Volume 51 / Number 1 / 2020

Social Psychology

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The latest knowledge on how to tackle the complexities of hoarding disorder

"If you wish to help those who suffer with the debilitating problem of hoarding, get this book and learn from these experienced scientist-practitioners."

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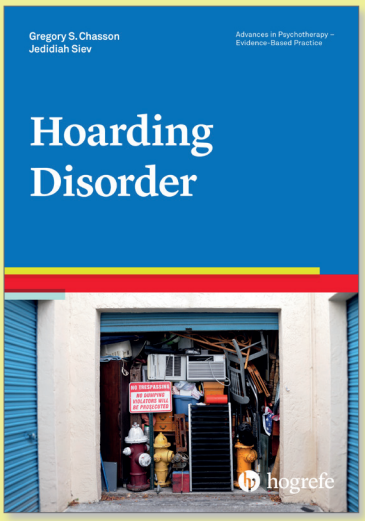

Gregory S. Chasson / Jedidiah Siev

Hoarding Disorder

(Series: Advances in Psychotherapy – Evidence-Based Practice – Volume 40)
2019, viii + 76 pp.
US $29.80 / € 24.95
ISBN 978-0-88937-407-2
Also available as eBook

Hoarding disorder, classified as one of the obsessive-compulsive and related disorders in the DSM-5, presents particular challenges in therapeutic work, including treatment ambivalence and lack of insight of those affected. This evidence-based guide written by leading experts presents the latest knowledge on assessment and treatment of hoarding disorder. The reader gains a thorough grounding in the treatment of choice for hoarding – a specific form of CBT interweaved with psychoeducational, motivational, and harm-reduction approaches to enhance treatment outcome. Rich anecdotes and clinical pearls illuminate the science, and the book also includes information for special client groups, such as older individuals and those who hoard animals. Printable handouts help busy practitioners. This book is essential reading for clinical psychologists, psychiatrists, psychotherapists, and practitioners who work with older populations, as well as students.

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Editor-in-Chief

Kai Epstude, University of Groningen, Department of Psychology, Grote Kruisstraat 2/1, 9712 TS Groningen,The Netherlands, Tel. +31 50 363-7632, Fax + 31 50 363-4581, E-mail k.epstude@rug.nl

Editorial Office

Wim Meerholz, University of Groningen, Department of Psychology, Grote Kruisstraat 2/1, 9712 TS Groningen,The Netherlands, Tel. +31 50 363-6393, Fax + 31 50 363-4581, E-mail SocialPsych.EditorialOffice@gmail.com

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North America: Hogrefe Publishing, 361 Newbury Street, 5th Floor, Boston, MA 02115, USA,
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Production

Antonia Laimina, Hogrefe Publishing, Merkelstr. 3, 37085 Göttingen, Germany,
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Subscriptions

Hogrefe Publishing, Herbert-Quandt-Str. 4, D-37081 Göttingen, Germany,
Tel. +49 551 99950-900, Fax +49 551 90050-998, zeitschriftenvertrieb@hogrefe.de

Advertising/Inserts

Melanie Beck, Hogrefe Publishing, Merkelstr. 3, D-37085 Göttingen, Germany,
Tel. +49 551 99950-423, Fax +49 551 99950-111, E-mail marketing@hogrefe.com

ISSN

ISSN-L 1864-9335, ISSN-Print 1864-9335, ISSN-Online 2151-2590

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Publication

Published in 6 issues per annual volume. *Social Psychology* is the continuation of *Zeitschrift für Sozialpsychologie* (ISSN 0044-3514), the last annual volume of which (Volume 38) was published in 2007.

Subscription Prices

Calendar year subscriptions only. Rates for 2020: Institutions – from US $454.00/€348.00 (print only; pricing for online access can be found in the journals catalog at hgf.io/journalscatalog); Individuals – US $223.00/€168.00 (print & online). Postage and handling – US $24.00/€18.00. Single copies – US $79.50/€62.50 + postage and handling.

