearing prejudiced statements or derogatory labels activates stereotypical associations (Carnaghi & Maass, 2007) that are likely to influence attitudes and behavior (Bargh, Chen, & Burrows, 1996; Wheeler & Petty, 2001). Hearing a racial slur, for example, negatively influenced participant evaluations of an ethnic minority's skill (Greenberg & Pyszczynski, 1985). Similarly, humor in which a group is characterized negatively serves as a releaser for prejudice. A study examining these issues showed that being exposed to a sexist joke led men who were high on hostile sexism to discriminate against women (Ford et al., 2008). Research in a burgeoning area in psychology, confronting prejudice, may provide ways to counter the negative effects of prejudice experienced by both targets and observers.

Confronting prejudice has been defined in past research as individuals' assertive responses to bias, which involves letting their distaste for the bias be known to others (Shelton, Richardson, Salvatore, & Hill, 2006). Specifically, the confrontation must involve an objection to the bias in the perpetrator's original message. According to interviews and diary data of women who confronted a variety of prejudices, the most common goal when people confront prejudice is educating the perpetrator in an effort to

reduce the perpetrator's prejudice (Hyers, 2007). Participants described other motivations for confronting prejudice besides perpetrator attitude or behavior change, including self-validation (e.g., improved mental health or victim empowerment; Chin, Czopp, & Hovey, 2009; Shelton et al.) and impression management. However, the present research focuses on the most common goal of confronting, which is attitude, behavior, and/or norm change leading to prejudice reduction.

Research on confronting prejudice has mostly lacked a theoretical basis and has instead examined various factors concerning the perpetrator's prejudiced message and the confronter's message. This research has examined two different aspects of confrontation: the *causes* of confronting prejudice (i.e., When does confronting occur?) and the *outcomes* of confronting prejudice (i.e., What is the effect of confronting?). As shown in Figure 1, the causes and outcomes of confronting prejudice can be seen as separate processes that are influenced by different variables, which I discuss in turn. Although a recent theoretical model has been applied to when people confront prejudice (Ashburn-Nardo, Morris, & Goodwin, 2008), no model has been proposed to explain the conditions relating to the outcomes of confronting prejudice.

In the present review, I integrate the recent work on confronting prejudice into existing social psychological theory. The focus of the review is the variables that influence the outcomes of confronting prejudice on perpetrators as well as observers. The effect of confrontation on perpetrators has been the focus of most confrontation research (Czopp & Monteith, 2003; Czopp, Moneith, & Mark, 2006); however, observers are also influenced by confrontations (e.g., Hillard & Ryan, 2011; Rasinski & Czopp, 2010). When asked to recall incidents of prejudice, nearly 82% of participants recalled a

situation in which they were observers of prejudice (i.e., third-party bystanders; Ashburn-Nardo, Goodwin, & Morris, 2009), which presents an opportunity for confrontation to influence observers. Fennimore (1994) claims that “Hearing a person make a prejudiced comment is one common experience that provides *everyone* with an opportunity to express commitment to human equality and thus have an immediate positive effect on social perceptions and behaviors” (pp. 202). However, the effect of confrontation on observers is not fully understood. In my review of the literature on confronting prejudice, I argue that a persuasion framework is useful for examining the outcomes of confronting prejudice, because it accounts for both low and high effort processes leading to attitude change in perpetrators as well as observers. First, however, I begin by presenting research and models on the causes of confronting prejudice.

Causes of Confronting Prejudice

The research that examines the causes of confrontation can be organized into the individual factors, situational factors, and barriers that influence whether confrontation occurs, as shown in Figure 2. In terms of personal factors, research has shown that those with activist goals or orientations are more likely to confront prejudice (e.g., Hyers, 2007; Swim & Hyers, 1999). For example, those who identify as feminists (Ayers, Friedman, & Leaper, 2009) or have a personal commitment to ending gender discrimination (Pratt-Hyatt, 2008) are more likely to confront sexism. People who are more communally oriented (vs. exchange oriented) are also more likely to confront prejudice (Gervais, Hillard, & Vescio, 2010). People who expect confrontation to be successful (e.g., reduce prejudice) are more likely to confront prejudice (Hillard, 2011; Hyers, 2007). For example, confrontation is more likely when people believe that personality is malleable

(i.e., that others can change) rather than fixed (Rattan & Dweck, 2010). Similarly, optimists generally expect positive outcomes and thus are more likely to confront prejudice than pessimists are (Sechrist, 2010; Wellman, Czopp, & Geers, 2009).

Another individual factor is prior experience with confronting prejudice. Multiple studies have examined ways to increase confronting responses from children and adults, including skills training and role-play. Several studies aimed to increasing students' confrontations concerning prejudice and bullying through interventions (e.g., Aboud & Joong, 2007; Lamb, Bigler, Liben & Green, 2009; Paluck, 2011). For example, students who received training and practiced confronting prejudice through role-play were more likely than wait-list students to later be nominated by peers as someone who confronts prejudice. Another study showed that having experience (vs. hearing stories about) confronting prejudice increases confronting in the short-term and six months later (Lamb et al., 2009). Other evidence indicates that adults also are more likely to confront prejudice if they have practiced doing so (e.g., Lawson, McDonough, & Bodle, 2010; Plous, 2000).

In terms of situational factors, women's likelihood of confronting depended on the type of sexism and relationships with the perpetrator (Ayers et al., 2009). Women were more likely to confront sexism when it involved sexist comments rather than discriminatory behavior or sexual harassment. Women were also more likely to confront familiar, equal status perpetrators than unfamiliar and/or high status perpetrators. A broader situational factor is the context or audience present for a confrontation. Targets who have solo or "token" status in the group are more likely to confront prejudice (vs. having other targets present; Swim & Hyers, 1999). In fact, majority members often look

to a minority target in response to offensive statements (Crosby, Monin, & Richardson, 2008).

There are several existing theoretical models or theories that are applicable to confronting prejudice. Two of these models come from literature on sexual harassment. Reporting sexual harassment and confronting prejudice are similar in that both are behaviors a complainant (i.e., the person who reports sexual harassment or the confronter) might engage in following unequal treatment. Knapp, Ekeberg, and Dubois's (1997) model for responses to workplace sexual harassment includes antecedents (individual characteristics, power, legal/economic environment, work group characteristics, and organizational characteristics) relating to predictor variables (reporting process, outcome expectancy, severity of sexual harassment, and level of distress), which finally relate to whether sexual harassment is reported by the victim. Although some aspects of the model are specific to reporting sexual harassment (e.g., reporting process), other aspects are similar to what the participants report across studies of individual and situational factors that influence prejudice (see also Hillard, 2011).

Bystander intervention models have been applied to both reporting sexual harassment (Bowes-Sperry & O'Leary-Kelly, 2005) and confronting prejudice. According to the Confronting Prejudiced Responses Model (Ashburn-Nardo et al., 2008), there are five loose steps involved in confronting prejudice, based on Latane and Darley's (1970) work on bystander intervention. Ashburn-Nardo et al. specify that steps in the process may be skipped, which is more likely when anger is involved. They suggest that their model can be taken as identifying common hurdles in confronting prejudice, when the decision to confront prejudice is made in a systematic manner. The model indicates

that individuals confront prejudice if and when they determine that the event is discriminatory and an “emergency” (i.e., an unexpected, harmful event that requires immediate attention). Next, individuals confront prejudice if they take responsibility, identify a response, and take action (Ashburn-Nardo et al.).

There are many challenges at each of these steps. The target of the prejudice likely influences whether an event is defined as discriminatory, which is the first step in the Confronting Prejudice Responses Model. For example, racism is more likely than other forms of bias to be seen as discriminatory because racism is considered widely unacceptable; however, bias based on gender or sexual orientation is more acceptable (Herek, 2007; Monteith & Mark, 2005). Norms concerning egalitarianism based on ethnicity are extremely strong, which leads people to be non-racist in situations where the bias would be obvious (e.g., aversive racism; Gaetner & Dovidio, 1986). However, it is often difficult for majority members to notice subtle, everyday forms of prejudice toward minorities (Sue et al., 2007).

Even if subtle racism is noticed and defined as discrimination, the second step in the model is seeing it as an emergency that requires immediate attention. The challenge at this step is that people may not be as upset about prejudice as they expect to be. Specifically, although participants in a recent study expected to be upset by a racist statement or event, actually experiencing the event was not as emotionally disturbing as anticipated (Kawakami, Dunn, Karmali, & Dovidio, 2009).

Thus, people may not confront prejudice in part because they are not as upset by prejudice as they expect to be, which may prevent people from defining the event as discriminatory and an emergency. It is a troubling that racism, which is widely

unacceptable, is less upsetting than expected. If racism does not cause confronting, it is unlikely that more acceptable forms of prejudice (i.e., sexism and anti-gay prejudice) cause confrontation either. These findings suggest that even the first two steps in the model present strong barriers to confronting.

Although the Confronting Prejudiced Responses Model is important to understanding why individuals confront, especially given negative consequences of confrontation for confronters (i.e., being disliked or seen as a complainer; Czopp et al., 2006; Dodd, Guiliano, Boutell, & Moran, 2001; Kaiser & Miller, 2001), it has three limitations. First, it explains confrontation only when the confronter has made a thoughtful, deliberative decision to confront prejudice rather than a more automatic decision. However, to my knowledge, there is no research indicating whether deliberative, systematic processes are used. Second, if and when systematic processes are used, there is only mixed evidence to support the steps of the model. In one study, participants' closed-ended responses, which were designed to assess steps of the model as factors, were interpreted to support the model; however, open-ended responses were not descriptive of the steps outlined in the model (Ashburn-Nardo et al., 2009; see also Hillard, 2011).

The third limitation of the Confronting Prejudiced Responses Model is that it does not address the conditions under which confrontations actually work. That is, this model says nothing about what makes confrontations effective in changing prejudiced attitudes in confrontation recipients (i.e., perpetrators or observers) in the event that confrontation does occur. Other research has examined the outcomes of confronting prejudice, but

questions remain. What are the outcomes of confronting prejudice, and what causes confrontation to be effective?

Outcomes of Confronting Prejudice in a Persuasion Framework

Research that examines the outcomes of confronting prejudice (vs. the conditions under which it occurs) for perpetrators and observers can be organized into four categories, as shown in Figure 3. Research has examined the following short-term outcomes immediately after confrontations: affective reactions, attitude change, behavior change, and confronter evaluation.

Affective Reactions

The examination of affective reactions as an outcome following confronting prejudice originates from research on the self-regulation of bias (Devine, Monteith, Zuwerink, & Elliot, 1991; Monteith, 1996; Monteith et al., 2002; Monteith & Mark, 2005). In order for individuals to regulate their bias, they must be aware of the bias. For both internal motivations to control bias (i.e., self-identifying as non-prejudiced) and external motivations to control bias (i.e., conforming to egalitarian social norms to avoid disapproval from others; Plant & Devine, 1998), being aware of bias causes negative affect. Monteith and colleagues' work has examined *guilt* and *shame* along with other self-directed emotions.

Guilt may serve as a self-regulation cue to control automatic bias. That is, cultural associations—which are often negative and stereotypical—are automatically activated in the presence of relevant cues (Devine, 1989). This type of automatic bias is assessed using implicit measures, such as the Implicit Association Test (IAT; Greenwald et al., 1998), which measure attitudes indirectly by comparing reaction times rather than asking

participants to report their attitudes (i.e., explicit measures). Research has shown that people taking the IAT detect that stereotypes facilitate their responses (i.e., it is easier to associate White [Black] exemplars with pleasant [unpleasant] words); detecting their implicit bias then causes guilt (Monteith, Voils, & Ashburn-Nardo, 2001). This guilt has then been shown to lead to self-regulation of bias. Specifically, experiencing guilt after completing a Black-White IAT led participants to more quickly associate stereotypically Black names with “like,” which is a measure of implicit liking (Monteith et al., 2002). Participants also showed inhibition of current behavior and engaged in self-reflection.

Other research has examined guilt as a trigger for prosocial behavior (Baumeister, Stillwell, & Heatherton, 1994; Maitner, Mackie, & Smith, 2006). By combining these views of guilt as a trigger for self-regulation and pro-social behaviors, research has examined whether increased guilt following a transgression was associated with decreased approach (i.e., inhibition; Amodio, Devine, & Harmon-Jones, 2007). All participants completed an electroencephalograph (EEG) task and were led to believe they had negative responses to Black faces. Participants then indicated their level of guilt and rated their interest in reading various articles based on article titles. The bogus feedback that participants received indicated negative responses to Black faces, which caused guilt. Participants who reported experiencing more guilt expressed greater interest in reading articles on prejudice reduction. Increased guilt was also related to interest in reparatory behavior and increased approach, paradoxically. However, the same was not true for other negative self-directed, social emotions such as shame. Although previous research conceptualized guilt as a construct by combining all negative self-directed emotions into

one construct, this research suggests that guilt and shame have different implications for future behavior.

Furthermore, the evidence to support the idea that guilt and/or negative self-directed affect is related to decreased prejudice is based largely on implicit measures of prejudice, such as increased implicit liking of Black names (Montieth et al., 2002) and more interest in reading articles about prejudice reduction, along with shifts in frontal lobe EEG activity (Amodio et al., 2007). There is not, to my knowledge, any research showing that guilt leads to a reduction in explicit prejudice or discrimination, especially not outside of the immediate measurement in a laboratory setting. In fact, negative self-directed affect and attempts at self-regulation may not generally reduce prejudice (Monteith, 1996).

Other Outcomes

Other research has examined attitude and behavior change as an outcome of confrontation rather than affective reactions. The most common goal(s) of confronting is attitude, behavior, and/or norm change (Hyers, 2007), which makes attitude and behavior change especially important outcomes of confronting. In fact, the first study of confronting prejudice showed that it changes attitudes (Citron, Chein, & Harding, 1950; see also Marcuse, 1951). Other research has shown that attitudes are more positive after confrontation (vs. before) for both perpetrators (e.g., Czopp et al., 2006) and observers (Hillard & Ryan, 2011). Similarly, confronting prejudice can reduce stereotypic behaviors (Czopp et al.; Mallet & Wagner, 2011) and increase egalitarian behavior (Paluck, 2011; Wellman et al., 2009). Still, evidence is needed that attitude and behavior change resulting from confrontation lasts over time.

Another outcome of confronting is the evaluation of the confronter by others, and research has shown that confronters are sometimes evaluated negatively (Czopp et al., 2006; Dodd et al., 2001; Kaiser & Miller, 2001). For example, negative evaluations of confronters are especially likely when the confrontation message is hostile (Czopp et al.; Gervais & Hillard, 2011) and when the confronter is a target (vs. non-target) group member (Gervais & Hillard; Rasinski & Czopp, 2010). However, confronters can sometimes also be seen positively (e.g., Mallett & Wagner, 2011; Saunders & Senn, 2009), which may depend on other variables such as the confrontation message (Swim, Gervais, Pearson, & Stangor, 2009) and context (Gervais & Hillard). Although the potential interpersonal costs may discourage people from confronting, confrontations may still be beneficial in terms of other outcomes of confronting. For example, confronting causes attitude and behavior change even if evaluations of confronters are negative (Czopp et al.).

But what existing theories might explain these outcomes of confronting?

Persuasion theories provide a framework to examine each of the four categories of outcomes (i.e., affective reactions, attitude change, behavior change, and confronter evaluation), as shown in Figure 4. Next, I specify how persuasion variables relate to these confrontation outcomes and also note whether this outcome is for perpetrators or observers.

Overview of Major Persuasion Theories

Persuasion and attitude change are major topics in social psychology, and many theories and models have been posited to explain the conditions under which persuasion occurs. The message-learning approaches by the Yale Group (Hovland, Janis, & Kelly,

1953; McGuire, 1985) provided a context for examining persuasion by placing it in a learning paradigm. According to this approach, the processes of persuasion are similar to learning and include attention, comprehension, yielding, and retention (Bettinghaus & Cody, 1994). That is, the message must have a person's attention and comprehension before causing lasting attitude change. Aspects of the communication, including the source, message, recipient/audience, and context, affect these message-learning processes.

These four categories of variables (i.e., source, message, recipient, and context) affect persuasion by influencing the learning of the persuasive message. Research in this area has shown, for example, that more credible and attractive sources are more influential (Chaiken, 1987; Petty & Cacioppo, 1981; Witkin, Goodenough, & Oltman, 1979). Messages that do not clearly intend to persuade and messages that refute counter arguments (i.e., two-sided messages) are more persuasive (Petty & Wegener, 1998). Recipient variables focus on individual differences that influence the likelihood of persuasion, such as need for cognition, issue knowledge, and issue involvement (Petty & Cacioppo, 1981; Petty & Cacioppo, 1986). In the case of confronting prejudice, the source is the confronter, the message is the confrontation message, and the recipient is the person who receives the confrontation (i.e., the perpetrator or observer). Context variables related to the setting include the channel (i.e., the message's mode of presentation) and audience distraction (Petty & Wegener). While the message learning approach provides a useful framework to examine the variables related to confronting prejudice, its limitation is the assumption that learning a message is tantamount to changing one's attitude. Research instead suggests that people's states as well as

interpretations of and reactions to a message influence persuasion (e.g., Gorn, 1982; Roberts & Maccoby, 1973).

Decades of research in persuasion produced contradictory findings until dual process theories were proposed to explain the inconsistencies. According to the Elaboration Likelihood Model (ELM; Petty & Cacioppo, 1986) and the Heuristic-Systematic Model (HSM; Eagly & Chaiken, 1993), persuasion can occur through low or high effort processing. These models explain when persuasion may occur through superficial examination of message content (i.e., low effort processing) or deeper examination of the message content and issue-relevant information (i.e., high effort processing).

ELM involves two routes to persuasion depending on the extent of elaboration, or scrutiny, given to the message. Elaboration exists on a continuum from low to high, and one's ability and motivation determine the extent of elaboration (Petty & Wegener, 1998). When elaboration likelihood is high (i.e., when one has the ability and motivation), the central route will be used, which involves more deep, effortful processing of message arguments. When elaboration likelihood is low (i.e., when one does not have the ability and motivation), the peripheral route will be used, which involves shallower and less effortful processing (e.g., attending only to the attractiveness of the speaker rather than the strength of arguments). Contrary to the message learning approach, the peripheral route of ELM implies that persuasion can occur without learning or even attending to a message, although attitude change brought about by peripheral processing is likely weaker than attitude change brought about by central processing.

Similarly, HSM features high effort (i.e., systematic) and low effort (i.e., heuristic) processing. Systematic processing is thought to occur when one has the ability for more effortful processing and requires a high level of confidence in the attitude. Heuristic processing occurs when ability is constrained and/or only low levels of confidence are necessary. Thus, HSM proposes a sufficiency threshold, where processing stops once the required level of confidence is achieved (Eagly & Chaiken, 1993). HSM also specifies that low effort processing involves heuristics and draws upon research on the development and activation of heuristics (Eagly & Chaiken).

Although ELM and HSM have different features (e.g., elaboration continuum for ELM vs. sufficiency threshold for HSM), both nevertheless can be used to explain the same findings. ELM is more descriptive and accounts for more variables, because ELM includes more low-effort processes than heuristics alone. On the other hand, HSM is more explanatory and accounts for fewer variables, because HSM limits low effort processing to the use of heuristics. Proponents of the contemporary versions of both ELM and HSM insist that the two processing methods can co-occur (Petty & Wegener, 1998; Eagly & Chaiken, 1993), although HSM focuses on the combined effects of the two methods of processing while ELM focuses on the distinct consequences of the two methods of processing. Given that both models revolve around the amount of effort or elaboration in processing messages, the term *high effort processing* refers to both ELM's central route to persuasion and HSM's systematic processing. *Low effort processing* refers to both ELM's peripheral route to persuasion and HSM's heuristic processing.

Now I turn to how high and low effort processes might be used to explain the effects or consequences of confronting prejudice for perpetrators and observers. The

message-learning categories (i.e., source, message, recipient, and context) are used to organize the variables that influence the outcomes of confrontation. The research findings across these categories of persuasion variables are then discussed with a focus on effort used in processing the confrontation. The goal is to show how variables related to the source, message, recipient, and context influence the participants' processing of a confrontation of prejudice from an ELM/HSM perspective.

Source

Manipulations of the race or gender of the confrontation source (i.e., confronter) have been shown to influence the effectiveness of confronting racism or sexism. Participants who were asked to imagine observing a confrontation reported feeling less guilt if the confronter was also the target of that prejudice (i.e., Blacks and women) than when confronters were not targets (i.e., Whites and men; Czopp & Monteith, 2003; Rasinski & Czopp, 2010).