Payment

Payment may be made by check, international money order, or credit card, to Hogrefe Publishing, Merkelstr. 3, D-37085 Göttingen, Germany. US and Canadian subscriptions can also be ordered from Hogrefe Publishing, 361 Newbury Street, 5th Floor, Boston, MA 02115, USA.

Electronic Full Text

The full text of *Social Psychology* is available online at http://econtent.hogrefe.com and in PsycARTICLES.

Abstracting Services

Abstracted/indexed in Current Contents/Social and Behavioral Sciences (CC/S&BS), Social Sciences Citation Index (SSCI), PsycINFO, PASCAL, PSYNDEX, ERIH, Scopus, and EM Care.
2018 Impact Factor 1.359, 5-year Impact Factor 2.339, *Journal Citation Reports* (Clarivate Analytics, 2019)

Contents

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Original Article

Prejudice and the Acceptance of Muslim Minority Practices

A Person-Centered Approach

Levi Adelman and Maykel Verkuyten

European Research Centre on Migration and Ethnic Relations (ERCOMER), Utrecht University, The Netherlands

Abstract: Growing Muslim minorities in Western societies has sparked debate about which Muslim practices should be accepted, with many people finding certain practices intolerable. Two competing perspectives on this intolerance argue that it represents either principled objections or prejudice. Using four large samples from the Netherlands, we apply latent profile analysis and find four groups of people: two groups that like and dislike Muslims and their practices respectively, but also two groups who are intolerant of some or most Muslim practices without necessarily displaying prejudice. A person-centered analysis of key demographic and psychological variables suggests that the two intolerant groups differ with one group's intolerance motivated more by anti-Muslim feelings, while the second group's intolerance is motivated more by principled objections.

Keywords: principled objection, toleration, prejudice, latent profile analysis, person-centered

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Questions of the accommodation of specific Muslim practices[1] and rights (e.g., headscarf, Mosques, Islamic education) within the limits of liberal societies are at the center of the polarized debate in Western Europe and other Western societies (e.g., Carol & Koopmans, 2013). Research has focused on understanding attitudes toward Muslim practices and rights in terms of group-based prejudice and Islamophobia (e.g., Kalkan, Layman, & Uslaner, 2009; Raiya, Pargament, Mahoney, & Trevino, 2008; Savelkoul, Scheepers, van der Veld, & Hagendoorn, 2012). Support for banning the headscarf, for example, reflects anti-Muslim feelings (Helbling, 2014; Saroglou, Lamkaddem, Van Pachterbeke, & Buxant, 2009). However, some studies suggest that the relationship between prejudice toward Muslims and acceptance of their religious practices is not straightforward (e.g., Sniderman & Hagendoorn, 2007; Van der Noll, 2014; Van der Noll, Poppe, & Verkuyten, 2010). The level of acceptance or rejection of Muslim practices may depend on the nature of the specific practice in question, and individuals can be intolerant because of more principled objections to that particular practice rather than due to prejudicial feelings.

Considering general feelings toward Muslims as well as the acceptance of a range of Muslim practices allows us to examine the possibility that people reject particular Muslim practices and the related rights (e.g., Islamic primary education) while thinking well of Muslims as a group, and vice versa. Further, if we were to investigate only one specific practice we risk missing the attitudes of those who would like to ban that practice but accept other Muslim practices, or conversely those who might accept the practice but want to forbid all else. Therefore, and in going beyond previous research on anti-Muslim attitudes, our primary goal is to jointly examine majority members' general feelings toward Muslims and their acceptance of a range of controversial but legal Muslim practices. Using four large datasets from the Netherlands and a person-centered approach we examined whether there are distinct groups of individuals within the majority population with different combinations of feelings and acceptance, and with different demographic and social psychological characteristics.

Anti-Muslim Reactions

Research in Europe (e.g., Spruyt & Elchardus, 2012; Strabac & Listhaug, 2008) and in the United States (e.g., Kalkan et al., 2009) indicates that anti-Muslim feelings are more wide-spread than negative feelings toward other immigrant

[1] When we refer to "Muslim practices" here, we are referring to practices linked to Muslims in Western Europe and frequently debated in broader society. Thus, this is not to say that the practices are essential or defining Muslim practices, as many of them are widely debated within the Muslim community.

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groups. Kalkan and colleagues (2009) found that an empirical distinction can be made between people's attitudes toward categories that are defined by racial, ethnic, and religious background and their attitudes toward cultural groups that are defined by dissenting practices and behaviors. Anti-Muslim feelings were found to be connected to both attitudes, and most strongly to the latter ones.

Using survey data, several studies try to examine the extent to which these anti-Muslim feelings reflect group-based prejudice and the extent to which these reflect specific forms of (religious) critique (Breton & Eady, 2015; Hagendoorn & Poppe, 2012; Imhoff & Recker, 2012; Kalkan et al., 2009; Sniderman & Hagendoorn, 2007). For instance, research has demonstrated that Enlightenment values (e.g., Gustavsson, Van der Noll, & Sundberg, 2016; Imhoff & Recker, 2012), secularism (Van Bohemen, Kemmers, & De Koster, 2011), and universalistic notions (Elchardus & Spruyt, 2014; Saroglou et al., 2009) predict anti-Muslim feelings over and above the statistical effect of generalized prejudice. These findings indicate that criticism of Islam cannot be reduced to anti-Muslim feelings.

Another strategy that can be used to understand anti-Muslim feelings is to distinguish between group-based prejudice and (in)tolerance of specific practices. Research in Western Europe has argued that people with more prejudicial attitudes toward Muslim minorities also more strongly oppose, for example, the building of Mosques, the wearing of headscarves, and Islamic schools. Thus, opposition to and rejection of these specific practices would reflect a general dislike of Muslims. In support of this perspective, several studies have found that higher prejudice is indeed associated with higher intolerance of Muslim practices and that the rejection of dissenting practices is used to justify anti-Muslim feelings (e.g., Helbling, 2014; Saroglou et al., 2009; Van der Noll, 2014; Van der Noll & Saroglou, 2015).

However, there is also research that supports an alternative perspective, indicating that (political) intolerance and prejudicial attitudes are distinct phenomena (Crawford & Pilanski, 2014; Hagendoorn & Poppe, 2012; Klein & Zick, 2013; Van der Noll, Poppe, & Verkuyten, 2010; Wirtz, van der Pligt, & Doosje, 2016). For example, in the context of Quebec, Canada, while those who hold prejudicial views supported a ban on religious symbols, a majority of the people supporting the ban did so out of principled secularism rather than prejudice (Breton & Eady, 2015). In another study in Quebec it was found that feelings of cultural threat and generalized prejudice predicted support for banning *minority* religious symbols whereas holding liberal values predicted support to ban *all* religious symbols (Bilodeau, Turgeon, White, & Henderson, 2018). Similarly, analyzing data from six European countries, Helbling (2014) found that Europeans with secular liberal values felt positively toward Muslims as a group, but felt torn regarding the legislation of religious practices such as the wearing of the headscarf. In addition, among national samples in the UK, France, Germany, and the Netherlands, a substantial portion of people with a positive attitude toward Muslims supported a ban on headscarves (Van der Noll, 2010; see also Saroglou et al., 2009) and also rejected Islamic education and the building of Mosques (Van der Noll, 2014).

Drawing on these two competing perspectives, an objection to a specific practice may represent (1) an expression of one's negative attitude toward Muslims as a group, (2) an expression of disapproval of that particular practice, or (3) a combination of these two. For example, majority members can reject the founding of Islamic schools because they dislike Muslims, or because they believe that religion in general has no place in education, or a combination of the two. In other words, the distinction between group-based attitudes toward Muslim minorities and practice-specific disapproval is important and requires greater attention (Bilodeau et al., 2018; Dangubic, Verkuyten, & Stark, 2019; Hurwitz & Mondak, 2002). One way to address this is by considering a range of Muslim practices in combination with general feelings toward Muslims. This allows us to develop a more detailed understanding of particular combinations of group-based feelings of Muslim minorities and the acceptance of specific Muslim practices.