This finding can be explained in terms of the confrontation observer's perceptions of the self-interest (or lack thereof) that motivates the confrontation. Taking an unexpected position that violates self-interest increases persuasion (Petty, Fleming, Priester, & Feinstein, 2001). Recent confrontation research shows that participants who were observers of confrontation were more surprised when a man (vs. a woman) confronted sexism (Gervais & Hillard, 2011). In this case, target confronters (i.e., Blacks and women) may have been seen as acting in their self-interest, thus conforming to expectations. However, non-target confronters (i.e., Whites and men) may have been seen as acting without self-interest, which violates expectations and causes surprise and increased persuasion in confrontation recipients.

In a similar study (Czopp et al., 2006; Study 2), the ethnicity of the target was manipulated and affective reactions were measured as in Czopp and Monteith (2003), but the confrontation was *in vivo* rather than imagined. That is, participants were led to make a stereotypic association and then confronted about this association by a confederate. The results, however, conflicted with the earlier study, as the White confronter did not cause more guilt than the Black confronter. Instead, the opposite effect was found. White confrontation recipients experienced more guilt when the confronter was Black (i.e., target confronter) than when the confronter was White (i.e., non-target confronter).

As Czopp et al. (2006) argue, the contradictory findings regarding whether target confronters cause more guilt may partly reflect the strength of the arguments used in the confrontation. The confrontation in Czopp and Monteith's (2003) research featured a weak message (i.e., a message with only one argument¹), whereas the confrontation used later in Czopp et al. featured a stronger argument.² Other research examining the influence of source effects has similarly manipulated argument strength. This research has shown that having a Black source led to more effortful processing, regardless of attempts to manipulate distraction or issue involvement (White & Harkins, 1994). Persuasion models predict that participants are more persuaded by strong than weak messages when processing effort is high; however, participants are equally persuaded by strong and weak messages when processing effort is low. White and Harkins could not support these predictions when the source was Black. The presence of a Black source automatically led to greater processing effort, which was explained in terms of aversive racism. That is, cultural stereotypes about Blacks are negative, but egalitarian norms are strong regarding racism (Gaertner & Dovidio, 1986). Thus, the mostly White participants

took extra care in weighing arguments made by a Black source to avoid appearing prejudiced, consistent with the theory of aversive racism.

Thus, dual process persuasion theories can explain these contradictory findings of Czopp and Monteith (2003) and Czopp et al. (2006). A Black confronter always led confrontation recipients to engage in high effort processing of confrontation messages, which led argument strength to be evaluated. Under these conditions, as persuasion theories would predict, confrontation recipients who were led to effortfully process the message (due to ethnicity of the source and aversive racism) were more persuaded by strong than weak arguments. As shown in Table 1, participants felt less guilt when confronted by a Black confronter using a weak argument (Czopp & Monteith), because they engaged in high effort processing and recognized the argument as weak. A white confronter using a weak argument, however, may have caused guilt because participants were not engaging in high effort processing, which led to persuasion even with a weak argument. On the other hand, participants felt more guilt when confronted by a Black confronter using a strong argument (Czopp et al.) because they engaged in high effort processing and recognized the argument as strong. Because a White confronter may not automatically trigger high effort processing, participants do not attend to argument strength and are equally persuaded by strong and weak arguments.

Czopp et al. (2006) also examined the consequences of confrontation on perpetrators' later stereotypic responding. The researchers compared the number of stereotypic inferences made prior to and after confrontation. They found that fewer stereotypic inferences were made post-confrontation than prior to confrontation, and there was no interaction with ethnicity of the confronter. Regardless of the confronter's

ethnicity, confronted participants showed less stereotypic responding and less prejudiced attitudes, as measured by pre- and post-confrontation attitudes, than did non-confronted participants. Whereas the findings regarding confrontation recipients' guilt were seemingly contradictory based on confronter's ethnicity, the findings regarding reduced stereotyping and prejudiced attitudes suggest that target status does not influence important outcomes of confrontation. Thus, it depends on one's definition of effective confrontation—whether it is increased the confrontation recipient's guilt or attitude change—that determines whether the source of confrontation affects the outcome. However, there is no evidence yet to support the notion that guilt motivates explicit attitude and behavior change. Indeed, Czopp et al. showed that attitude change in confrontation recipients did not depend on confronter ethnicity, whereas feelings of guilt did.

Message

Research has also examined aspects of the confrontation message that influence the effectiveness of confronting prejudice, including argument strength and hostility. The confrontation message's argument strength was just discussed regarding its interaction with source, but message strength has yet to be manipulated within a single experiment. However, a recent study examined whether the number of arguments influences confrontation's effect (Hillard & Ryan, 2011). Participants either attended to a 30-second public service announcement (PSA) confronting “that’s so gay” or a five-minute speech on anti-gay bullying and its consequences that featured the same PSA, which had been pre-tested to cause negative attitudes toward the slang use of gay and more positive attitudes toward gay men and lesbians. However, the number of arguments did not

influence attitude change. Participants in a control condition had more negative attitudes toward gay men and lesbians than participants who attended to the speech featuring the PSA or the PSA-only, who did not significantly differ. Assuming that more arguments represented a stronger argument overall, these findings support Czopp et al. (2006). That is, Czopp et al. showed that guilt depended on the source of the message, but attitude and behavior change did not. Similarly, the findings from Hillard and Ryan show that argument strength does not affect attitude change, but it might have influenced other outcomes like guilt and confronter evaluations.

Manipulating the hostility of the confrontation message affects some but not all outcomes of confronting. Perpetrators have more accepting reactions to low-hostility confrontation (e.g., acknowledging bias and apologizing; Czopp et al., 2006), which feature egalitarian norms, than to high-hostility confrontations, which feature accusations of racism. This study showed that high hostility confrontations caused perpetrators to report more anger toward the confronter, more general discomfort, and less favorable evaluations of the confronter than low hostility confrontations. However, neither negative self-directed affect (e.g., guilt, shame, and disappointment toward the self) nor stereotypic responding following the confrontation depended on the hostility of the confrontation message. Across levels of hostility, confrontation was effective in decreasing stereotyping in perpetrators.

Two other studies have similarly shown that, for sexism and heterosexism, message hostility affects evaluations of confronters but not effectiveness of confrontations. Low hostility, indirect confrontations similarly lead to more positive evaluations of confronters of sexism (Gervais & Hillard, 2011). Those who directly

confront using high-hostility accusations of sexism were more negatively rated than those who indirectly confront using low-hostility appeals to egalitarianism. Confrontation hostility, however, did not have an effect on participants' attributions of sexism. Similarly, there was no difference in terms of behavior change following a confrontation of heterosexism based on message hostility, but high- (vs. low-) hostility messages caused more negative evaluations of the confronters (Hyers, 2010). However, men who imagined being confronted for sexual harassment rated a female confronter relatively positively regardless of confrontation hostility, but she was seen as more irritating if she used a high hostility message (Saunders & Senn, 2009). Thus, although interpersonal consequences of confronting (i.e., confronter evaluation) are sometimes influenced by the hostility of the confrontation message, confrontation messages can increase recognition of bias and decrease stereotypic responding and prejudiced behavior regardless of the hostility of the message.

Persuasion theories, on the other hand, would suggest that a more hostile message should receive more effortful processing. HSM suggests that a more hostile message requires individuals to have more confidence in their position, which causes more effortful processing. However, people may be motivated to process confrontations with some effort regardless of the hostility of the message, because accusations of bias draw attention given strong egalitarian norms.

Recipient/Audience

Individual differences in the confrontation recipients, such as level of prejudice, influence the outcomes of confronting prejudice. Low (vs. high) prejudice people are expected to experience more negative self-directed affect if they transgress their

egalitarian standards (Devine et al., 1991). Low prejudice individuals would be expected to experience more negative self-directed affect if that transgression is then confronted. This prediction has some support; low-prejudice (vs. high) confrontation recipients experience both more negative self- and other-directed affect when confronted (Czopp et al., 2006; Study 3). Low prejudice participants decreased stereotypic responding more than high prejudice participants did following a confrontation, although confrontation led to significantly less stereotypic responding regardless of prejudice level (Czopp et al.). Further, decreased stereotyping was correlated with guilt, suggesting that although confrontation causes some guilt for high prejudice individuals, it causes more guilt (and thus less stereotyping) for low prejudice individuals.

The greater guilt for confrontation of low than high prejudice individuals can be explained in terms of individual differences in issue relevance or importance. That is, low prejudice individuals have internalized egalitarian standards (Devine et al., 1991; Plant & Devine, 1998). Low prejudice individuals may thus be more motivated than high prejudice individuals to effortfully process confrontation messages. Low prejudice individuals may also have more knowledge concerning the issues surrounding prejudice and discrimination. Whereas more knowledge of the topic leads to more effortful processing, low knowledge leads to less effortful processing that relies on cues (Petty & Cacioppo, 1986).

Further, more prejudiced confrontation recipients may face two interconnected obstacles (i.e., less knowledge and guilt) to high effort processing. When knowledge is low, the affective cues used in low effort processing become more important (Petty & Cacioppo, 1986). Because high prejudice individuals may have low knowledge of the

issues surrounding prejudice, high prejudice individuals do not experience as much negative self-directed affect as low prejudice individuals. This lower level of negative self-directed affect may be part of the reason that high-prejudice individuals are not as influenced by confrontations of prejudice as low-prejudice individuals. It is of course unfortunate, as these are the individuals who may need to be confronted the most!

Although there is no research on confronting prejudice that has examined this variable, I expect that need for cognition influences the outcomes of confronting. Those high in need for cognition enjoy effortful processing and are intrinsically motivated to do so in general (Petty & Cacioppo, 1986). Thus, if effortful processing changes attitudes following confrontation, those high in need for cognition should attend more to the strengths of arguments made during confronting prejudice. Those high in need for cognition should then be more persuaded by strong arguments and less persuaded by weak arguments than are those low in need for cognition. Future research placing confronting prejudice in a persuasion framework should measure this individual differences variable.

Context

Prejudiced Message

The original prejudiced message provides a context for confronting prejudice. Three variables involving aspects of the prejudiced message may influence the outcomes of confronting prejudice, including the target of the prejudiced message, the offensiveness of the prejudiced message, and the source of the prejudiced message.

Targets. The targets of the prejudiced statement may influence the outcome of confrontation because the acceptability of prejudice varies across targets (Fiske &

Stevens, 1993). That is, social norms against racism are stronger than norms against sexism or anti-gay prejudice (Czopp & Monteith, 2003). Two studies indicate that the same discriminatory action or prejudiced statement is viewed as more offensive and more prejudiced when the target is an ethnic minority rather than a woman or gay man (Cowan & Hodge, 1996; Rodin, Price, Bryson, & Sanchez, 1990). Thus, Czopp and Monteith hypothesized that participants would anticipate feeling more apologetic when confronted about racism than about sexism. This hypothesis was supported; being accused of race bias resulted in more negative self-directed affect (e.g., guilt) and concern than did being accused of gender bias (Czopp & Monteith). Given that there was a correlation between negative self-directed affect and decreased stereotypic responding in later work (Czopp et al., 2006), this guilt was likely accompanied by decreased stereotypic responding. Thus, confronting race bias may be more effective than confronting gender or other bias.

Offensiveness. The effectiveness of confronting prejudice also depends on the offensiveness of the prejudiced message. Prejudiced messages aimed at ethnic minority targets have been shown to be more offensive than similar messages aimed at other targets (Cowan & Hodge, 1996; Rodin et al., 1990). However, even within the same target, the effect of confrontation appears to depend on the offensiveness of the prejudiced statement.

Specifically, Hillard and Ryan (2009) manipulated offensiveness in a recorded conversation (i.e., a statement including “that’s so gay” was rated as less offensive than the same statement including “homo,” which was included in the more offensive condition) and confrontation (whether confrontation occurred). Hearing a confrontation (vs. no confrontation) of “that’s so gay” resulted in more positive attitudes toward gays,

whereas hearing a confrontation of “homo” did not. In the “that’s so gay” condition, the confrontation produced more positive attitudes toward gays in observers than did the no confrontation condition. In the “homo” condition, however, the confrontation and no confrontation conditions had the same attitudes toward gays. Compared to a control condition, however, both the no confrontation and confrontation conditions for “homo” produced more positive attitudes toward gays.

In this case, it may be that the confrontation draws the observers’ attention to the prejudice inherent in the less offensive statement (e.g., “that’s so gay)—prejudice that may otherwise go unnoticed—causing guilt. As suggested by Czopp et al. (2006), guilt may lead confrontation to be more effective. For more offensive statements (e.g., “homo”), however, prejudice is obvious; in this case, hearing a confrontation may alleviate guilt brought on by the prejudiced message, decreasing the confrontation effectiveness. The finding that attitudes towards gays did not significantly differ between participants that heard a confrontation of “that’s so gay” and participants that heard “homo” without a confrontation supports this interpretation.

Source. Important aspects relating to the prejudiced message have just been reviewed, but what of the variables related to the person who says the prejudiced message? Research shows that, from the perspective of ingroup members and outgroup bystanders, *intergroup* criticism (i.e., criticism from an outgroup member towards another group or its members) appears to be an expression of prejudice, while *intragroup* criticism does not (i.e., criticism towards a group or its members from an ingroup member; Sutton, Douglas, Elder, & Tarrant, 2007). The difference between *intergroup* and *intragroup* criticism may be due to the intergroup sensitivity effect, which is the

“tendency to respond more favorably to internal than to external criticism of groups”
(Sutton et al.).

The intergroup sensitivity effect would suggest that the motivation for criticism is different based on group membership or identity. If an ingroup member voices criticism, it is assumed by others to be motivated by good intentions toward the group. On the other hand, the same criticism coming from an outgroup member would be interpreted to be motivated by prejudice, because the outgroup member should have no motivation to improve the group. In fact, this effect may not be limited to criticism. Expressing positive beliefs about a group as a whole, when coming from an outgroup member, leads that speaker to be negatively evaluated by others (i.e., is less liked and leaves a more negative impression; Mae & Carlston, 2005). Thus, criticisms or praise from an outgroup member are more likely attributed to prejudice than the same statements by an ingroup member. Because outgroup comments are more likely attributed to prejudice and recognizing a comment as prejudice is an important feature in the CPR model, I would expect that outgroup members are more likely to be confronted.

While not all criticism or praise is motivated by prejudice, group membership and social identity of the speaker may influence bystanders' perceptions of the motivation underlying the speaker's comments. Thus, the source of the message may influence the offensiveness of a statement and whether the statement is viewed as prejudiced. Offensiveness and acceptability of prejudice may then go on to influence the effectiveness of confrontation.

Integration of Prejudiced Message Context Variables. The research discussed in terms of the target, offensiveness, and source of the original prejudiced message is

interconnected and may be explained by dual-processing models. Confronting offensive messages may inspire effortful processing of confrontation messages, whether the messages are deemed offensive because of their target, source, or degree of intended harm. In some cases, it may be the offensiveness of the prejudiced message that captures attention and serves as a cue for elaboration, particularly for low prejudice individuals. The offensiveness of a statement may also depend on who is saying it, as identical statements may be seen as more offensive when made by outgroup than by ingroup members. In this way, offensiveness of the prejudiced message, whether through aspects of the target or the source of the message, may be causing more effortful processing and negative self-directed affect, which then increases the effectiveness of confrontation in the presence of strong arguments.

Public vs. Private Context

One study examined the effect of physical context by manipulating whether the confrontation occurred publically or privately (Gervais & Hillard, 2011). The effect of context on leadership perceptions of the confronter depended on the confronter's gender in a way that is consistent with gender role stereotypes. For a female confronter, overall leadership ratings were highest when she confronted *privately*. For a male confronter, overall leadership ratings were highest when he confronted *publically*. The same was true for ratings of competence, charisma, and helpfulness, but not respect, which was rated higher when confrontation was private rather than public regardless of confronter gender. The outcome that was the focus of the study was confronter evaluations, but this study also shows that perpetrators are considered more sexist when confrontation occurred in public rather than private. It may be because public confrontations led to more effortful

processing, and participants thus determined that the comment was more sexist. If private confrontations led to less effortful processing, participants would be less likely to determine that the comment was sexist. Thus, public but not private confrontations lead observers to believe the comment was more sexist.

Audience Response

The broader context for confrontation may at times include bystanders. The effectiveness of confrontation for these observers is influenced by the perpetrator's response to the confrontation message. Rasinski and Czopp (2008) asked participants to imagine a confrontation scenario in which they were the observers; following a confrontation, the perpetrator had either an apologetic or hostile reaction. Type of perpetrator reaction (along with individuals' level of prejudice) influenced observers' ratings of their likelihood to confront prejudice in the future. For low prejudice observers, both apologetic and hostile reactions caused participants to be more likely to confront prejudice in the future and less likely to tell a prejudiced joke. For high prejudice observers, though, only an apologetic reaction from the perpetrator decreased the future likelihood of telling a prejudiced joke.

These findings can again be explained in terms of levels of processing. Low prejudice observers are motivated to effortfully process the confrontation because of personal issue relevance, knowledge, and importance. For these low prejudice individuals, the response of the perpetrator did not determine effectiveness. Instead, they were persuaded by the confrontation arguments, which they attended to because of their motivation to be non-prejudiced. However, high prejudice observers are not motivated to effortfully process the confrontation because of low personal issue relevance, knowledge,

and importance. For high prejudice individuals, only an apologetic reaction led to increased elaboration. In this case, it may be that high prejudice observers were motivated to process the confrontation because they were unsure why the perpetrator needed to apologize. High prejudice individuals may see the apology as an unexpected response that is inconsistent with the perpetrator's previous statement. The resulting surprise may cause high prejudice individuals to process the confrontation more effortfully than the confrontation followed by a hostile reaction.

Further, although confrontations of prejudice do not have to be hostile, an argument would not be an unexpected result and might only involve low effort processing. It then makes sense that these expected, hostile reactions decreased the effectiveness of confrontations for high prejudice observers; they did not inspire high effort processing in high prejudice individuals. Thus, personal prejudice level and context cues, such as perpetrator reactions to being confronted about prejudice, interact and can be explained by low versus high effort processing.

In support of this explanation, research has shown that interpretations of racial harassment depend greatly upon the harasser's reaction to the instance (McClelland & Hunter, 1992; see also Swim et al., 2009). Participants imagined observing racism and rated the seriousness of situations differing in offensiveness. The account of the harasser had a greater influence on ratings of incident seriousness than did the account of the victim. In fact, this effect was such that harassers' accounts had five times the influence of the victim's account of the incident. Similarly, other work indicates that sexual harassers' accounts have eight times the influence of victim accounts on ratings of incident seriousness (Hunter & McClelland, 1991). Participants may use the response of

the harassers (and by extension, the perpetrators, who say the original prejudiced statement) as a cue for elaboration, whereas the response of the victims (and by extension, confronters) are discounted. As discussed before, perhaps confronters are discounted because the confronter is disliked and seen as a complainer (Kaiser & Miller, 2001; Czopp et al., 2006) or acting out of self-interest (Petty et al., 2001).

Outline of the Present Study

Confronting prejudice is an effective way to reduce prejudice and discrimination. Literature on confronting prejudice has examined characteristics related to confrontation message, source, recipient/audience, and context, which influence the outcomes of confronting for perpetrators and observers. Apparent contradictions in the literature may be explained by examining effort or elaboration of confrontation message. Thus, research on confronting can be integrated with existing theory on persuasion, which provides a model for future research. Further, examining confronting from an ELM/HSM perspective provides a more process-oriented view of the outcomes of confronting prejudice on recipients and observers. This perspective on confronting can then be tested empirically (e.g., by directly manipulating motivation and/or ability for effortful processing).

One of the major limitations of the existing work on confronting is its examination of only short-term change in the laboratory. Although persuasion dissipates over time (Cook & Flay, 1978), persistence of attitude change is greater when individuals have the motivation and ability to elaborate on the message (Petty & Wegener, 1998). For example, messages concerning more interesting and involving issues (e.g., socio-political issues) lead to greater attitude change persistence than less involving issues (e.g.,

information on past presidents; Ronis, Baumgardner, Leippe, Cacioppo, & Greenwald, 1977). Further, individuals high (vs. low) on need for cognition show greater persistence of attitude change after a two-day delay (Haugtvedt & Petty, 1992). In addition, attitude change persistence is greater over a two- to five-month delay when participants are told they will explain an argument presented in the essay to others (vs. not told this; Boninger, Brock, Cook, Gruder, & Romer, 1990). Thus, those with motivation—whether that comes from an interesting issue, individual differences in need for cognition, or an experimental manipulation—elaborate on persuasive messages, which causes lasting attitude change. Based on this work, I expect that the effect of confronting prejudice would persist over time to the degree that the confrontation inspires effortful issue-relevant elaboration.