Intolerance and Multiple Practices

The combination of negativity toward a minority group with nevertheless accepting this minority group's civil rights is central in the literature on political tolerance (Gibson, 2006; Sullivan, Piereson, & Marcus, 1982). However, negative out-group feelings are not a precondition for (political) *in*tolerance because one can reject a specific practice (e.g., ritual slaughter of animals) of people or groups (Jews, Muslims) to whom one has neutral or even positive feelings (Hurwitz & Mondak, 2002; Sniderman, Tetlock, Glaser, Green, & Hout, 1989).

Considering various dissenting practices simultaneously makes it possible to take into account the distinction between majority members who consistently accept or rather reject various Muslim practices, and people who do not consistently accept or reject the different practices. Cross-practice consistency can indicate a general like or dislike of Muslim minorities and inconsistencies can indicate principle considerations or rather social normative concerns about specific practices (Dangubic et al., 2019; Sniderman et al., 1989). Specifically, some people may display positive feelings toward Muslims and are consistent in accepting the various practices ("liking"), while others

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have negative feelings and consistently reject all practices ("disliking"). The existence of these two groups is in agreement with the literature that links support for Muslim minority practices with group-based feelings. Additionally, however, there is the possibility that these positive or negative feelings go together with the acceptance of some Muslim practices but not of other practices. This practice-based inconsistency indicates that people do not only consider information about whom they are asked to tolerate but also on the nature of the dissenting practices (e.g., Gibson & Gouws, 2003; Petersen, Slothuus, Stubager, & Togeby, 2011). The consideration of multiple practices might demonstrate that people have objections to a particular practice (e.g., wearing of a headscarf) but not toward another practice (e.g., Islamic education). A particular practice might raise specific moral concerns, such as the wearing of a headscarf which might evoke the issue of gender equality and religious education in public schools that can evoke concerns about the secular nature of the state (Moss, Blodorn, Van Camp, & O'Brien, 2017; Sarrasin, 2016). However, the rejection of some Muslim practices might also result from social normative standards that make it socially acceptable to express one's prejudiced feelings by rejecting these practices and not others (Crandall & Eshleman, 2003). Thus, some individuals may be practicing a form of principled intolerance, where their disapproval emerges from specific views held about specific practices, while other individuals may be practicing a prejudiced intolerance, where the disapproval of specific practices emerges from a prejudice toward the group that they may be unwilling to express. Using data from four representative samples of the Dutch majority and a person-centered approach, we will examine whether these four groups of individuals do indeed exist and how many people can be described as practicing, for example, a principled or prejudiced intolerance. Furthermore, as a matter of construct validity we consider whether some key demographic and social psychological constructs characterize the different groups.

Person-Centered Approach

In general, social psychological research typically investigates associations using a variable-centered approach in which the evaluation of an out-group and its practices is considered to have a common underlying dimension that ranges individuals from low to high prejudice (see Meeusen, Meuleman, Abts, & Bergh, 2018). This approach ignores the possibility that individuals are not equally negative toward distinct sets of practices and therefore that a single score does not accurately reflect the stances that majority members take. Further, it is possible that people combine their feelings and objections in different ways leading to groups of individuals with distinct constellations of ratings. In other words, a variable-centered approach ignores the complex constellation of characteristics that make up individuals and precludes the possibility that there are profiles of people which would provide a nuanced and more detailed understanding of how majority members perceive and evaluate Muslims as a group and the various practices they are engaged in.

Taking a person-centered approach makes it possible to consider these combinations of out-group feelings and the evaluation of different out-group practices simultaneously (e.g., Bergman & Magnusson, 1997; Meeusen et al., 2018; Muthén & Muthén, 2000). This type of analysis seeks to identify unobserved groups, or categories, of individuals who differ in the particular ways in which they combine their out-group feelings and objections to a range of out-group practices. For example, in a research on political tolerance, the best model for the data required four categories of individuals rather than a continuum of tolerance (McCutcheon, 1985). In addition to a group of individuals who were consistently positive toward various minority groups and three different practices and a group of people who were consistently negative, two other categories of people were found (see also Herson & Hofstetter, 1975; McIntosh, Mac Iver, Abele, & Nolle, 1995; Merton, 1976; Sniderman et al., 1989). Individuals in these two categories accepted some groups and some practices but rejected others. These four categories could not be readily placed on a unidimensional positive-negative continuum because there was no monotonic change across the four groups of individuals. Rather, they formed four latent classes of political tolerance.

Thus, a person-centered approach allows us to investigate whether feelings toward Muslims as a group of people and objections to various Muslim practices are combined in different ways by different groups of individuals. This makes it possible to examine whether, and how many, majority members have, for example, a liking-based, a disliking-based, or a principled or prejudiced intolerant profile concerning their attitudes toward Muslims and Muslim practices. One group of individuals might reject almost all Muslim practices while another group of individuals might reject the building of Mosques but accept the establishment of Islamic schools. Thus, rather than a unidimensional continuum, by considering different dissenting practices simultaneously it is possible to assess potentially different patterns among different groups of individuals that may speak to the underlying attitudes that affect which practices they accept. While it might be reasonable for someone to have a fair objection to a single specific practice, it seems unlikely that they would have such objections to all out-group practices. Rather someone who is generally prejudiced toward Muslims as a group will be less likely to differentiate based on the specifics of individual practices

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but will tend to be broadly intolerant of a range of Muslim practices.

Profiles and Their Correlates

Beyond identifying groups of individuals based on their acceptance of outgroup practices and general out-group feelings, it is important to examine whether the different groups of individuals differ for some key characteristics that are typically considered in research on minority group prejudice. This is important because both principled and prejudiced intolerance involve practice-inconsistent rejection and therefore are largely indistinguishable in terms of observed responses. However, several factors should strengthen or weaken a tendency to principled tolerance and these can be examined as a matter of construct validity. We will look into the role of educational background and political orientation as two main demographic predictors of prejudicial attitudes and of important social psychological constructs.

Research on the effect of education on prejudicial attitudes suggests a complex relationship. The ideological refinement perspective (Jackman & Muha, 1984) views education as endowing majority members with more advanced cognitive skills and ideological commitments to support abstract ideas of equality and justice, while simultaneously using their cognitive skills to protect the status quo by rejecting social policies designed to overcome group-based inequalities. Research also suggests that the association between higher education and a more positive attitude toward minority groups is not explained by a greater tendency of the higher educated to respond in a socially desirable way (Heerwig & McCabe, 2009; Ostapczuk, Musch, & Moshagen, 2009; Wagner & Zick, 1995). This does not mean that the higher educated have less spontaneous negative reactions toward ethnic minorities. Research on aversive racism (Dovidio & Gaertner, 2004) has shown that aversive racists show relatively strong prejudice on implicit but not on explicit measures (Son Hing, Chung-Yan, Hamilton, & Zanna, 2008), and the higher educated have been found to have lower explicit but not implicit prejudices (Kuppens & Spears, 2014). However, the association between higher education and a more positive self-reported attitude toward ethnic and cultural minority groups is one of the most replicated findings in the social sciences (Jenssen & Engesbak, 1994) and has been predominantly explained in terms of cognitive development and the learning of liberal values. Education is associated with cognitive ability and flexibility (Bobo & Licari, 1989; Ohlander, Batalova, & Treas, 2005) and is a strong correlate of political sophistication (Bennett, 1996; Delli Carpini & Keeter, 1996; Highton, 2009; Hillygus, 2005). When the educational level in the population increases, the ideological sophistication also increases (Tedin, 1987). Education implies political socialization which involves a better understanding of the values and beliefs that underlie political-ideological differences (Osborne & Sibley, 2012; Stenner, 2005) and making the higher educated better able to understand the importance of basic norms and values of equality and tolerance underlying the democratic culture (Vogt, 1997). The higher educated are not only more likely to be tolerant in general but also as a matter of principle (Sniderman et al., 1989). Thus, the literature leads us to expect that the "general liking" and "principled intolerant" groups are more highly educated than the "prejudiced intolerant" and "generally disliking" groups. In addition to education, we also considered a direct measure of cognitive sophistication as the tendency to consider how issues that one feels strong about can have multiple perspectives. Based on our reasoning for the role of education we can expect that the "general liking" and "principled intolerant" will display higher cognitive sophistication than the prejudiced intolerant and the disliking groups.