The present study examines the immediate and longer-term (i.e., after a delay lasting up to two weeks) effects of confronting (vs. not confronting) sexist jokes; it also manipulates participants' elaboration of the confrontation message. I test three hypotheses derived from the literature on confronting prejudice and persuasion. First, I expected that confronting a sexist joke will cause less hostile attitudes toward women in the short-term (i.e., Hypothesis 1a) and less discrimination against women in the long-term (i.e., Hypothesis 1b) among observers than will not confronting a joke. Second, I expected that elaboration (vs. no elaboration) of confrontation messages enhances the effectiveness of the confrontation in the short-term, causing greater attitude change and less discrimination (i.e., Hypothesis 2). Third, I expected that attitude change following confrontation lasts longer among those who engage in elaboration (vs. no elaboration) of confrontation messages (i.e., Hypothesis 3).

CHAPTER 3

Method

Participants

The planning of the study and recruitment of participants was informed by power analyses. Based on past research (e.g., Czopp et al., 2006), I assumed a medium effect of confrontation that decreases over time. For a 70% chance of rejecting the null hypothesis that there is no difference between conditions if a difference truly exists, the target number of participants needed for the study at Time 3 was 150. I therefore attempted to recruit twice as many participants at Time 2, anticipating that many participants would not complete all three phases of the study.

The recruitment procedures varied by phase of the study. Time 1 participants were recruited from students enrolled in psychology courses at the University of Nebraska-Lincoln. These participants completed departmental mass screening, including measures relevant to this study, in exchange for partial course credit. Participants also indicated whether they wanted to be contacted by researchers for future studies. All Time 1 participants were eligible to complete Time 2. Time 2 recruitment occurred either through an email invitation (for participants who provided permission for such contact) or through a departmental, online database for participant recruitment, which included completion of Time 1 as an eligibility requirement. During Time 2, participants were again asked if they would like to be contacted for a future study. Time 2 participants who provided their consent for future research either at Time 1 and/or Time 2 were eligible to complete Time 3 and invited to complete an online study via email.

Several items were included at each time point for the purpose of connecting participants' data. Specifically, participants provided the last four digits of their phone number, their ACT score, and demographic information (i.e., age, gender, and ethnicity) at all time points. At Time 2 and 3, participants also provided their high school mascot. To connect participants from Time 2 to Time 1 and from Time 2 to Time 3, I initially used the last four digits of the phone number, and the additional items were used to confirm a participant's identity.

Using the procedure just described to identify and match participants' data across phases of the study, there were a total of 304 participants at Time 1, 361 participants at Time 2, and 161 participants at Time 3. There is complete data (i.e., across all three time points) for 156 participants, of which 63.1% were women. Participants with complete data had a mean age of 19.44 years ($SD = 2.77$) and most identified as White/European American (87.9%), with 8.3% identifying as Hispanic/Latino American, 5.1% identifying as Asian American, 3.2% identifying as African American, and 0.6% identifying as Native American. (The percentages add up to more than 100% because some participants indicated multiple categories.)

There is incomplete data (i.e., either Time 1 or 3 missing) for 205 participants, of which 54.1% were women. Participants with incomplete data had a mean age of 19.07 years ($SD = 1.99$) and most identified as White/European American (85.8%), with 6.1% identifying as Asian American, 3.4% identifying as Hispanic/Latino American, 2.0% identifying as African American, 0.7% identifying as Native American, and 2.0% identifying as other.

Procedure

Time 1: Pre-test. Participants completed pre-test measures through mass screening conducted online for participant selection within the Department of Psychology. The effect of elaborating (vs. not elaborating) on confrontation messages may depend on individual differences in motivation to process the sexist joke and/or confrontation. For example, personal involvement provides a motivation to increase effortful processing (Petty & Cacioppo, 1986), and individuals who think gender activism is important are likely to elaborate on confrontation messages. At Time 1, then, I measured individual differences in motivation to control sexism and gender activism as well as measures of attitudes and feelings toward women, as described below.

Time 2: Laboratory Manipulation. Participants completed a study ostensibly examining perceptions of social interactions, as in Ford et al. (2008). Participants provided informed consent, received general instructions, and were randomly assigned to one of three conditions: no-confrontation control, confrontation-only, or confrontation+elaboration. Participants imagined themselves in situations presented in vignettes, which described interactions between co-workers, as shown in Appendix A. One vignette scenario included pre-tested sexist jokes (i.e., scenario #2; Ford et al., 2008) shared by three of the five co-workers. The manipulation of confrontation involved a co-worker either not confronting the sexist jokes (i.e., control condition) or confronting the sexist jokes (i.e., confrontation conditions) using approximately three arguments (adapted from Czopp et al., 2006): “Have you thought about what that suggests about women? Women aren’t dumb or the only ones that do housework. You should try to think about women in less prejudiced ways... those jokes seem kind of sexist.” However, to assure that the manipulation of confrontation was not confounded with negativity, participants in

the control condition read a critique of the jokes that did not involve sexism but was still a negative evaluation of the jokes (adapted from Czopp et al.): “I thought some of those jokes seemed a little stupid. Couldn’t you think of anything better than that?”

In order to manipulate elaboration of the confrontation message, participants were asked to write essays that either focused on the confrontation or not.⁴ Participants in the confrontation+elaboration condition wrote a one-page essay describing why the situation might be offensive to someone. Participants in the control and confrontation-only conditions wrote a one-page essay describing in detail their plans for the rest of the day, as in Burton and King (2004) and King and Miner (2000). To further strengthen the manipulation, participants were told that they would explain their essay to others, which is related to greater attitude change persistence (Boninger et al., 1990). Thus, participants in the confrontation+elaboration condition thought more about the confrontation, whereas participants in the control and confrontation-only conditions focused on something unrelated to the vignette, which was intended to inhibit their ability to elaborate on the confrontation. Participants then completed the dependent measures, including attitudes and feelings toward women, affect, and behavioral intentions. Finally, participants were thanked and provided with a general rather than specific debriefing statement, because there was an additional phase of the study.

Time 3: Delayed Post-test. One day following their participation at Time 2, participants were invited (via contact information and permission provided at Time 1 or 2) to complete Time 3, which was an ostensibly different study on how budget cuts should be implemented to student organizations on campus. To further differentiate this phase of the study, participants were offered \$5 for their participation rather than partial

course credit. Participants were invited to complete an online questionnaire within one week and were sent a reminder email as that deadline approached. Participants who had not completed the survey within one week and had provided a phone number at Time 1 were contacted and offered an additional extension for their participation (i.e., up to two weeks following Time 2). The mean time between Time 2 and 3 for the 159 participants for which this data is available was 3.97 days ($SD = 3.14$).

During this phase, participants allocated organizational budgets, including the budget for a women's organization, as shown in Appendix B (adapted from Ford et al., 2008). Consistent with Ford et al., participants discriminated against women if they allocated more of the overall budget cut (20%) to the women's organization. Participants also rated how they expected others to react to their budget cuts and completed measures of attitudes and feelings toward women. Finally, participants were thanked, debriefed, and received \$5 (in cash they picked up later or to a PayPal account) for their participation.

Measures

As shown in Table 2, participants completed most individual difference variables prior to the manipulation (i.e., either at Time 1 or before manipulations at Time 2). After the manipulations at Time 2 and at Time 3, participants completed items related to behavior, attitudes, and feelings toward women.

Motivation to Control Sexism. At Time 1, participants completed the Internal and External Motivation to Respond Without Sexism Scales (Klonis, Plant, & Devine, 2005). Participants responded to five items concerning internal motivation (e.g., "Being nonsexist toward women is important to my self concept"; $\alpha = .81$) and five items

concerning external motivation (e.g., “I try to act in nonsexist ways because of pressure from others”: $\alpha = .73$) on a Likert-type scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*).

Gender Activism. At Time 1 or 2, participants completed the Gender Role Journey Scale’s subscale for personal-professional activism (O’Neil, Egan, Owen, & Murray, 2005). Participants responded to 13 items (e.g., “I reflect on my feelings about gender role conflict and then act on them”) on a Likert-type scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*), $\alpha = .90$. Some participants completed this measure at Time 1; other participants completed this measure at the conclusion of Time 2 because of space restrictions on mass screening during the last semester of data collection.

Need for Cognition. At Time 2 but prior to the lab manipulations, participants completed the Need for Cognition Scale (Cacioppo & Petty, 1982). Participants responded to 18 items (e.g., the reverse-coded “I only think as hard as I have to” and “I prefer my life to be filled with puzzles that I must solve”) on a Likert-type scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*), $\alpha = .90$.

Need for Affect. At Time 2 but prior to the lab manipulations, participants completed the Need for Affect Scale (Maio & Esses, 2001). Participants responded to 13 items concerning their tendencies to approach emotion (e.g., “I am a very emotional person”; $\alpha = .83$) and 13 items concerning their tendencies to avoid emotion (e.g., the reverse-coded “Displays of emotions are embarrassing”; $\alpha = .86$) on a Likert-type scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*).

Social Desirability. At Time 2 but prior to the lab manipulations, participants completed Ray's (1984) short form of the social desirability scale. Participants responded to eight items (e.g., "Are you quick to admit making a mistake?" and "Are you always a good listener, no matter whom you are talking to?") by indicating either *Yes* (coded as 1), *Unsure* (coded as 2), or *No* (coded as 3), $\alpha = .58$.

Feeling Thermometers. At Time 1, 2 (post-manipulation), and 3, participants rated their feelings toward subgroups of women (i.e., women, homemakers, career women, feminists, and party girls; $\alpha = .70$ at Time 1) and men (i.e., men, male caretakers, career men, womanizers, and jokesters; $\alpha = .66$ at Time 1) as well as other, filler groups selected from the American National Election Studies survey to support the cover story (e.g., "people on welfare") on a feeling thermometer ranging from 0 (*very coolly*) to 100 (*very warmly*).

Ambivalent Sexism Inventory. At Time 1, 2 (post-manipulation), and 3, participants completed Glick and Fiske's (1996) measure of benevolent ($\alpha = .77$ at Time 1) and hostile sexism ($\alpha = .84$ at Time 1). Participants responded to 22 items (e.g., "Women should be cherished and protected by men" for benevolent sexism and the reverse-coded "Feminists are making entirely reasonable demands of men" for hostile sexism) on a Likert-type scale ranging from 1 (*strongly disagree*) to 7 (*strongly agree*).

Positive and Negative Affect. Following the manipulations at Time 2, participants completed the Positive and Negative Affect Schedule (PANAS; Watson, Clark, & Tellegen, 1988) to measure affect. Participants responded to 10 items for positive affect (e.g., "enthusiastic"; $\alpha = .87$) and 10 items for negative affect (e.g., "irritated"; $\alpha = .87$) on a scale ranging from 1 (*very slightly or not at all*) to 7 (*extremely*).

Participants completed portions of the expanded form of the PANAS (PANAS-X) on the same scale. Specifically, they completed the guilt subscale including six items (e.g., “ashamed” and “angry at self”; $\alpha = .89$) and the surprise subscale including three items (e.g., “amazed”; $\alpha = .74$). Similarly, participants completed items from Czopp et al. (2006) on the same scale for negative self- and other-directed affect in nine items (e.g., “regret”; $\alpha = .93$) and five items (e.g., “angry at others”; $\alpha = .90$), respectively. Finally, participants reported discomfort as in Czopp et al. using eight items (e.g., “tense” and “anxious”; $\alpha = .85$) on the same scale.

Ratings of Scenarios, Jokes, and Speakers. Immediately following the manipulations at Time 2, participants rated how entertaining, humorous, and offensive overall the scenario was on a scale ranging from 1 (*not at all*) to 7 (*very*). Later at Time 2, participants indicated their liking of each individual joke on a Likert-type scale ranging from 1 (*disliked strongly*) to 7 (*liked strongly*).

Also, participants rated how much they liked each of the five speakers on a Likert-type scale ranging from 1 (*disliked strongly*) to 7 (*liked strongly*) as well as how reasonable they found each of the speakers’ behaviors on a Likert-type scale ranging from 1 (*not reasonable*) to 7 (*very reasonable*). Ratings of liking and reasonableness were combined across the three speakers who shared a sexist joke (i.e., Michael, Donna, and Cindy, as shown in Appendix A), $\alpha = .88$, and for the speaker who evaluated the jokes negatively (i.e., either confronted or not), $\alpha = .80$.

Behavioral Intentions. Following the manipulations at Time 2, participants indicated how likely they were to use sexist jokes/statements (e.g., “In the future, how likely are you to make statements demeaning women to others”; $\alpha = .79$) and confront

sexism in the future (e.g. “In the future, how likely are you to confront someone using sexist jokes”; $\alpha = .89$). Participants rated their agreement on a scale ranging from 1 (*not at all likely*) to 7 (*very likely*).

Perceived Attention and Attitude Certainty. Following the manipulations at Time 2, participants indicated their perceived amount of attention to the sexist jokes and attitude certainty, using items adapted from Barden and Petty (2008). Participants indicated how much attention they paid to the vignette and jokes using two questions (e.g., “To what extent did you take the time you needed to carefully read the last jokes and statements?”) on a scale ranging from 1 (*not at all*) to 7 (*definitely*), $\alpha = .76$. Participants indicated how certain they were of their attitudes using three questions (e.g., “How confident are you of your opinion about the jokes?”) on a scale ranging from 1 (*not at all*) to 7 (*very*), $\alpha = .97$.

Budget Cut Norms. At Time 3, participants rated how they expect others in the immediate context (i.e., local norms) and other students in general (i.e., general norms) to react to their budget allocations for each organization on a scale ranging from 1 (*strongly disapprove*) to 7 (*strongly approve*).

Attitudes Toward Social Organizations. At Time 3, participants rated their feelings toward the organizations in the budget task on a scale ranging from 1 (*very negative*) to 7 (*very positive*) as well as how important they consider each organization on a Likert-type scale ranging from 1 (*very unimportant*) to 6 (*very important*) and how wisely each group will spend the allocated money on a scale ranging from 1 (*not wisely*) to 7 (*very wisely*). These three items were used to create a composite variable for attitude toward each organization.

Feelings Toward Organizations. At Time 3, participants rated their feelings toward groups relevant to the social organizations (e.g., “Jews” and “agriculture students”) on a feeling thermometer ranging from zero (*very coolly*) to 100 (*very warmly*).

CHAPTER 4

Results

To test my hypotheses concerning the effects of confrontation (vs. no confrontation) and elaboration (vs. no elaboration) of confrontation messages, I examined the effect of condition (i.e., no confrontation control, confrontation-only, or confrontation+elaboration) on measures included at one time point as well as measures included across time points (i.e., benevolent sexism, ambivalent sexism, and feeling thermometers toward women). First, I examined the effects of condition on dependent variables measured only at Time 2 or 3 using one-way between-subjects analyses of variance (ANOVAs) or mixed model ANOVAs including a within-subjects factor where appropriate. I also examined whether there were interactions between condition and participant gender that influenced Time 2 and 3 dependent variables using, for example, 3 (condition) \times 2 (gender) between subjects ANOVAs. Unless otherwise noted, participant gender did not qualify the results of condition. Second, I examined the change over time in longitudinal dependent variables through a series of multilevel models, including unconditional, missingness, multivariate, and finally conditional models.

Effects of Confrontation and Elaboration at Time 2

Positive and Negative Affect. One-way ANOVAs were conducted to examine the effect of condition (control vs. confrontation vs. confrontation+elaboration) on positive affect, negative affect, surprise, guilt, negative self-directed affect, negative other-directed affect, and discomfort. Omnibus tests indicated significant differences across condition for surprise, $F(2, 355) = 5.10$, $MSE = 1.16$, $p < .01$, and discomfort, $F(2, 355) = 4.36$, $MSE = 0.78$, $p < .05$; there were also marginal differences based on

condition for negative affect, $F(2, 355) = 2.57$, $MSE = 0.81$, $p = .08$; negative self-directed affect, $F(2, 355) = 2.42$, $MSE = 0.99$, $p = .09$, and negative other-directed affect, $F(2, 355) = 2.92$, $MSE = 1.36$, $p = .055$. As shown in Table 3, focused tests (i.e., LSD pairwise comparisons) revealed that participants in the confrontation-only condition experienced less surprise and discomfort than did participants in both the control and confrontation+elaboration conditions. In addition, participants in the confrontation-only condition experienced less negative affect and negative other-directed affect than did participants in the confrontation+elaboration condition.

Ratings of Scenario and Jokes. Separate one-way ANOVAs were estimated to examine the effects of condition on participants' ratings of the extent to which the scenario including sexist jokes was humorous, entertaining, and offensive. However, there were no significant effects of condition according to both omnibus tests, $ps > .23$, and focused tests, $ps > .17$.

To examine the effect of condition on participants' liking of specific sexist and neutral jokes included in the scenario, a 3 (condition, between subjects) \times 2 (participant gender, between subjects) \times 2 (joke type, within subjects: sexist, neutral) mixed ANOVA was estimated. The effect of joke type was significant, $F(1, 350) = 29.11$, $MSE = 1.84$, $p < .001$, but was qualified by a significant interaction between joke type and participant gender, $F(1, 350) = 48.69$, $MSE = 1.84$, $p < .001$. As shown in Table 4, participants generally liked neutral jokes more than sexist jokes; however, women liked sexist jokes less than men did, $p < .05$, whereas women and men equally liked neutral jokes. Still, there was no significant effect of condition or its interaction with joke type or gender, $ps > .17$.

Ratings of Speakers. To examine the effect of condition on participants' evaluations of speakers who either shared sexist jokes, a 3 (condition) \times 2 (participant gender) between subjects ANOVA was estimated. The only significant effect was a main effect of gender, $F(1, 351) = 54.31$, $MSE = 1.44$, $p < .001$, which indicated that women ($M = 3.14$, $SD = 1.21$) rated the speakers of sexist jokes more negatively than men ($M = 4.12$, $SD = 1.18$); all other $ps > .43$.

Because confronters in previous research were sometimes evaluated negatively (e.g., Kaiser & Miller, 2004), a parallel analysis was conducted for participants' evaluations of Paula, the speaker who either confronted or gave a negative evaluation of the jokes. There was a significant main effect of gender, $F(1, 351) = 12.72$, $MSE = 1.98$, $p < .001$, which indicated that women ($M = 4.95$, $SD = 1.42$) rated Paula more positively than men ($M = 4.39$, $SD = 1.41$). There also was a marginal interaction between gender and condition, $F(2, 351) = 2.30$, $MSE = 1.98$, $p = .10$. As shown in Table 5, women's ratings of Paula were more positive when Paula confronted prejudice (i.e., in the confrontation-only condition) than when Paula did not confront prejudice (i.e., in the control condition), $p < .01$; however, men's ratings of the confronter did not differ by condition, $p > .50$.

Behavioral Intentions. Separate one-way ANOVAs indicated that there was no effect of condition on intentions to use sexist jokes and statements, $F(2, 357) < 1$, or intentions to confront sexism, $F(2, 357) = 1.72$, $MSE = 2.67$, $p > .18$; focused tests also indicated no differences based on condition, $ps > .10$.

Perceived Processing and Attitude Certainty. Separate one-way ANOVAs indicated that there was no effect of condition on perceived attention paid to the jokes,

$F(2, 357) = 2.35, MSE = 1.36, p = .10$, or attitude certainty, $F(2, 357) = 2.10, MSE = 1.70, p = .13$. Focused tests ($p < .05$), however, indicated that participants in the confrontation+elaboration condition ($M = 5.25, SD = 1.10$) perceived paying more attention to the jokes than did participants in the confrontation-only condition ($M = 4.95, SD = 1.30$). In addition, participants in the confrontation+elaboration condition ($M = 5.56, SD = 1.29$) felt more certain about their attitudes toward the jokes than did participants in the confrontation-only condition ($M = 5.22, SD = 1.37$).

Donation. A one-way ANOVA indicated that there was a marginal effect of condition on the amount (\$0-20) participants reported being willing to donate to a women's organization, $F(2, 354) = 3.02, MSE = 38.17, p = .05$. According to focused tests ($p = .015$), participants in the confrontation+elaboration condition ($M = \$8.25, SD = \7.04) donated more than did participants in the control condition ($M = \$6.27, SD = \5.10). In partial support of Hypothesis 2, participants who elaborated on a confrontation message donated more to a women's organization than did control participants.

Previous research examined the effects of sexist jokes on hostile sexist men (Ford et al., 2008). Thus, I estimated a 3 (condition) \times 2 (gender) analysis of covariance (ANCOVA) with hostile sexism as a covariate. There were significant main effects of gender, $F(1, 347) = 21.88, MSE = 34.20, p < .001$, and condition, $F(2, 347) = 3.60, MSE = 34.20, p < .03$, and hostile sexism was a significant covariate, $F(1, 347) = 9.74, MSE = 34.20, p < .01$. Again, participants in the confrontation+elaboration condition donated more than participants in the control condition did, but women ($M = \$8.80, SD = \6.36) donated more than men did ($M = \$5.22, SD = \5.33). However, there was not a significant interaction between gender and condition, $F(2, 347) < 1$.

In Hypothesis 2, I predicted that elaboration (vs. no elaboration) of confrontation messages decreases discrimination. Because people with higher need for cognition are likely to engage in more elaboration regardless of experimental condition, I examined whether need for cognition, condition, and/or their interaction influenced donation. Need for cognition was not directly related to the amount participants were willing to donate, $r(355) = .05, p > .32$. Across three separate regression models, I predicted donation with need for cognition (centered at midpoint), one contrast code for condition (i.e., *either* 1 = control, -1 = confrontation-only; 1 = confrontation-only, -1 = confrontation+elaboration; *or* 1 = control, -1 = confrontation+elaboration), and the interaction between need for cognition and the contrast code. None of the interactions between the contrast code for condition and need for cognition were significant, $ps > .59$. Thus, need for cognition does not qualify the results of condition, contrary to Hypothesis 2.

Time 2 Summary. Participants in the confrontation+elaboration condition experienced more surprise, discomfort, negative affect, and negative other-directed affect than did participants in the confrontation-only condition. Participants in the confrontation+elaboration condition also reported paying more attention to the jokes and were more certain about their attitudes toward the jokes than participants in the confrontation+elaboration condition, which indicates that the manipulation of elaboration was effective. In addition, women but not men more positively evaluated a speaker who spoke up about prejudice (as in the confrontation-only condition) rather than not confronting prejudice (as in the control condition). Finally, participants in the confrontation+elaboration condition donated more to a women's organization than did participants in the control condition. This finding partially supports Hypothesis 2 that

elaboration on confrontation increases the effectiveness of confrontation. However, need for cognition (i.e., an individual difference related to one's tendency to elaborate) was not related to donation and did not interact with condition, which is contrary to Hypothesis 2.

Effects of Confrontation and Elaboration at Time 3

Budget Discrimination. Separate one-way ANOVAs were estimated to examine the effects of condition on both the percent of the participant's total budget cut (i.e., approximately \$30,000 overall, across seven organizations) allocated to the women's organization and the absolute amount cut from the women's organization budget, which started at \$24,050. Although there was no significant effect of condition for percentage of cut allocated to the women's organization, $F(2, 154) = 1.13$, $MSE = 49.27$, $p > .32$, there was a marginal effect of condition on amount cut from the women's organization, $F(2, 156) = 2.85$, $MSE = 7,714,904.86$, $p = .06$. Focused tests ($p < .05$) indicated that participants in the control condition ($M = \$5,883.15$, $SD = \$3,733.28$) cut more from the women's organization than did participants in either the confrontation-only ($M = \$4,800.08$, $SD = \$2,634.29$; $p = .04$) or confrontation+elaboration ($M = \$4,706.98$, $SD = \$1,438.29$; $p = .04$) conditions, who cut similar amounts, $p > .83$. In support of Hypothesis 1b concerning the effect of confrontation (vs. no confrontation), participants in both confrontation conditions cut approximately \$1,000 less from the women's organization than did participants in the control condition. However, contrary to Hypothesis 2 concerning the effect of elaboration (vs. no elaboration), there was no additional decrease in discrimination for elaborating on the confrontation message.

I next examined whether need for cognition, condition, and/or their interaction influenced discrimination. Need for cognition was not directly related to the percent or

amount cut from the women's organization by participants, $r(157) = .06, p > .47$ and $r(157) = .05, p > .55$, respectively. Across three separate regression models, I predicted donation with need for cognition (centered at midpoint), one contrast code for condition (i.e., *either* 1 = control, -1 = confrontation-only; 1 = confrontation-only, -1 = confrontation+elaboration; *or* 1 = control, -1 = confrontation+elaboration), and the interaction between need for cognition and the contrast code. None of the interactions between the contrast code for condition and need for cognition were significant, $ps > .44$. These analyses do not support the idea that elaborating (vs. not elaborating) on confrontation messages (through condition, individual differences in need for cognition, and/or their interaction) decreases discrimination, which is contrary to Hypothesis 2.

Perceived Budget Cut Norms. To examine the effect of condition on participants' perceptions of local and general norms, a 3 (condition, between subjects) \times 7 (organization, within subjects) mixed ANOVA was estimated. There was a significant main effect of organization on local norms, $F(6, 900) = 8.24, MSE = 1.41, p < .001$. Focused tests between the women's organization and all other organizations indicated that participants perceived greater local agreement with their cuts to the women's organization ($M = 4.08, SD = 1.50$) than the study abroad program ($M = 3.69, SD = 1.68$), $p = .01$. Participants also perceived greater local agreement with their cuts to cinema club ($M = 4.58, SD = 1.63$) than the women's organization, $p < .001$. However, there was no effect of condition or interaction between condition and organization on local norms, $ps > .29$.

For general norms, there was a significant main effect of organization, $F(6, 918) = 24.19, MSE = 1.32, p < .001$. Focused tests between the women's organization and all

other organizations indicated that participants perceived greater general agreement with their cuts to the Jewish organization ($M = 4.56$, $SD = 1.33$) than the women's organization ($M = 4.10$, $SD = 1.29$), $p < .001$. Participants also perceived more general agreement with their cuts to the women's organization than the study abroad program ($M = 3.43$, $SD = 1.64$; $p < .001$) or Black student union ($M = 3.77$, $SD = 1.40$; $p = .001$), but more general agreement for participants' cuts from cinema club ($M = 4.78$, $SD = 1.59$) than the women's organization, $ps < .001$. However, there was no effect of condition or interaction between condition and organization on general norms, $ps > .58$.

Because past research on the effects of sexist jokes on perceived norms was conducted on men (Ford et al., 2008), I conducted a 3 (condition) \times 2 (gender) between subjects ANOVA on perceived local and general norms. For local norms, there was a marginal interaction between condition and gender, $F(2, 155) = 2.70$, $MSE = 2.21$, $p = .07$; all other $ps > .15$. As shown in Table 6, men and women perceived similar local norms in the control condition. In the confrontation conditions, however, men perceived less agreement with their cuts to the women's organization than did women. For general norms, there was a significant main effect of gender, $F(1, 151) = 3.93$, $MSE = 1.67$, $p < .05$; all other $ps > .61$. Again, men ($M = 3.81$, $SD = 1.22$) perceived less general agreement with their cuts to the women's organization than did women ($M = 4.22$, $SD = 1.32$).

Attitudes Toward Organizations. To examine the effect of condition on participants' attitudes toward organizations, a 3 (condition, between subjects) \times 7 (organization, within subjects) mixed ANOVA was estimated. There was a significant main effect of organization on attitudes, $F(6, 936) = 66.40$, $MSE = 0.76$, $p < .001$.

Focused tests ($p = .001$) between the women's organization and all other organizations indicated that participants had less positive attitudes toward the women's organization ($M = 4.51, SD = 1.17$) compared to four other organizations (Agricultural: $M = 5.02, SD = 1.05$; Safe Arrival: $M = 5.19, SD = 1.20$; Study abroad: $M = 5.60, SD = 1.10$; Black Union: $M = 4.76, SD = 1.16$). Participants had more positive attitudes toward the women's organization than the cinema organization ($M = 3.90, SD = 1.26$). However, there was no effect of condition or interaction between condition and organization, $ps > .14$.

Time 3 Summary. Consistent with Hypothesis 1b that confrontation (vs. no confrontation) decreases discrimination, participants in the confrontation-only and confrontation+elaboration conditions discriminated against women less than participants in the control condition did. However, contrary to Hypothesis 2, there was no additional effect of elaborating (vs. not elaborating) on confrontation; participants in the confrontation conditions cut equivalent amounts from a women's organization on the budget task. Also, contrary to Hypothesis 2, individual differences in need for cognition and its interaction with condition were not related to discrimination. Finally, confrontation shifted perceived social norms. Specifically, men in confrontation conditions perceived less local agreement with their budget cuts to a women's organization than did men in the control condition.

Longitudinal Dependent Variables: Analytic Rationale

Hypotheses 1, 2, and 3 concerned the longitudinal effects of confrontation (vs. no confrontation) and/or elaboration (vs. no elaboration) of confrontation messages on attitude and feeling variables (i.e., benevolent sexism, ambivalent sexism, and feeling

thermometers for subtypes of women). I examined these hypotheses using a multilevel approach for several reasons. Because the same participants completed the measures across three time points, there was dependency in the data that can be modeled through either repeated measures ANOVA or multilevel modeling. However, there were unequal intervals between time points. A traditional ANOVA framework collapses across these unequal intervals (i.e., forcing all data points together at “Time 3”), whereas a multilevel approach models change as a function of exact time that has passed since the manipulation (i.e., 1-14 days). In addition, there was missing data, and ANOVA examines only complete data (i.e., participants present at Time 1, 2, and 3), whereas multilevel modeling allows for some use of incomplete data that ANOVA would eliminate (i.e., participants who completed only Time 1 and 2 or Time 2 and 3). Finally, although both ANOVA and multilevel approaches allow for the inclusion of covariates, analysis of covariance (ANCOVA) assumes that the relationship between the covariate and the dependent variable is the same across groups (i.e., homogeneity of regression slopes). However, multilevel modeling does not make this assumption, and the effect of a covariate can depend on condition, for example. Because a multilevel approach is advantageous for unbalanced time, incomplete data, and covariates, I investigated my hypotheses concerning change over time using multilevel modeling.

In order to examine the overall pattern of individual differences in longitudinal dependent variables, SAS PROC MIXED was used to estimate a series of models. Initial analyses provided descriptive information about the data. Next, unconditional models (i.e., including no predictors) were estimated in order to properly situate time in the models. Missingness analyses then were undertaken to examine whether participants who

completed Time 3 differed from participants who did not, which allowed for any differences to be accounted for in the conditional models. Next, multivariate models were undertaken to examine whether the patterns of change in the feeling thermometer items were similar across subtypes, which indicated whether a composite variable is appropriate for these items when testing conditional models. Finally, conditional models were estimated to examine the effects of predictors including condition, gender, and need for cognition and directly test Hypothesis 1, 2, and 3.

Across all of these models, maximum likelihood (ML) was used to assess the significance of random or fixed effects, which is appropriate because of the larger sample size in the study. Degrees of freedom were estimated using the Satterthwaite method. Participants' benevolent sexism, hostile sexism, and feeling thermometers toward subtypes of women were measured before the manipulation (Time 1), immediately following the lab manipulation (Time 2), and 1-14 days after manipulation (Time 3), leading to three total occasions of measurement. Time was centered at Time 2; thus, intercepts represent sexism or feelings measured immediately following the lab manipulation in all models. However, time could be treated in the model as either measurement occasion (i.e., -1, 0, 1) or days before (for Time 1) and after (for Time 3) the lab manipulation. Both options were assessed in the analyses that follow.

First, I examined whether there was any change over time in longitudinal variables. A random intercept model was specified to produce the interclass correlation (ICC), which indicated the proportion of the variation that exists between and within persons. The ICC for benevolent and hostile sexism was .73 and .75, respectively, which indicated that 73-75% of variation in sexism is between persons and 27% or 25% of the

variation is within person (i.e., over time). The ICC range across feeling thermometers toward subtypes of women was .36 to .60, which indicated that 36-60% of the variation in feeling thermometers is between persons and 40-64% of the variation is within person. Thus, feeling thermometers were more variable over time than sexism.

Second, a random intercept only model with saturated means was specified as a baseline model. This model was used to create a plot of the individual means for each longitudinal dependent measure. Individual trajectories seem to indicate change (increase or decrease) at Time 2, as shown in Figure 5 for hostile sexism. Although unconditional models of change centered at Time 2 were pursued, alternative conceptions of time were considered before adding predictors, because the effects of predictors are specified as a function of the time parameters in the model. Piecewise rather than polynomial models were used because the lab manipulation was expected to be the source of any change, and time was not equal interval in the study. Two piecewise variables were created; one represented the slope between Time 1 and 2 (i.e., slope12), and another represented the slope between Time 2 and 3 (i.e., slope23).

Unconditional Models for Longitudinal Dependent Variables

Covariance Structure. Because the same participants completed the same measures (i.e., ASI and feeling thermometers) over three time points, there was dependency in the data. To ensure that dependency was adequately taken into account when producing standard estimates for the fixed effects, the covariance matrix was modeled by estimating three alternative covariance structures across the seven longitudinal dependent variables (i.e., benevolent sexism, hostile sexism, and five feeling thermometers for subtypes of women). For each dependent variable, the unstructured

covariance model was the baseline for comparison to compound symmetry (CS; i.e., both variance and covariance as constant) and compound symmetry-heterogeneous (CSH; i.e., separate variances but constant covariance) models. ML likelihood ratio tests, AIC, and BIC were used to compare these nested variance structures.

Across measures of sexism, the unstructured covariance model produced significantly better fit than the CS model, ML deviance difference (4) = 16.90, $p < .01$, and (4) = 44.60, $p < .001$, respectively for benevolent and hostile sexism. The unstructured covariance model also produced significantly better fit than did the CSH model, ML deviance difference (2) = 16.00, $p < .001$, and (2) = 41.60, $p < .001$, respectively for benevolent and hostile sexism. Across four feeling thermometer items, the unstructured covariance model produced significantly better fit than CS (ML deviance difference ranging from [4] = 17.60 to 43.10, $p \leq .001$) or CSH (ML deviance difference ranging from [2] = 9.80 to 13.30, $p < .01$). The exception was feelings toward feminists, for which CS was adequate compared to unstructured covariance, ML deviance difference (4) = 4.60, $p = .33$. However, to ensure that any differences in effects were not due to the underlying covariance structures, the unstructured covariance model was used for all dependent variables; unstructured covariance fits all dependent variables perfectly but used more degrees freedom than necessary for the feminist feeling thermometer for which a simpler, more restrictive structure fit adequately. Because unstructured covariance models allow the variances and covariances over time to be what they were, no random effects of time were necessary.

Real Versus Balanced Time. Time in these models could be estimated in terms of occasion of measurement (e.g., -1, 0, 1 respectively for Time 1, 2, and 3) and/or time

in days before/after Time 2. To examine how to best represent time in the model, a series of unconditional models was estimated in which real time in days was included as a moderator. These models include only participants with complete data in order to make comparison possible across models (i.e., differences were not due to varying numbers of participants at time points). The baseline model included the two piecewise variables and a random intercept. Including days between Time 1 and 2 (centered) as a moderator (i.e., including its main effect and interactions between each piecewise slope) did not improve model fit for benevolent or hostile sexism, ML deviance difference (3) = 1.50, $p = .68$, and (3) = 2.60, $p = .46$, respectively. Including days between Time 1 and 2 as a moderator also did not improve model fit for feeling thermometer items, ML deviance difference (3) = 0.40 to 3.80, $p > .28$. Thus, change between Time 1 and 2 was not dependent on the amount of time that passed between the pre-test and lab manipulation.

Compared to the baseline model with two piecewise slopes and a random intercept, including days between Time 2 and 3 (centered) as a moderator did not improve model fit for benevolent or hostile sexism, ML deviance difference (3) = 3.20, $p = .36$, and (3) = 6.00, $p = .11$, respectively. Including days between Time 2 and 3 as a moderator also did not improve model fit for feeling thermometer items, ML deviance difference (3) = 0.40 to 4.60, $p > .20$. Thus, change between Time 2 and 3 was not dependent on the amount of time that passed between the lab manipulation and post-test, which challenged the underlying assumption of Hypothesis 3 that the effect of the lab manipulation fades over time.

These analyses indicated that representing time more coarsely through occasion of measurement does not lose information compared to representing time more exactly

through days before/after Time 2. As such, time was balanced in all following models, with occasion of measurement used to create piecewise slopes between Time 1 and 2 as well as Time 2 and 3. In addition, using occasion of measurement to represent time allowed all data (vs. only complete data) to be used in the following models.

Missingness Analysis for Longitudinal Dependent Variables

To examine whether participants who completed Time 3 differed from participants who did not return for Time 3, pattern mixture models were estimated. A dummy coded variable was created that represented whether participants had completed Time 3 (i.e., did not complete = 0, completed = 1), which was included as a covariate in the unstructured model with piecewise slopes. Tests of fixed effects indicated that the effect was not significant for benevolent or hostile sexism, $b = -0.06$, $SE = 0.09$, $p = .47$, and $b = -0.10$, $SE = 0.10$, $p = .30$, respectively.

Similarly, the effect of whether participants completed Time 3 was not significant for feelings toward career women and feminists, $ps > .27$. However, the predictor was significant for feelings toward women, $b = 3.85$, $SE = 1.65$, $p = .02$; homemakers, $b = 3.81$, $SE = 1.87$, $p < .05$; and party girls, $b = -6.18$, $SE = 2.65$, $p = .02$. Participants who completed Time 3 had more positive feelings toward women and homemakers as well as more negative feelings toward party girls than participants who did not complete Time 3.

Next, the interaction between completion of Time 3 (dummy coded) and the piecewise slope indexing change from Time 1 to Time 2 was added to the models. The interaction was not significant for benevolent or hostile sexism, $b = -0.03$, $SE = 0.08$, $p = .68$, and $b = 0.13$, $SE = 0.09$, $p = .15$, respectively. Similarly, none of the interactions were significant in separate tests of responses to the feeling thermometer, all $ps > .19$.

The significant main effects of whether participants completed Time 3 were carried through to multivariate and conditional models for feelings toward women, homemakers, and party girls, which controlled for these effects related to completion of Time 3. (Thus, any conditional effects were not due to the more positive feelings toward women shown by participants who completed Time 3.)

Multivariate Analyses of Longitudinal Dependent Variables

To examine whether changes in feelings toward women depended on the specific subtype of women, multivariate analyses that directly compare subtypes were necessary. If patterns of change were similar across subtypes, a composite variable for feelings toward women was appropriate in conditional models. However, if patterns of change were different across subtypes, feelings toward specific subtypes of women, rather than a composite variable, should be used to examine conditional effects.

A model was estimated with a categorical variable that represented which subtype of women and its with each slope (i.e., Time 1 to 2 and Time 2 to 3). ESTIMATE statements were used to obtain focused tests between feelings toward subtypes of women. The intercepts, slopes, and focused tests for each subtype of women are shown in Table 7. Focused tests indicated that feelings at Time 2 (i.e., the fixed intercept) were more positive for women than career women; career women than homemakers; and homemakers than feminists and party girls, who had equivalent feeling ratings.

However, the focus of this analysis is patterns of change over time. Focused tests for slope¹² indicated that the significant increase in positive feelings toward women, career women, and homemakers from Time 1 to 2 differed from the non-significant (or marginally negative) change in feelings toward feminists and party girls. Focused tests

for slope23 indicated that feelings toward feminists became more positive from Time 2 to 3, which differs from the non-significant (or marginally negative) change in feelings toward women, career women, homemakers, and party girls. The patterns of change in attitudes were different for feminists (i.e., from Time 1 to 2 and Time 2 to 3) and party girls (i.e., from Time 1 to 2) than the other subtypes, which suggested that conditional effects for feelings toward feminists and party girls should be examined separately. However, this analysis also indicated that a composite variable of the other subtypes (i.e., women, career women, and homemakers) was appropriate. Thus, conditional effects for feeling thermometers were subsequently examined for feelings toward feminists, party girls, and a composite variable for women.

Conditional Models for Longitudinal Dependent Variables

To examine Hypothesis 1, 2, and 3 concerning attitude change, the time-invariant (level-2) predictor for condition and its interactions with each slope were added to the best fitting unconditional piecewise model selected from preliminary analyses. Thus, the model estimated for each longitudinal dependent variable (i.e., benevolent sexism, hostile sexism, feelings toward women, feelings toward feminists, and feelings toward party girls) included a slope between Time 1 and 2, slope between Time 2 and 3, condition, the interaction between slope12 and condition, and the interaction between slope23 and condition. In addition, feelings toward women and feelings toward party girls included the effect of whether participants completed Time 3, given the result of the missingness analyses. Condition was parameterized such that the control is the reference (control vs. confrontation-only; control vs. confrontation+elaboration). ESTIMATE statements were used to obtain the missing contrast (confrontation-only vs. confrontation+elaboration).

For benevolent and hostile sexism, adding the effect of condition and its interaction with slopes did not improve model fit, ML deviance difference (3) = 2.60, $p > .45$, and (3) = 2.10, $p > .55$, respectively. As shown in Table 8, there also was no significant change in benevolent or hostile sexism from Time 1 to 2 or Time 2 to 3. However, there was a marginal decrease in hostile sexism between Time 1 and 2 for the confrontation+elaboration condition. There also were no significant focused tests between conditions for the intercept or piecewise slopes, $ps > .15$. Although I predicted through Hypothesis 1 and 2 that confrontation (vs. no confrontation) and elaboration (vs. no elaboration) of confrontation messages would decrease sexism, there is little support for these predictions. However, the (marginal) decrease in hostile sexism from Time 1 to 2 for the confrontation+elaboration condition is consistent with Hypothesis 2.

For feelings toward women from Time 1 to 2, including the effects of condition did not significantly improve model fit, ML deviance difference (3) = 5.10, $p > .16$. Nevertheless, the simple slopes in Table 8 show that participants in the confrontation-only and confrontation+elaboration conditions were more positive toward women from Time 1 to 2. However, there was no significant change in feelings toward women for participants in the control condition. Focused tests for the slope between Time 1 and 2 by condition further indicated that the confrontation-only slope was significantly more positive than the control, $b = 7.36$, $SE = 2.43$, $p < 0.01$; this finding is consistent with Hypothesis 1a, as participants' feelings toward women became more positive for the confrontation-only condition than they did in the control condition. In addition, the slope between Time 1 and 2 for the confrontation+elaboration condition was marginally more positive than the control condition, $b = 4.22$, $SE = 2.45$, $p = 0.09$. However, the

confrontation conditions did not significantly differ from each other, $p > .18$, which is contrary to Hypothesis 2 that elaboration increases attitude change in the short-term compared to the confrontation-only condition.

Participants in the confrontation-only condition were less positive toward women from Time 2 to 3. Focused tests for the slope between Time 2 and 3 by condition further indicated that the confrontation-only slope was marginally more negative than the control, $b = -4.06$, $SE = 2.39$, $p = .09$. Although feelings toward women increased from Time 1 to 2 in confrontation conditions, they decreased from Time 2 to 3 for the confrontation-only condition. Additional analyses indicated that participants in the confrontation-only condition had marginally more positive attitudes toward women at Time 3 than Time 1, $b = 3.92$, $SE = 2.02$, $p = .05$; thus, the significant increase in feelings toward women for participants in the confrontation-only condition from Time 1 to 2 was greater in magnitude than the (smaller but significant) decrease in feelings toward women from Time 2 to 3.

For feelings toward feminists from Time 1 to 2, including the effects of condition did not significantly improve model fit, ML deviance difference (3) = 4.40, $p > .22$. The simple slopes in Table 8 show that participants in the control condition were marginally more negative toward feminists from Time 1 to 2. However, no focused tests for condition were significant for the slope between Time 1 and 2, $ps > .32$. The simple slopes also indicated that participants in the confrontation-only condition became more positive toward feminists from Time 2 to 3 for the confrontation-only condition. However, no focused tests for condition were significant for the slope between Time 2 and 3, $ps > .27$. Thus, feelings toward feminists did not improve in the confrontation

conditions (vs. control) in the short-term, contrary to Hypothesis 1 and 2, but confrontation (vs. no confrontation) increased feelings toward feminists in the long-term.

For feelings toward party girls from Time 1 to 2, including the effects of condition significantly improved model fit, ML deviance difference (3) = 13.50, $p < .001$. The simple slopes in Table 8 show that participants in the control and confrontation+elaboration conditions became more negative toward party girls from Time 1 to 2. In addition, participants in the confrontation-only condition became marginally more positive toward party girls from Time 1 to 2. Focused tests by conditions for change between Time 1 and 2 further indicated that the confrontation-only slope was significantly more positive than the control, $b = 10.27$, $SE = 3.67$, $p < 0.01$, and confrontation+elaboration conditions, $b = 10.09$, $SE = 3.60$, $p < 0.01$, which did not differ from each other, $p > .95$. No simple slopes or condition focused tests were significant for the slope between Time 2 and 3, $ps > .30$. Thus, although feelings toward party girls decreased for the control and confrontation+elaboration conditions, feelings toward party girls marginally increased for the confrontation-only condition; these differences were maintained over time, as there was no significant change from Time 2 to 3. This pattern of effects is consistent with Hypothesis 1a, because confrontation (vs. no confrontation) caused less negative feelings toward party girls, but inconsistent with Hypothesis 2, because elaborating (vs. not elaborating) on confrontation messages did not cause less negative feelings toward party girls.

Individual Differences: Gender. Next, I examined whether participant gender (i.e., a time invariant, level-2 predictor, with 0 = men, 1 = women) qualified the results of condition and/or change over time. Specifically, I added participant gender, gender's

interaction with condition, gender's interaction with each slope, and the interactions between gender, condition, and each slope. These models included only participants with complete data in order to be directly comparable. I report significant effects of gender from the model including all gender effects in the text, and the resulting main effects are specific to Time 2. If the model improved according to ML deviance difference tests and/or the effects are significant, the effects were carried forward. Non-significant effects of gender are removed from the model one at a time, moving from higher- to lower-order effects. Final models for gender are shown in Table 9; if no interactions were significant, the main effect in these models represented the effect of gender across time.

For benevolent sexism, including gender and its interactions did not improve model fit, ML deviance difference (9) = 13.90, $p > .12$. There was a significant main effect of gender, $b = -0.46$, $SE = 0.24$, $p < .01$, which indicated that women endorsed benevolent sexism less than men did at Time 2; all other effects of gender $ps > .35$. Because gender remained a significant predictor after nonsignificant higher order interactions were removed individually from the model, gender was maintained in the model for benevolent sexism, as shown for the final gender model in Table 9.

For hostile sexism, including gender and its interactions improved model fit, ML deviance difference (9) = 24.90, $p < .01$. There was a significant main effect of gender, $b = -0.57$, $SE = 0.29$, $p < .01$, which indicated that women endorsed hostile sexism less than men did at Time 2. There also were marginal interactions between gender, condition, and change between Time 1 and 2 as well as gender, condition, and change between Time 2 and 3, $ps < .07$; all other effects of gender $ps > .39$. Because gender remained significant

after nonsignificant higher order interactions were removed individually from the model, the effect of gender was maintained in the model for hostile sexism, as shown in Table 9.

For feelings toward women, including gender and its interactions did not improve model fit, ML deviance difference (9) = 8.60, $p > .47$. There was a marginal main effect of gender, $b = 4.24$, $SE = 4.26$, $p < .10$, which indicated that women had more positive feelings toward women than men did at Time 2; all other effects of gender $ps > .15$. Because the effect of gender remained marginal after nonsignificant higher order interactions were removed individually from the model, the model for feelings toward women did not include the effect of gender.

For feelings toward feminists, including gender and its interactions improved model fit, ML deviance difference (9) = 25.40, $p < .01$. There was a significant main effect of gender, $b = 27.32$, $SE = 6.91$, $p < .001$, which indicated that women had more positive feelings toward feminists than men did at Time 2; all other effects of gender $ps > .12$. Because the effect of gender remained significant after nonsignificant higher order interactions were removed individually from the model, the effect of gender was kept in the model for feelings toward feminists, as shown in Table 9.

For feelings toward party girls, including gender and its interactions improved model fit, ML deviance difference (9) = 17.40, $p < .05$. There was a significant effect of gender, $b = -13.73$, $SE = 7.49$, $p = .02$, which indicated that women had more negative feelings toward party girls at Time 2 than men did. There also was a marginal interaction between gender and change from Time 1 to 2, $b = 9.34$, $SE = 7.24$, $p = .09$, which indicated that women's feelings toward party girls became more positive from Time 1 to 2 than did men's; all other effects of gender $ps > .15$. Because the interaction between

gender and slope₁₂ became significant after nonsignificant higher order interactions were removed individually from the model, the effects of gender and the interaction between gender and change from Time 1 to 2 were maintained in the model for feelings toward party girls. As shown in Table 9 for the final gender model, the main effect of gender indicated that women's feelings toward party girls were 9.47 more negative than were men's feelings at Time 2. The Gender × Slope₁₂ interaction indicated that men's feelings toward party girls decreased by 13.79 from Time 1 to 2, but women's feelings toward party girls decreased by 5.61 ($-13.79 + 8.18$) from Time 1 to 2.

Across these analyses, gender did not qualify the effects of confrontation (vs. no confrontation) or elaboration (vs. no elaboration). Instead, gender had a direct effect on participant's intercepts or means at Time 2 for benevolent sexism, hostile sexism, feelings toward feminists, and feelings toward party girls. Specifically, men endorsed both benevolent and hostile sexism more and had more negative feelings toward feminists as well as more positive feelings toward party girls than did women. Participant gender also influenced change in feelings toward party girls from Time 1 to 2. Both men's and women's feelings toward party girls became more negative from Time 1 to 2, but the decrease in women's feelings toward party girls was smaller than was men's.

Individual Differences: Need for Cognition. Finally, I investigated how individual differences in need for cognition (i.e., a time invariant, level-2 predictor measured prior to the manipulation at Time 2) related to change in attitudes and feelings toward women and condition, because need for cognition is relevant to Hypothesis 2 concerning the effect of elaborating (vs. not elaborating) on confrontation messages. Models were estimated with need for cognition (centered at its midpoint), its interaction

with each slope, and its interaction with condition. These models included only participants with complete data in order to be directly comparable. If the model improved according to ML deviance difference tests and the effects were significant, the effects were carried forward. (Non-significant effects were dropped from the model except in the case of interactions. If one interaction with a slope was significant, the main effect and other slope interaction were retained in the model for interpretability.)

For benevolent sexism, including the effects of need for cognition did not improve model fit, ML deviance difference (5) = 3.10, $p > .68$. There also were no significant effects of need for cognition, $p > .15$. Thus, the final model did not include any effects of need for cognition, as shown in Table 10.

For hostile sexism, including the effects of need for cognition marginally improved model fit, ML deviance difference (5) = 9.60, $p = .09$. Need for cognition was significant, $b = -0.33$, $SE = 0.14$, $p < .001$, which indicated that hostile sexism at Time 2 decreased by 0.33 for each point above the midpoint on need for cognition. Thus, the final model shown in Table 10 included the main effect of need for cognition.

For feelings toward women, including the effects of need for cognition did not improve model fit, ML deviance difference (6) = 6.20, $p > .28$. However, there was a marginal interaction between need for cognition and change from Time 2 to 3, $b = 1.96$, $SE = 1.11$, $p = .08$, which indicated that a one point increase in need for cognition makes change from Time 2 to 3 marginally more positive. Thus, those higher in need for cognition had a marginally greater increase in feelings toward women from Time 2 to 3, consistent with Hypothesis 2. However, the interaction between need for cognition and change from Time 2 to 3 remained marginal when other nonsignificant effects of need for

cognition were removed from the model. Therefore, the final model shown in Table 10 did not include need for cognition.

For feelings toward feminists, including the effects of need for cognition improved model fit, ML deviance difference (5) = 14.10, $p = .01$. Although the effect of need for cognition was not significant, $b = 4.12$, $SE = 3.39$, $p > .22$, a one point increase from the midpoint on need for cognition increased feelings toward feminists by 4.12 at Time 2. Although there was not a significant interaction between need for cognition and change from Time 1 to 2, $b = -0.62$, $SE = 2.26$, $p > .78$, the interaction was significant for change from Time 2 to 3, $b = 4.69$, $SE = 1.96$, $p = .02$. The latter interaction indicated that each point above the midpoint on need for cognition increased feelings toward feminists from Time 2 to 3 by 4.69, which supports Hypothesis 2. Thus, the final model shown in Table 10 included the main effect of need for cognition and its interactions with slopes.

For feelings toward party girls, including the effects of need for cognition marginally improved model fit, ML deviance difference (5) = 10.70, $p = .06$. Need for cognition significantly contributed, $b = -8.92$, $SE = 3.84$, $p < .05$, which indicated that a one point increase from the midpoint on need for cognition decreased feelings toward party girls by 8.92 at Time 2. All other effects of need for cognition were not significant, $p > .29$. Thus, the final model shown in Table 10 included only the main effect of need for cognition.

Summary for Longitudinal Dependent Variables. Analyses indicated that there was no significant change from Time 1 to 2 or Time 2 to 3 in benevolent and hostile sexism overall or within specific conditions. This finding is contrary to Hypotheses 1a

and 2 concerning reductions in sexism from Time 1 to 2. However, feelings toward women (but not feminists or party girls) became more positive from Time 1 to 2 for the confrontation conditions (vs. control), consistent with Hypothesis 1a. Contrary to Hypothesis 2, however, feelings toward women did not become more positive from Time 1 to 2 for the confrontation+elaboration condition.

Although participants in the confrontation-only condition became more positive toward women from Time 1 to 2, these participants became less positive toward women from Time 2 to 3. While this pattern of results suggests a rebound effect whereby feelings toward women return to previous levels for participants in the confrontation-only condition, the increase in positive feelings toward women from Time 1 to 2 is of larger magnitude than the decrease from Time 2 to 3; analyses indicated that participants in this condition had marginally more positive attitudes toward women at Time 3 than Time 1. Therefore, there are some residual positive effects of confrontation (vs. no confrontation) on feelings toward women.

The pattern for feelings toward feminists was different than that of feelings toward women. Participants in the control condition became marginally more negative toward feminists from Time 1 to 2, and this marginal decrease in feelings was maintained from Time 2 to 3, where there was no significant change. However, participants on the confrontation-only condition did not have significant change in feelings toward feminists from Time 1 to 2, but these participants' feelings became less negative toward feminists from Time 2 to 3. This " sleeper effect " for confronting sexism (vs. not confronting) on feelings toward feminists may be interpreted as support for Hypothesis 1. Specifically, participants who observed a confrontation of sexism eventually had more positive

attitudes toward feminists than participants who did not observe confrontation. However, feelings toward feminists did not change from Time 1 to 2 or Time 2 to 3 for participants in the confrontation+elaboration condition, which suggests that elaborating (vs. not elaborating) on confrontation messages may have undermined the sleeper effect for the confrontation-only condition on feelings toward feminists.

The pattern across conditions and over time for feelings toward party girls differed from that of both feelings toward women and feminists. Although the control and confrontation+elaboration condition became more negative from Time 1 to 2, the confrontation-only condition became marginally more positive from Time 1 to 2. From Time 2 to 3, there were no significant changes in feelings toward party girls for any condition, which suggests that this pattern was maintained over time. Again, this pattern provides some support for Hypothesis 1a, because confrontation (vs. no confrontation) resulted in less negative feelings toward party girls from Time 1 to 2. Overall, the patterns of change in feelings toward women, feminists, and party girls provide show that confrontation (vs. no confrontation) can reduce negative feelings toward women, which is consistent with Hypothesis 1. Further, analyses indicated that the effect of confrontation (vs. no confrontation) is not dependent on participant gender.

Hypothesis 2 concerned the effect of elaboration, and after finding few effects of elaborating (vs. not elaborating) on confrontation messages on feelings toward women over time, I extended this hypothesis to individual differences in need for cognition. The analyses examining this factor produced little evidence for the hypothesis that elaborating (vs. not elaborating) on confrontation messages reduces prejudice. Contrary to this extension of Hypothesis 2, need for cognition did not influence change over time in

benevolent sexism, hostile sexism, feelings toward women, or feelings toward party girls. However, there was a significant interaction between need for cognition and change over time for feelings toward feminists. Consistent with Hypothesis 2, greater need for cognition was related to more positive feelings toward feminists from Time 2 to 3.

Finally, there is no support for Hypothesis 3 concerning longer-lasting attitude change for the confrontation+elaboration condition compared to the confrontation-only condition. Instead, attitude and feeling change did not depend on the length of time between Time 2 and 3. Still, feelings toward women became more positive from Time 1 to 2 for participants in both confrontation conditions, whereas feelings toward women decreased significantly from Time 2 to 3 for participants in the confrontation-only condition but not the confrontation+elaboration condition. This finding suggests that elaborating (vs. not elaborating) on confrontation messages may help maintain the increase in attitudes toward women seen from Time 1 to 2 at Time 3.

However, examining the cumulative effects as well as the magnitude of change over time suggests that participants in both confrontation conditions have a similar increase in feelings toward women from Time 1 to 3. Additional analyses support this proposition, because change from Time 1 to 3 was not different between confrontation conditions, $b = -2.14$, $SE = 2.51$, $p > .39$. That is, participants in the confrontation-only condition increased feelings toward women from Time 1 to 2 but slightly decreased from Time 2 to 3; still, there was a net increase in feelings toward women for participants in the confrontation-only condition from Time 1 to 3. Participants in the confrontation+elaboration condition, on the other hand, increased in feelings toward women from Time 1 to 2, but there is no significant change from Time 2 to 3. Thus, the

cumulative effects of these changes from Time 1 to 3 are similar for both confrontation conditions, which is contrary to Hypotheses 2 and 3.

CHAPTER 5

Discussion

Although previous research has shown that confrontation reduces prejudice and discrimination (e.g., Czopp & Monteith, 2003; Czopp et al., 2006; Hillard & Ryan, 2011; Mallet & Wagner, 2011), the effect of speaking up about prejudice has rarely been examined over time. In addition, the processes underlying the prejudice-reducing effects of confrontation on perpetrators and observers are not known. The purpose of the present study was to fill these gaps in the literature by examining whether confronting prejudice (vs. not confronting) reduces prejudice in observers over time as well as whether elaborating (vs. not elaborating) on confrontation messages further reduces prejudice. Based on an integration of the confronting prejudice and persuasion theory literatures, I developed and tested three hypotheses about the effects of confrontation and elaboration on observers' attitudes and behavior in the short- and longer-term. Hypothesis 1 concerned the effect of confronting (vs. not confronting) prejudice; I expected that confronting would decrease sexism in the short-term and discrimination in the longer-term. Hypothesis 2 concerned the effect of elaborating (vs. not elaborating) on confrontation messages; I expected that elaborating on confrontation messages would decrease sexism and discrimination more than confrontation alone. Hypothesis 3 concerned the effect of elaborating (vs. not elaborating) on confrontation messages over time; I expected participants who elaborated on confrontation messages to maintain the reduction in prejudice over a longer period of time than participants who did not elaborate on confrontation messages.

To test these hypotheses, college students who had completed pretest measures of attitudes and feelings towards women (i.e., Time 1) were exposed to sexist jokes, which can cause discrimination (Ford et al., 2008). I manipulated whether a speaker either negatively evaluated the jokes or confronted prejudice in the jokes; I also manipulated whether participants wrote a control essay or an essay elaborating on the confrontation message. Thus, 361 participants were randomly assigned to one of three conditions: control (i.e., no confrontation with control essay), a confrontation without elaboration (i.e., confrontation with control essay), or a confrontation with elaboration (i.e., confrontation with elaboration essay). Participants then completed the same measures of attitudes and feelings towards women immediately after the confrontation message (i.e., Time 2) and after a 1-14 day delay through an ostensibly unrelated study (i.e., Time 3). My hypotheses and the results of the analyses that tested them are summarized in the sections that follow.

Hypothesis 1: The Effect of Confrontation (vs. No Confrontation)

Based on previous research, I expected confronting sexism to cause less hostile attitudes toward women in the short-term (Hypothesis 1a) and less discrimination in the longer-term (Hypothesis 1b) in observers than not confronting sexism. There was some evidence to support this hypothesis. Specifically, longitudinal analyses indicated that participants' feelings toward women in the confrontation conditions became more positive from Time 1 to 2, whereas there was no change in feelings toward women for participants in the control condition. Participants in the control condition, though, became more negative toward party girls from Time 1 to 2, whereas participants in the confrontation-only condition became marginally more positive toward party girls from

Time 1 to 2. These findings support Hypothesis 1a, because feelings toward women and party girls increased from Time 1 to 2 for participants in the confrontation-only condition but not for participants in the control condition. In addition, these effects of confrontation (vs. no confrontation) were not dependent on participant gender. However, contrary to Hypothesis 1a, there were no significant changes from Time 1 to 2 in benevolent sexism, hostile sexism, or feelings toward feminists for any condition.

Although early research on confronting prejudice focused mostly on perpetrators' affective responses to confronting prejudice (e.g., Czopp & Monteith, 2003), one study indicated that confronting perpetrators can reduce racism (Czopp et al., 2006). However, I did not find that observing a confrontation (vs. no confrontation) of sexist jokes reduced sexism. The present study differs from Czopp et al.'s in two important ways. The present study examined *observer* outcomes of confronting *sexism*, whereas Czopp et al. examined *perpetrator* outcomes of being confronted about *racism*. However, other findings were more parallel across these studies. Czopp et al. examined prejudice reduction through ratings of racist jokes. They found that confrontation (vs. no confrontation) did not decrease participants' ratings of racist jokes. Similarly, I did not find that confrontation (vs. no confrontation) reduces liking of sexist jokes.

More broadly, though, I found that confrontation (vs. no confrontation) influenced feelings toward women but not the endorsement of sexism itself. Prejudice, like other attitudes, involves three interrelated components—*affect*, *behavior*, and *cognition*. Previous research on confronting prejudice has examined overall racism as well as stereotypic behavior and beliefs, which addresses behavioral and cognitive components of prejudice (Czopp et al., 2006). The present study shows that the affective component

of prejudice (i.e., negative evaluations) can also be reduced through confronting (vs. not confronting) prejudice. It also examined discriminatory behavior, which I turn to next.

Participants in the control and confrontation-only conditions donated similar amounts to a women's organization immediately following the manipulations. However, 1-14 days following the manipulations, participants in the control condition discriminated against women more than participants in the confrontation-only condition. This finding provides full support for Hypothesis 1b, because observing a confrontation of sexism decreased discrimination against women in the long-term compared to not observing a confrontation of sexism.

Past research on the behavioral effects of confrontation indicated that confronted (vs. not confronted) perpetrators stereotyped less (Czopp et al., 2006) and used less biased language (Mallet & Wagner, 2011). The only behavioral measures of confrontation for observers rather than perpetrators of prejudice indicated that people who were likely to have observed confrontations of prejudice were more likely to sign an anti-prejudice petition (Paluck, 2011). The present research extends this research by showing observers of a confrontation of sexist jokes were less discriminatory towards women than observers of sexist jokes that were not confronted.

Further, the measure of discrimination in the present study occurred 1-14 days after the confrontation, which addresses an important limitation of previous research on confronting prejudice. That is, nearly all research on confronting prejudice as well as most research on prejudice reduction (e.g., Paluck, 2006; Paluck & Green, 2009) examines attitude change only in the immediate context of the laboratory and rarely examines behavioral outcomes. This study examines behavior over time as well as

beyond the laboratory and shows that confrontation (vs. no confrontation) decreases discrimination in a more real-world context and on an ecologically valid task of distributing budget cuts.

Hypothesis 2: The Effect of Elaborating (vs. Not Elaborating) on Confrontation

Messages

Because elaboration on high quality messages can increase persuasion, I expected elaborating (vs. not elaborating) on confrontation messages to cause less hostile attitudes toward women in the short-term (Hypothesis 2a) and less discrimination in the long-term (Hypothesis 2b) in observers. There was only limited support for this hypothesis.

Compared to participants in the control condition, participants in the confrontation+elaboration condition donated more to a women's organization. However, there was no significant difference between the amounts donated for participants the confrontation-only and confrontation+elaboration conditions; instead, it appears to be the combined effects of confrontation and elaboration that increased donation rather than the effect of elaboration alone, which is contrary to Hypothesis 2.

Participants in the confrontation conditions discriminated less against women than participants in the control condition 1-14 days following the manipulations. Again, however, there was no significant difference between the budget cut amount from a women's organization for participants in the confrontation-only and confrontation+elaboration conditions. This finding is contrary to Hypothesis 2b, because elaborating (vs. not elaborating) on confrontation messages did not cause less discrimination in the long-term.

In addition, longitudinal analyses did not support Hypothesis 2a, because there was not greater attitude or feeling change from Time 1 to 2 for participants in the confrontation+elaboration condition than participants in the confrontation-only condition. In fact, feelings toward party girls became more negative from Time 1 to 2 for participants in the confrontation+elaboration condition, whereas feelings toward party girls became more positive from Time 1 to 2 for participants in the confrontation-only condition. Further, these differences were maintained over time, as there was no significant change for either condition from Time 2 to 3.

Because I found few effects of elaborating (vs. not elaborating) on confrontation messages, I extended Hypothesis 2 to examine whether individual differences in general tendencies to elaborate or effortfully process messages (i.e., need for cognition) decreased prejudice and discrimination. However, need for cognition did not predict donation at Time 2 or budget discrimination at Time 3 and did not interact with condition. I also examined whether need for cognition influenced change in attitudes and feelings toward women from Time 1 to 2. Although need for cognition did not influence change across time points for benevolent sexism, hostile sexism, feelings toward women, and feelings toward party girls, need for cognition did influence change in feelings toward feminists from Time 2 to 3. Specifically, participants with higher need for cognition showed a greater increase in attitudes toward feminists from Time 2 to 3. Because feminists may be perceived as more likely to confront sexism, this finding suggests that a tendency toward elaboration may make feelings toward confronters more positive in the long-term.

Overall, there was little support for the hypothesis that elaborating (vs. not elaborating) on confrontation messages enhances confrontation's effect. Manipulation check items indicated that participants in the confrontation+elaboration condition perceived paying more attention to sexist jokes and were more certain of their attitudes toward them than participants in the confrontation-only condition. This finding suggests that elaborating on confrontation messages was achieved in this study; however, other comparisons between the confrontation conditions indicated that elaborating on confrontation messages is not the primary mechanism through which confrontation has its prejudice-reducing effect.

Hypothesis 3: Length of Effects Over Time

I hypothesized that attitude change would last longer for participants in the confrontation+elaboration condition compared to the confrontation-only condition. Although I had planned to examine how long these the effects lasted over time as a function of the number of days between Time 2 and 3, initial unconditional models indicated that real time did not moderate participants' change over time. Instead, I used a more parsimonious treatment of time (i.e., as occasion of measurement 1, 2, and 3) that did not lose information compared to a less parsimonious treatment of time (i.e., real time in days between Time 2 and 3). This finding suggests that the effects of confrontation (vs. no confrontation) and/or elaboration (vs. no elaboration) of confrontation messages may not decay significantly, or at least not on a daily basis. In terms of Hypothesis 3, there is no support because the amount of time that passed between Time 2 and 3 did not influence attitudes or feelings. Thus, the effects of confrontation (vs. no confrontation) and elaboration (vs. no elaboration) of confrontation messages found in the present study

are maintained at post-test regardless of the amount of time that has passed, at least for the range of time (i.e., 1-14 days) examined in this study.

Overall, confrontation (vs. no confrontation) reduced prejudice and discrimination; however, elaborating on confrontation messages did not further reduce prejudice and discrimination compared to confrontation alone. Thus, there was evidence to support Hypothesis 1 but little evidence to support for Hypothesis 2. Because the number of days between Time 2 and 3 did not moderate the change from Time 2 to 3, there was no evidence to support Hypothesis 3 that elaborating on confrontation messages lasts longer than confrontation without elaboration. Although persuasion theories may provide a framework to examine the various outcomes of confronting prejudice, the present study indicates that confronting prejudice may in some ways differ from other attempts at persuasion that are influenced by elaboration. This study indicates that simply breaking the perceived consensus that exists after sexist jokes reduces prejudice and discrimination, regardless of elaboration of confrontation messages.

Findings Beyond Hypotheses: Affect and Confronter Evaluation

Outside of the specific hypotheses, this study replicates and extends some previous research on confronting prejudice. For example, past research had shown that perpetrators experience negative affect after being confronted (vs. not confronted; Czopp et al., 2006). The present study indicates that observers of confrontation who elaborate on confrontation messages experience more discomfort and negative affect than observers of a confrontation who do not engage in elaboration. According to the Self-Regulation of Prejudice Model (Monteith & Mark, 2005), discomfort and/or negative affect may lead to the development of cues for self-regulation, which prevents future prejudice. Thus, the

present study extends previous research on the affective outcomes of confrontation by showing that observers who elaborate (vs. do not elaborate) on confrontation also experience discomfort and negative affect, which may help these observers avoid future prejudice.

This study also examined the evaluations of confronters. Previous research indicated that confronters are sometimes evaluated negatively (e.g., Dodd et al, 2001; Kaiser & Miller, 2001). However, other research indicated that men do not more negatively evaluate a confronter following confrontations about sexism or sexual harassment (vs. no confrontation control; Mallet & Wagner, 2011; Saunders & Senn, 2009). In the present study, there were no differences between men's evaluations of a woman who either confronted sexism or negatively evaluated the jokes without mention of sexism. This finding supports previous research indicating that confronters of sexism may not be evaluated negatively by confrontation recipients (e.g., Mallet & Wagner, 2011; Saunders & Senn, 2009). However, women more positively evaluated a woman who confronted prejudice (vs. negatively evaluated the jokes). Thus, the present study replicates research on men's evaluations of confronters of sexism but also extends this research by indicating that women positively evaluate confronters of sexism. Therefore, targets of prejudice may evaluate confronters more positively. As such, the present research adds to a growing literature indicating that confronting prejudice may not always be costly to confronters (see also Gervais & Hillard, 2011).

Limitations

As in any study, there are limitations that may affect the internal and external validity of the results. The manipulation of elaboration in this study is new; recent

research on prejudice reduction has manipulated elaboration differently. For example, Husnu and Crips (2010) asked participants to imagine that they engaged in conversation with an outgroup member and learned something interesting about him/her. Elaboration in this work was manipulated by asking participants in the high elaboration condition to imagine when and where intergroup contact might occur, whereas participants in the low elaboration condition did not receive this additional instruction. This manipulation of elaboration aimed to increase the vividness of the imagined contact, which was expected to make the scenario more accessible in memory. After the imagined contact, participants reported how willing they were to engage in future intergroup contact. The results of this work indicate that participants who had more (vs. less) detailed imagined contact have stronger intentions to engage in intergroup interaction.

In the present study, participants were similarly asked to imagine observing an exchange of sexist jokes between co-workers. Participants in the confrontation+elaboration condition then wrote an essay describing the reasons that sexist jokes might be considered offensive. Behavioral intentions were also measured in the present study; however, neither elaborating (vs. not elaborating) on confrontation messages nor confrontation itself (vs. control) influenced intentions regarding the use or confronting of sexism. On the other hand, it is also possible that elaboration did not affect intentions in the present study because participants were not asked to imagine using sexist language or confronting sexist language, which would have been more parallel to Husnu and Crisp (2010).

Second, the effects of elaborating (vs. not elaborating) on confrontation messages were limited. It is possible that the essay manipulation was simply ineffective; it is thus

far unclear whether participants' essays reflect elaboration of confrontation messages. However, manipulation checks indicated that participants in the confrontation+elaboration condition reported thinking more about the jokes than did participants in the confrontation-only and control conditions. Participants in the confrontation+elaboration condition also felt more confident about their opinions of the jokes, which is consistent with HSM's sufficiency threshold (Eagly & Chaiken, 1993). These participants had the ability (and to varying degrees, motivation) to conduct high effort processing until they had a high level of confidence in their attitude.

An additional issue related to internal validity of the experiment concerns the repeated measures design. Participants completed highly similar measures at three time points; seeing similar measures at Time 3 may have cued participants to the real purpose of the supposedly unrelated study. However, behavioral measures (i.e., the budget cut task) were completed prior to the measures included at all time points, and confrontation (vs. no confrontation) influenced behavioral measures as well as longitudinal measures completed across time points.

Other potential limitations concern the generalization of the present findings across situations and people. In the present study, sexist jokes were confronted because previous research indicated that jokes, but not statements, serve as a releaser for prejudice (Ford et al., 2008). Specifically, hostile sexist men exposed to a sexist joke—but not a sexist statement—donated less to a women's organization and cut more of the budget for a women's organization. The goal of the present study was to examine whether confrontation with or without elaboration might overcome the effect of sexist jokes in the short- and long-term. The control condition in the present study was parallel to Ford et

al.'s sexist joke condition, but I found that confrontation buffered the negative effects of sexist humor. Specifically, participants in the confrontation+elaboration condition donated more to a women's organization than did participants in the control condition. Further, participants in both confrontation conditions cut less from a women's organization 1-14 days later. While sexist jokes but not statements served as a releaser for prejudice (Ford et al.), it remains unclear whether overtly prejudiced statements or behaviors (vs. jokes) cause later discrimination that confronting prejudice might buffer.

It is also not clear whether the effect of confrontation would apply to other types of prejudice. According to past research, confronting racism is more effective than confronting sexism (Czopp & Montieth, 2003). Because the present study found that confronting (vs. not confronting) sexism reduced discrimination and increased positive feelings toward women, it is likely that confronting racism similarly reduces discrimination and increases feelings toward ethnic minorities in the long-term. In addition, confronting casual anti-gay statements (e.g., "That's so gay") has been shown to reduce prejudice (Hillard & Ryan, 2011). As such, I expect the short- and long-term effects of confronting (vs. not confronting) sexism found in the present study to generalize to other -isms; again, however, the present study does not address this question.

The present study also examined the effect of imagined rather than real confrontation. That is, participants in this study imagined witnessing a confrontation rather than actually witnessing an *in vivo* confrontation. The extent to which the results of these types of lab manipulations generalize to real-world behavior is not clear; certainly, there are reasons to be concerned. Consider, for example, that people overestimate how

emotionally disturbed they would be by prejudiced behavior (Kawakami et al., 2009) as well as their likelihood to confront prejudiced behavior (Swim & Hyers, 1999) in imagined versus *in vivo* scenarios. Such findings suggest that actually experiencing an event has different effects than imagining an event. However, an imagined event's influence could be underestimated in comparison to actual experience. As such, overhearing a confrontation *in vivo* could reduce prejudice more than imagining this scenario. For example, demand characteristics and social desirability may be less influential when the confrontation is real rather than imagined. One then might argue that the present study presents a stricter test of my hypotheses. That is, if imagining observing a confrontation (vs. no confrontation) has reduces prejudice and discrimination, as the present indicates, I would expect that actually witnessing that confrontation would reduce prejudice and discrimination to a greater degree.

Note also that in the present study, participants observed confrontation; they were not perpetrators of sexism who were confronted. However, I expect the effect of *being* the recipient of the confrontation as a perpetrator would be greater than *observing* a confrontation of a perpetrator. Again, the present study presents a stricter test of my hypotheses. If observing a confrontation (vs. no confrontation) reduces prejudice and discrimination, as found in this study, I would expect that being personally confronted would have a greater effect on prejudice and discrimination. On the other hand, personally being confronted may cause stronger emotional reaction (e.g., Czopp et al., 2006), including more negative emotion and increased defensiveness, which may undermine prejudice reduction. However, the present study does not address this question.

Finally, it remains unclear whether the present findings generalize to other populations. Participants in the present study were a convenience sample of college students enrolled in psychology courses, which is typically and problematically the case in most social psychological research (e.g., Smart, 1966). College students are relatively homogenous on two particularly influential variables—age and educational level. Thus, for example, it is possible that the pattern of results would differ for older participants. College students' attitudes may be more malleable than older populations (Sears, 1986). Adolescents and young adults are likely to be developing their identities, which may cause their attitudes and beliefs to be less defined than are attitudes among an older population. Adolescents and young adults may also be more susceptible to social influence and peer pressure, which is a particular problem for the present research. On the other hand, college students often have more liberal attitudes on social issues (Nagourney & Thee, 2007), including more egalitarian attitudes about gender (e.g., Beere, King, Beere, & King, 1984), than the general public. As such, a college sample may present a stricter test of hypotheses related to prejudice. The present study's results may be conservative estimates of actual prejudice reduction following confrontation, because a college sample has lower prejudice than the general population prior to any confrontation.

Implications

Nevertheless, the present study has both theoretical and practical implications. This study examined whether elaborating (vs. not elaborating) on confrontation messages increases confrontation's effect. I found little consistent evidence that elaborating on confrontation decreased prejudice and discrimination relative to confrontation alone. As

discussed above, it is possible that a different manipulation of elaboration may be more effective, but it does not appear that elaborating on the message is necessary for confrontation to influence feelings and behaviors toward social groups.

The present study addresses an alternate explanation or confound of all prior research on the outcomes of confronting prejudice—demand characteristics. Experimental demand effects occur when participants are cued to what response is expected or “demanded” of them in the experiment. The cues that participants receive and/or the interpretations that participants make are most problematic when those cues or interpretations are parallel to the true purpose of the study. For example, participants who are aware of the true purpose of the study may be more likely to respond in a way that is consistent expectations. Across studies of confronting, the finding that participants’ attitudes appear more positive in experimental conditions than in control conditions might lead one to conclude that demand characteristics were largely responsible for the pattern of effects obtained. That is, the confrontation condition is more transparent than the control condition in terms of the participants being aware of the purpose of study, which may cause participants to conform to the study’s purpose.

However, the present research indicates that the effect of confrontation (vs. no confrontation) lasted outside of the laboratory on a behavioral measure of discrimination that occurred 1-14 days later. There are several ways in which this finding challenges experimental demand interpretations of the effects of confrontation (vs. no confrontation). First, the long-term measure of discrimination occurred ostensibly in the context of a different study. In fact, steps were taken to maximize the degree to which the studies appeared unconnected. Time 2 occurred in the laboratory and in exchange for

partial course credit, whereas Time 3 occurred online and in exchange for cash payment. In addition, different experimenter names were provided on the consent forms, which also had different explanations for the purpose of the study (i.e., Time 2, perceptions of interactions; Time 3, distribution of budget cuts on campus). Differentiating the two studies minimizes demand explanations because any cues or interpretations the participants received and made at Time 2 should not carry through to the “unrelated” Time 3. Second, behavioral intentions are more influenced by experimental demand than actual behavior. In the present study, discrimination was measured through behavior rather than intentions. Third, Time 3 occurred 1-14 days later, which likely further differentiated the phases of the study but also decreased the likelihood that participants would remember and apply and cues or interpretations they made at Time 2 to Time 3. In addition, they may have completed other studies during the same time frame to further dilute any cues or interpretations. Thus, the reduced discrimination as a result of confrontation (vs. no confrontation) does not appear to be limited to the immediate context of the laboratory or due purely to demand characteristics.

Practically, this study suggests how people might approach confronting prejudice in their everyday lives. According to most outcomes examined in this study, one needs to simply break the perceived consensus that occurs after a sexist joke in order to buffer prejudice and discrimination that might otherwise be the result of the joke. Thus, the present study as well as others (e.g., Czopp et al., 2006) indicates that confrontation reduces prejudice. Although certain messages may be more or less effective at accomplishing this goal, my review of the literature indicates that confrontation message hostility generally affects confrontation recipients’ emotional reaction and evaluations of

confronters but not attitude or behavior change. Thus, it is possible that any confrontation may reduce prejudice and discrimination regardless of its message. Instead, message variables such as hostility may influence more peripheral outcomes of confrontation (e.g., affect and evaluation of confronters) instead of the main goal of confronting prejudice (i.e., attitude/behavior change; Hyers, 2007).

Future Research

The data collected in the present study can be examined to consider additional questions related to the effect of confrontation on prejudice and discrimination across time. For example, I included measures of feelings toward men and can thus examine how attitudes toward men might vary over time and condition. Also, I measured individual differences in internal and external motivation to respond without sexism, gender activism, and need for affect. It will be interesting to examine whether the effect of confrontation is larger in the short-term for those with high external motivation to respond without sexism but later disappears when the social pressure is no longer being applied. I would expect that people with internal motivation to respond without sexism and/or people who have greater gender activism to have less prejudice and to show less discriminatory behavior, which may mean that confrontation (vs. no confrontation) does not have a dramatic effect on these groups. I also will be able to examine whether tendencies to approach or avoid emotion relates to outcomes of confrontation, which related to the theoretical explanation of a barrier to confronting (Aboud & Joong, 2007).

I also plan to further examine how self-regulation may play a role in the outcomes of confronting prejudice. According to the Self-Regulation of Prejudice Model (Monteith & Mark, 2005), negative affect and guilt cause people to develop cues for control of

prejudice. In the present study, I did not find that confrontation (vs. no confrontation) or elaboration (vs. no elaboration) of confrontation messages influenced participants' guilt, but I did find that participants in the confrontation+elaboration condition experienced more negative affect than participants in the confrontation-only condition. Further, at Time 3, men perceived less local agreement with their cuts to a women's organization in both confrontation conditions relative to the control. Men's shift in perceived local norms at Time 3 as an effect of confrontation (vs. no confrontation) may be related to their emotional experiences at Time 2. Thus, I plan to examine how Time 2 post-confrontation affect might relate to reductions in prejudice and discrimination as well as perceptions of social norms at Time 3.

I also plan to use mixed method analyses (i.e., quantitative + qualitative) to examine the effect of elaborating (vs. not elaborating) on confrontation messages on later attitudes and behavior. I plan to code the essays participants provided to gain an understanding of why participants may or may not have found sexist jokes offensive. Qualitative themes emerging from this analysis may warrant further examination of the quantitative outcomes of the study based on those themes. Specific themes within participants' essays may be associated with study outcomes (e.g., Hillard, Ryan, & Gervais, 2011). For example, perhaps some participants developed arguments about why sexist jokes were offensive that later were related to reduced prejudice compared to other arguments. Alternatively, the absolute number of arguments that participants generated rather than kind of arguments might influence reductions in prejudice and discrimination. Finding, for example, that more arguments for why sexist jokes are offensive caused greater attitude change would provide some support for Hypothesis 2 (i.e., that

elaborating on confrontation messages causes less prejudice and discrimination than confrontation alone).

Finally, because I found little change over time and no effects of confrontation (vs. no confrontation) or elaboration (vs. no elaboration) of confrontation messages for benevolent and hostile sexism, I can examine underlying assumptions of ambivalent sexism. First, I can control for condition and examine this data for test-retest reliability of a frequently used measure, the Ambivalent Sexism Inventory. Second, I can re-examine whether benevolent and hostile sexists have polarized feelings toward subtypes of women (i.e., a replication of Glick, Diebold, Bailey-Werner, & Zhu, 1997). For example, benevolent sexists have positive attitudes toward women – especially traditional women (e.g., homemakers). On the other hand, hostile sexists have negative attitudes toward women – especially non-traditional women (e.g., career women and feminists). Because I included feeling thermometers for women in general as well as specific subtypes of women, I can use this data to examine whether these classic findings of ambivalent sexism still hold nearly 15 years after the original work.

Future research outside of the data collected as part of the present study may provide additional support of the effects of confrontation. In the introduction, I argued that some contradictory findings regarding the source of the confrontation might be explained by dual process theories of persuasion. This argument was the basis for the present study, which examined whether elaborating (vs. not elaborating) on confrontation messages increases the effects of confrontation in the short- and/or long-term. However, a different study could be designed to directly examine the effects of confronting prejudice using a persuasion paradigm. Consistent with the general approach to research on the

dual processes of persuasion, this study would manipulate argument quality and either ability or motivation to process the confrontation message. For example, participants might be exposed to strong or weak confrontation message under conditions of high or low cognitive load, which would limit participants' ability to process the message. If confrontation is similar to any other attempt at persuasion, participants under high cognitive load will be equally persuaded by a strong or weak confrontation message; however, participants under low cognitive load will be more persuaded by a strong than weak confrontation message. On the other hand, if confrontation has its effect on attitudes and behavior by simply breaking the perceived consensus or increasing perceptions of egalitarian social norms, weak confrontation messages may still be persuasive for participants under low cognitive load.

There are two reasons why confrontation might prove to differ from other persuasive attempts through future research. First, the present study did not show consistent enhancement of confrontation through elaboration on confrontation messages. Second, similar effects of confrontation have been shown for fairly weak confrontation arguments (e.g., Hillard & Ryan, 2011). Thus, although persuasion theories provide a framework to examine the effects of confronting, additional research is necessary to examine whether confronting prejudice can be seen like any other persuasive attempt.

Future research also should continue to examine prejudice reduction strategies through ecologically valid means. There is a clear need for experimental field research to understand the best approaches for prejudice reduction (Paluck & Green, 2009). As mentioned in the introduction, there are a variety of approaches to prejudice reduction that have not been widely examined by social scientists (e.g., diversity education; Paluck,

2006). These approaches provide the opportunity to examine the effects of everyday interventions to reduce prejudice, and various means of reducing prejudice—including confronting prejudice—might be effectively examined in this context.

Conclusion

This study replicates and extends previous research indicating that confronting prejudice effectively reduces prejudice and discrimination in observers compared to not confronting prejudice. Further, these effects were shown over three time points and multiple measures of feelings and behavior toward women. The present study shows that the effect of confrontation (vs. no confrontation) is not limited to lab manipulations and does not have limited duration. As such, people can have a lasting impact on others' prejudice and discrimination through the simple act of speaking up about prejudice.

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Footnotes

¹ There were three scenarios in Czopp & Monteith (2003) in which one argument was made per confrontation. In scenario 1, the confrontation was: “I think that’s racially (gender) biased, maybe we could have a Black (female) surgeon.” In scenario 2, the confrontation was: “Do you know that you just called Dr. Osgood ‘Mr. (Mrs.)’ but you called Dr. Johnson ‘Dr.’ ... which shows some racial (gender) bias.” In scenario 3, the confrontation was: “I really don’t think people should tell or laugh at jokes that play on stereotypes.”

² The confrontation in Czopp et al.’s (2006) Study 2 was: “I thought you typed pretty fast, but I noticed that for some of the pictures of Black people you said some stereotypical things like criminal, bum, and welfare. I mean, I guess that fits, but maybe that’s being a little biased. Don’t you think we should really try to treat everyone equally?” This confrontation was moderately hostile and makes two arguments; one regarded the prejudice, and one appealed to egalitarianism. Compared to the arguments in Czopp & Monteith (2003), the egalitarian appeal seems to be a stronger argument.

³ The low hostility confrontation was: “But maybe it would be good to think about Blacks in other ways that are a little more fair? It seems that a lot of times Blacks don’t get equal treatment in our society, you know what I mean?” The high hostility confrontation was: “But you should really try to think about Blacks in other ways that are less prejudiced. It just seems that you sound like some kind of a racist to me. You know what I mean?”

⁴ Another possibility that was considered is a distraction paradigm. That is, a divided attention task was considered that would inhibit participants’ ability to process a verbal message that elaborated on the confrontation. In this type of task, participants visually monitor a string of numbers presented every one (for high distraction) or five (for low distraction) seconds while listening to the message and press a key when the target number was presented on the screen.

Unfortunately, distraction from a persuasive message is murkier than it may seem for four reasons. First, there are four levels of message involvement (i.e., pre-attention, focal attention, comprehension, and elaboration; Petty & Cacioppo, 1986), and distraction addresses message attention or comprehension rather than elaboration. Thus, a distraction manipulation follows from message-learning paradigm, and the problems inherent in that paradigm were already discussed. Dual process theories, on the other hand, focus on elaboration. Elaboration involves developing one’s own arguments, which is at a deeper level of understanding than comprehension.

Second, research shows that there must be a large decline in comprehension to decrease attitude change (Buller & Hall, 1998). Thus, the outcome of a distraction manipulation would be that one group pays attention to an additional message while the distracted group does not. The manipulation then actually compares the number of arguments. My research already shows that there is no difference in participants’ ($N = 145$) attitude change between a 30-second public service announcement (PSA) showing a confrontation of “that’s so gay” and a five-minute speech featuring the PSA (Hillard & Ryan, 2011).

Third, there are conflicting findings regarding distraction’s effect on persuasion. In some studies, distraction decreases persuasion, which may be caused by decreased

message comprehension. However, in other studies, distraction *increases* persuasion, which may be caused by decreased counter arguing (Perloff, 2003). The distraction hypothesis indicates that distraction increases persuasion by blocking cognitive responses to the message. In this case, people are distracted not from the message but from counter-arguing the message. A meta-analysis suggests that whether the distraction is communication-relevant or communication-irrelevant is a feature of distraction that influences the outcome (Buller & Hall, 1998). The previously mentioned distraction task would be classified as a communication-irrelevant distraction. However:

“communication-irrelevant distractions merely divert the receiver’s attention away from the persuasive appeal with *unpredictable* results. [Emphasis in the original.] They can produce a sustained disruption and lower comprehension, reducing persuasion or represent a temporary, albeit annoying, diversion with little effect on persuasion.” (Buller & Hall, pg. 162)

In addition, three studies that reported decreased message comprehension also found *increased* attitude change. Thus, even if one is able to address comprehension rather than counter-arguing, the effect of distraction may have the opposite of the desired effect.

Fourth, there is one more way in which distraction manipulations are unstable. The distraction hypothesis posits that distraction decreases counter-argument; however, other research suggests that it blocks the dominant cognitive response to the message, whether that be counter-arguing or arguments in support of the message. For example, egalitarian people should generate supportive thoughts regarding the confrontation message, whereas less egalitarian people should counter-argue. If the dominant cognitive response is dampened by distraction, egalitarian people should be less persuaded by a confrontation message, whereas less egalitarian people should be more persuaded by a confrontation message. Therefore, the effect of the distraction task would depend on individual differences in prejudice.

In short, there are four problems with a distraction task: (1) distraction may interfere with comprehension rather than elaboration; (2) distraction would compare the number of arguments; (3) message-irrelevant distraction can have unpredictable results; and (4) distraction may have conflicting results based on individual differences. To avoid these problems, I selected an essay manipulation. An essay manipulation required one group to *elaborate* – using their own arguments—on why the sexist jokes may be offensive to someone. In order to increase the strength of the manipulation, the control essay described a mundane task in detail unrelated to the confrontation. This control essay prevented any further elaboration of the confrontation message, whereas the essay relevant to the message required participants to elaborate on issues related to the confrontation.

Table 1

Participants' guilt as a function of confronter's target status and argument strength

	Weak argument	Strong argument
Target confronter	Less guilt	More guilt
Non-target confronter	More guilt	Less guilt

Table 2

Measures Participants Completed by Time

Measure	Time 1	Time 2: pre-manipulation	Time 2: post-manipulation	Time 3
Motivation to control sexism	✓			
Gender activism	✓		✓	
Need for cognition		✓		
Need for affect		✓		
Social desirability		✓		
Feeling thermometers	✓		✓	✓
Ambivalent sexism	✓		✓	✓
Affect			✓	
Ratings of scenario, jokes, and speakers			✓	
Behavioral intentions			✓	
Perceived processing and attitude certainty			✓	
Donation			✓	
Budget cut discrimination				✓
Budget cut norms				✓
Attitudes toward organizations				✓

Table 3

Time 2 Dependent Variables by Condition, M(SD)

	Control	Confront- only	Confront+ elaboration
Positive affect	3.78 (0.96)	3.52 (1.15)	3.75 (1.15)
Negative affect ⁺	2.02 ^{ab} (1.05)	1.80 ^a (0.81)	2.03 ^b (0.84)
Surprise*	2.00 ^a (1.10)	1.71 ^b (0.92)	2.14 ^a (1.19)
Guilt	1.73 (1.10)	1.56 (0.84)	1.79 (0.92)
Negative self-directed affect ⁺	1.85 (1.17)	1.60 (0.85)	1.83 (0.96)
Negative other- directed affect ⁺	1.99 ^{ab} (1.24)	1.71 ^a (1.13)	2.06 ^b (1.14)
Discomfort*	2.12 ^a (1.05)	1.81 ^b (0.78)	2.07 ^a (0.82)

Note. Means with different superscripts within rows significantly differ, $p < .05$.

* $p < .05$, ⁺ $p < .10$.

Table 4

Liking of Jokes by Joke Type and Participant Gender, M(SD)

	Neutral jokes	Sexist jokes
Women ($n = 212$)	3.98 (1.58)	3.08 ^a (1.43)
Men ($n = 144$)	4.53 (1.33)	4.61 ^b (1.47)
Total ($N = 356$)	4.20 (1.36)	3.70 (1.63)

Note. Means with different superscripts within columns significantly differ, $p < .05$.

Table 5

Evaluation of the Confronter by Condition and Participant Gender, M(SD)

	Control	Confrontation- only	Confrontation+ elaboration
Women ($n = 212$)	4.64 ^a (1.40)	5.29 ^b (1.46)	4.84 ^{ab} (1.32)
Men ($n = 145$)	4.39 (1.32)	4.28 (1.48)	4.47 (1.45)

Note. Means with different superscripts within rows significantly differ, $p < .01$.

Table 6

Perceived Norms for Participants' Budget Cuts by Condition and Gender, M(SD)

	Control	Confront- only	Confront+ elaboration
Local Norms*			
Women	4.03 (1.58)	4.00 (1.50)	4.56 (1.22)
Men	4.50 (1.79)	3.42 (1.31)	3.67 (1.46)
General Norms			
Women	4.39 (1.33)	4.10 (1.26)	4.21 (1.40)
Men	3.90 (1.17)	3.68 (1.29)	3.81 (1.22)

Note. Means with different superscripts within rows significantly differ, $p < .05$.

* $p < .05$

Table 7

Intercepts and Slopes for Feelings Toward Subtypes of Women

	Women	Career women	Feminists	Homemakers	Party girls
Intercept	$M = 83.30^a$ $SE = 1.00$ $p < .001$	$M = 81.52^b$ $SE = 0.99$ $p < .001$	$M = 48.75^c$ $SE = 1.36$ $p < .001$	$M = 76.63^d$ $SE = 1.09$ $p < .001$	$M = 47.00^c$ $SE = 1.49$ $p < .001$
Slope12	$b = 3.59^a$ $SE = 1.17$ $p < .01$	$b = 3.06^a$ $SE = 1.18$ $p < .01$	$b = -2.27^b$ $SE = 1.33$ $p = .09$	$b = 4.47^a$ $SE = 1.18$ $p < .001$	$b = -2.07^b$ $SE = 1.43$ $p = .15$
Slope23	$b = -2.54^a$ $SE = 1.48$ $p = .09$	$b = -1.19^a$ $SE = 1.48$ $p > .42$	$b = 5.39^b$ $SE = 1.70$ $p < .01$	$b = -1.26^a$ $SE = 1.50$ $p > .40$	$b = -0.80^a$ $SE = 1.82$ $p > .66$

Note. Bolded values are significant, and marginal p -values are bolded. Values with different superscripts within rows significantly differ, $p < .05$.

Table 8

Simple Slopes for Longitudinal Dependent Variables by Condition

	Benevolent sexism	Hostile sexism	FT- Women	FT- Feminists	FT- Party girls
Slope 12					
Control	$b = -0.04$ $SE = 0.07$ $p > .56$	$b = -0.05$ $SE = 0.08$ $p > .51$	$b = -0.18$ $SE = 1.77$ $p > .91$	$b = -4.05$ $SE = 2.40$ $p = .09$	$b = -6.05$ $SE = 2.67$ $p < .05$
Confront-only	$b = 0.04$ $SE = 0.07$ $p > .64$	$b = -0.10$ $SE = 0.07$ $p > .15$	$b = 7.18$ $SE = 1.67$ $p < .0001$	$b = -2.03$ $SE = 2.26$ $p > .36$	$b = 4.23$ $SE = 2.52$ $p = .09$
Confront+elaboration	$b = -0.10$ $SE = 0.07$ $p > .14$	$b = -0.13$ $SE = 0.07$ $p = .07$	$b = 4.04$ $SE = 1.70$ $p < .05$	$b = -0.78$ $SE = 2.31$ $p > .73$	$b = -5.86$ $SE = 2.57$ $p < .05$
Slope 23					
Control	$b = 0.01$ $SE = 0.07$ $p > .91$	$b = -0.06$ $SE = 0.07$ $p > .40$	$b = 0.55$ $SE = 1.76$ $p > .75$	$b = 3.80$ $SE = 2.90$ $p > .19$	$b = -2.13$ $SE = 2.76$ $p > .44$
Confront-only	$b = -0.05$ $SE = 0.07$ $p > .48$	$b = -0.09$ $SE = 0.07$ $p > .17$	$b = -3.51$ $SE = 1.63$ $p < .05$	$b = 8.22$ $SE = 2.71$ $p < .01$	$b = 0.25$ $SE = 2.55$ $p > .92$
Confront+elaboration	$b = 0.07$ $SE = 0.07$ $p > .34$	$b = -0.003$ $SE = 0.07$ $p > .96$	$b = -2.31$ $SE = 1.81$ $p > .20$	$b = 4.43$ $SE = 2.96$ $p > .13$	$b = 1.95$ $SE = 2.82$ $p > .49$

Note: Models for FT women and party girls include the effect of whether participants completed Time 3, based on the results of the missingness analyses.

Table 9

Final Model Estimates Including Gender by Dependent Variables (b)

	Benevolent sexism	Hostile sexism	FT- Women	FT- Feminists	FT- Party girls
Intercept	4.27	4.12	79.41	41.15	47.91
Completion of Time 3	--	--	0	--	0
Slope12	-0.03	0.08	-1.08*	-6.42+	-13.79*
Slope23	0.004	-0.13+	0.85+	3.92*	-1.18
Condition: control	0	0	0	0	0
Condition: confront-only	0.11	0.02	4.37	1.55	3.41
Condition: confront+elaboration	-0.04	0.07	3.71	-4.27	1.36
Participant gender	-0.42*	-0.46*	--	13.91*	-9.47*
Slope12*Participant gender	--	--	--	--	8.18*
Slope12*control	0	0	0	0	0
Slope12*confront-only	-0.001	-0.16	9.13*	2.07	12.30*
Slope12*confront+elaboration	-0.08	-0.19	3.95	6.55	3.18
Slope23*control	0	0	0	0	0
Slope23*confront-only	-0.03	0.03	-4.64+	4.05	1.68
Slope23*confront+elaboration	0.07	0.12	-3.00	1.08	3.05

Note. Completion of Time 3 was included as a predictor for feelings toward women and party girls, as indicated by missingness analyses, but was not a predictor in these models.

* $p < .05$, + $p \leq .10$

Table 10

Final Model Estimates Including Need for Cognition by Dependent Variables (b)

	Benevolent sexism	Hostile sexism	FT- Women	FT- Feminists	FT- Party girls
Intercept	4.27	4.21	76.92	39.76	60.96
Completion of Time 3	--	--	3.25*	--	0
Slope12	-0.03	0.08	-0.18	-6.09	-21.98*
Slope23	0.004	-0.13	0.55	1.50*	-1.18
Condition: control	0	0	0	0	0
Condition: confront-only	0.11	-0.03	3.63+	2.20	1.85
Condition: confront+elaboration	-0.04	0.05	2.62	-3.96	0.62
Need for cognition	--	-0.18*	--	2.56	-6.17*
Participant gender	-0.42*	-0.47*	--	14.03*	-9.71
Slope12*Participant gender	--	--	--	--	8.18
Slope12*control	0	0	0	0	0
Slope12*confront-only	-0.001	-0.16	7.36*	1.88	12.36*
Slope12*confront+elaboration	-0.08	-0.20	4.22+	6.48	3.16
Slope23*control	0	0	0	0	0
Slope23*confront-only	-0.03	0.03	-4.06+	5.34	1.68
Slope23*confront+elaboration	0.07	0.12	-2.86	1.64	3.05
Need for cognition*Slope12	--	--	--	-0.64	--
Need for cognition*Slope23	--	--	--	4.68*	--

Note: * $p < .05$, + $p < .10$

Figure 1. Model of the antecedents and consequences of confronting prejudice.

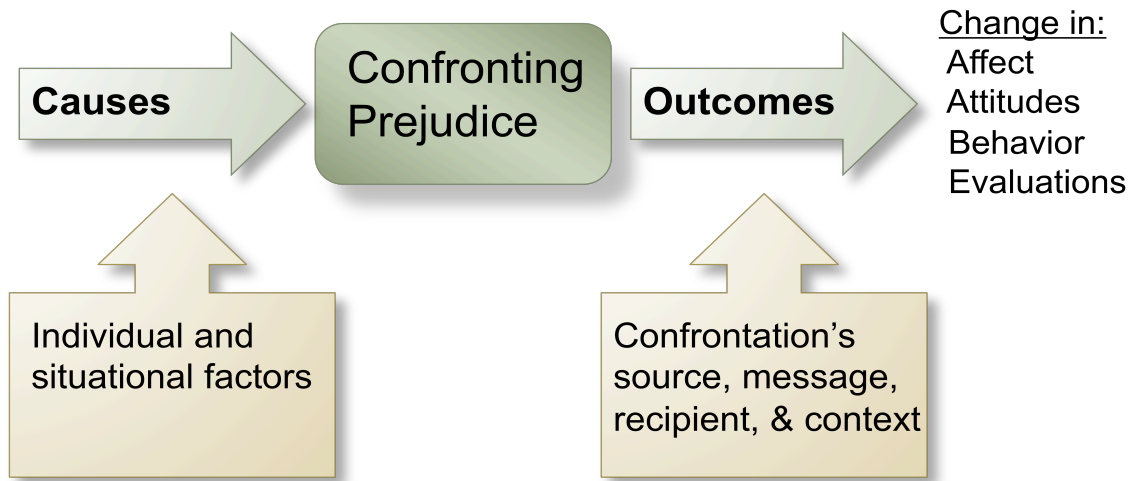


Figure 2. A map of the literature examining when confronting occurs.

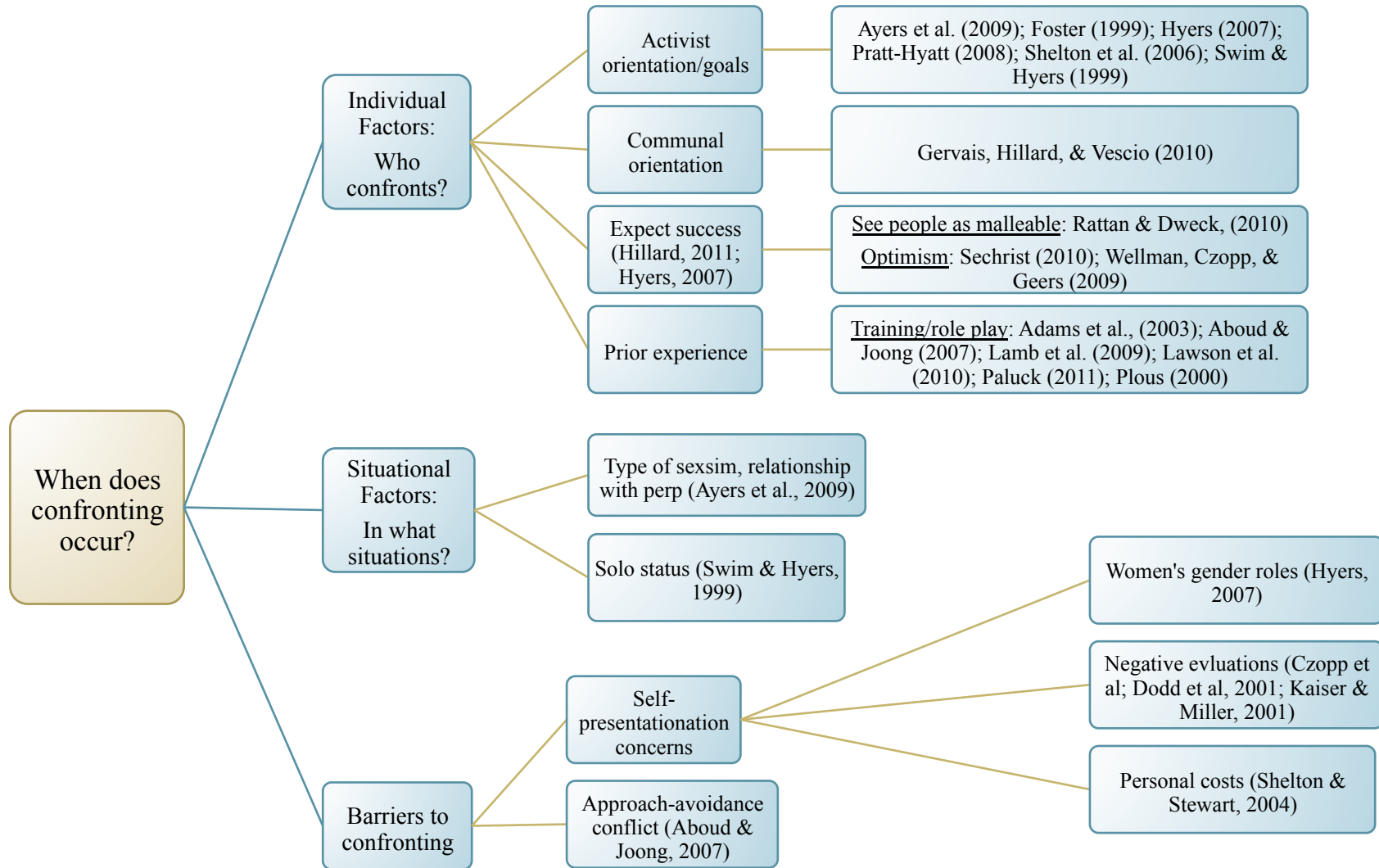


Figure 3. A map of the literature examining outcomes of confronting for perpetrators and observers.

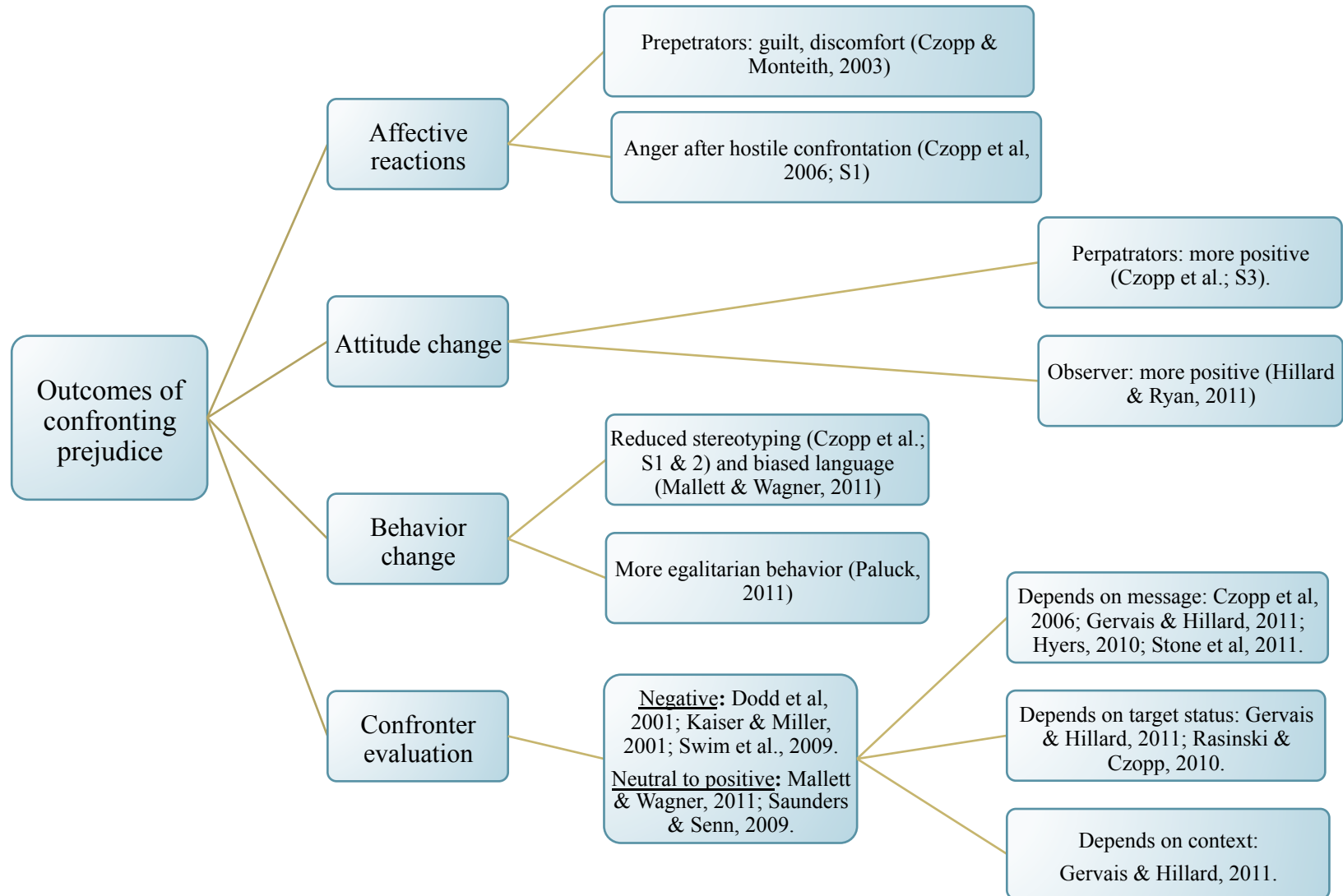


Figure 4. A map of the literature on confronting prejudice using the message-learning approach.

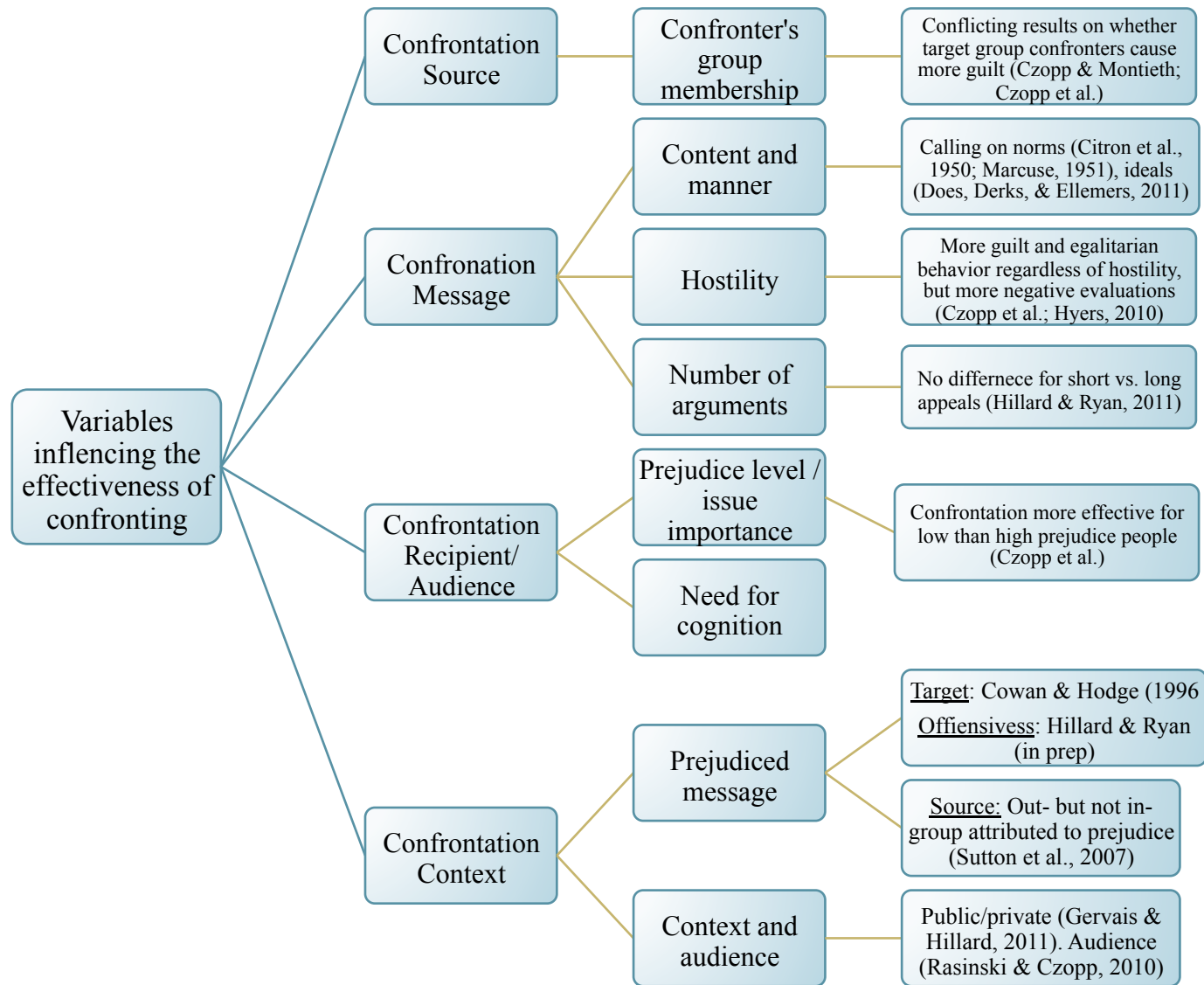
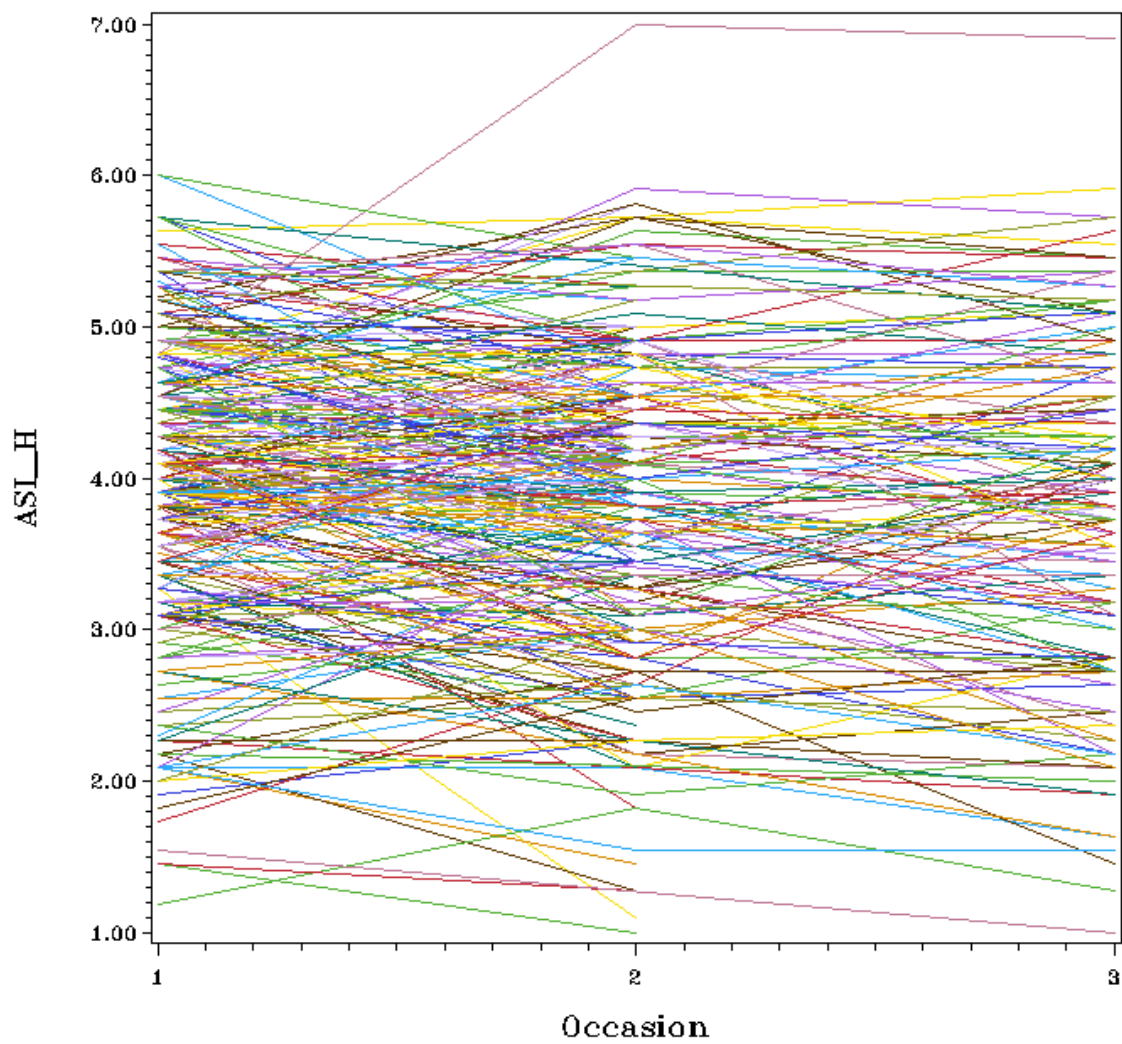


Figure 5. Individual trajectories across time for hostile sexism.



Appendix A

I. The Context

The following interactions occurred among a group of staff members in the distribution department for the local newspaper. At lunch time the staff members typically get together to socialize while they eat their lunches. The four passages describe interactions that occurred during lunch one day.

Imagine that you are a member of this newspaper group and a part of each of these interactions.

II. The Interactions

1. Cindy describes a humorous event from her “wild” weekend. “I was on my way to pick up my boyfriend from the bus station on Friday night in a car I had borrowed from one of my friends. I did not notice that my friend and three others were following me in another car. When my boyfriend and I returned to where I had parked, the car was missing. We both panicked and rushed to the bar across the street to call my friend. There the four were sitting, grinning at the prank they had executed, and at the anxiety they had aroused.”

2. After Cindy's story, the group discussion gave way to a giddy exchange of the staff members' favorite jokes. Here are a few of those jokes.

- David:** ... I have a joke for you.
Q: What did Jeffrey Dahmer say to Lorena Bobbit?
A: "Are you going to eat that?"
- Paula:** ...laughter... That's disgusting! Okay, I got one.
Q: What did the right breast say to the left breast?
A: If we get any lower, people are gonna think we're nuts!
- Michael:** ... laughter... Okay, have you heard this one?
Q: How can you tell if a blonde's been using the computer?
A: There's white-out on the screen!
- Donna:** ...laughter... all right, here's another one.
Q: Why did the woman cross the road?
A: Who cares? What the hell is she doing out of the kitchen?
- Cindy:** ... laughter... Okay, here's one.
A man and a woman were stranded in an elevator and they knew they were gonna die. So, the woman turns to the man and says, "Make me feel like a woman before I die." So he takes off his clothes and says, "Fold these!"

3. Martin describes a time when he and Larry (two intermediate skiers) went skiing. “We take the thunder chair to the top of Vail Peak. There we find this run called "Devil's Revenge" (marked as Expert) and another called "WimpOut" marked as a beginner slope. I take a long look down Devil's Revenge. "That sucker looks straight down. Let's blow this one off" I said. Larry, as you might imagine, disagrees, "I'm tired of beginner slopes.” He says. “I'm going to go for it." I tried to convince him to go down the beginner slope. I tell him that its late in the day, we're both tired. This is the time of day that people make mistakes and get hurt and this run is a killer. Look 20 yards to the right by that broken ski. What do you see?" Larry pulls down his goggles and says, "It is just some red snow, dude. Let's live dangerously." I told him to “go ahead and I'll meet you at the bottom."

4. The National Council of Women is an organization committed to serving and promoting the political and social advancement of women and women's issues. It has just released "The ABCs of Women's Issues." The Council is soliciting donations from you and your coworkers in the distribution department.

Again, imagining yourself in the context of this newspaper group, how much of your own money would you be willing to donate to the National Council of Women? Please confine your donations to an amount between \$0.00 and \$20.00.

Amount you are willing to donate:

Appendix B

Next year's funding for RSOs (registered student organizations) at UNL have to be cut by 20% (\$30,000) from the 2009-10 budget of \$150,000. The RSOs that will be affected by the budget cut are listed on the following page. You will be provided with a description of each of those RSOs.

The Association of Students of the University of Nebraska (ASUN), the student governing body, is investigating how the student body believes these funding cuts should be allocated among those organizations. ASUN has commissioned researchers on campus to aid them in determining how the student population wishes the university to allocate the funding cuts. ASUN has given us the form on the next page to be completed by participants in our studies.

Each organization has reported that the 2009-10 budgets were sufficient in funding their needs. However, each has expressed serious concerns that a 20% decrease will severely curtail their programs and possibly threaten their ability to continue operations.

Your task is to allocate budget cuts so that across the seven organizations, the overall RSO budget is reduced by 20% (\$30,000). Allocate the budget cuts to the organizations you see fit. We understand that your budget cuts may not add up to exactly \$30,000. However, please try to match an overall budget cut of \$30,000 as closely as you can. After you complete your budget cut allocations, you will be asked to give your perceptions of how other students might respond.

Keep in mind that your opinions are important. ASUN will use student allocations to make recommendations to the Student Senate, who will represent the student body in the final allocation decisions.

ASUN Student Government
 (Association of Students of the University of Nebraska)

Student Organization	2009-10 Budget	Your Proposed Funding Cut	Remaining Budget
Agricultural Association	\$15,500	_____	_____
Jewish Cultural Collective	\$23,500	_____	_____
Safe Arrival for Everyone (SAFE)	\$22,200	_____	_____
National Student Council of Women	\$24,050	_____	_____
Study Abroad Learning Program	\$26,200	_____	_____
Nebraska Black Student Union (NBSU)	\$24,050	_____	_____
Cinema Club	\$14,500	_____	_____

Do you belong to any of these student organizations?

YES NO

If yes, which ones do you belong to?