Social psychological research on the social cognition model has argued and demonstrated that two core aspects capture the most important differences between the political right and the left (Jost, Glaser, Kruglanski, & Sulloway, 2003). Political orientation is manifested in a specific ideological configuration in which respect for tradition and acceptance of inequality are central (Jost, 2006, 2017). The first dimension concerns attitudes toward cultural tradition and social deviance, while the second relates to (in) equality and egalitarianism. Individuals on the political right tend to prefer traditions and social conformity, and to accept inequalities. In contrast, those on the left more strongly embrace socio-cultural change and equality. Extensive empirical research in political psychology and in different Western countries (Piurko, Schwartz, & Davidov, 2011) confirms that these two core dimensions capture the most important ideological differences between right-wing and left-wing political orientations (Jost 2006, 2017; Schwartz, Caprara, & Vecchione, 2010). This means that we expected that the Muslim liking and principled intolerant groups have a stronger left-wing political orientation compared to the Muslim disliking and prejudiced intolerant groups.

We further investigated Social Dominance Orientation (SDO), Right Wing Authoritarianism (RWA), and perceived out-group threat as three key social psychological constructs that have been extensively linked to tolerance as well as prejudicial attitudes toward minority groups. Examining these constructs allows us to assess whether the expected groups of individuals do not only differ in their attitude toward Muslims and Muslim practices but also in these important correlates. Specifically, the "generally like" group and the "principled intolerance" group should have

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lower SDO, lower RWA, and lower perceived threat compared to the "prejudiced intolerance" and "generally dislike" groups. Furthermore, we also sought to determine whether the groups of individuals differ in their internal motivation to control and not express prejudices. This measure can give a further understanding of the underlying difference between principled and prejudiced intolerant individuals. Specifically, we can expect that the "general liking" and "principled intolerant" will display a stronger internal motivation to control prejudice than the prejudiced intolerant and the disliking groups.

Finally, we tried to extend beyond attitudes and ideologies to behavioral intentions. Specifically, we considered the willingness to engage in anti-discrimination activities. We selected this variable because someone who is intolerant of specific practices for more principled reasons should not accept discriminatory treatment of minority members or members of other minority groups. Therefore, support for anti-discrimination activities is expected to serve as a useful identifier for making a distinction between the principled and prejudiced intolerant groups of individuals.

Summing Up

In this article, we drew on data from four large representative datasets of majority Dutch participants collected in the Netherlands between 2014 and 2018 which allows us to investigate how well the data aligns over time and across sampling error. We examine feeling thermometer ratings toward the two most prominent and typical groups of Muslims in the Netherlands (of Turkish and Moroccan background), together with the acceptance of five different Muslim practices that are strongly debated in society (public expression of Muslim religion, wearing of the headscarf, celebration of Islamic holidays, building of Mosques, founding of Islamic schools). Our first aim is to investigate whether there are groups of individuals who are characterized by particular combinations of general feelings and acceptance of specific practices. More specifically, we expect to identify the four types of dislike-based (prejudiced feelings and rejection of all practices), like-based (non-prejudiced feelings and acceptance of all practices), principled intolerant (non-prejudiced feelings and differential rejection of practices), and prejudiced intolerant (non-prejudiced feelings and general rejection of practices).

Second, we expect that the groups of individuals differ in terms of several well-known correlates of prejudice toward minority groups. Based on existing variable-centered research we expect that the groups differ in level of education, political orientation, RWA, SDO, perceived out-group threat, the internal motivation to control prejudice, cognitive sophistication, and the tendency to be involved in anti-discrimination actions. Specifically, we expected that the liking group and the principled intolerance group differ on these correlates from the disliking group and the prejudiced intolerance group.

Method

Participants

For this paper, we analyzed data of four large representative datasets that contain various measures and that have been used for other purposes in previous research (e.g., Mepham & Verkuyten, 2017; Verkuyten, Martinovic, Smeeks, & Kros, 2016). In all four studies the data were collected online among probability samples drawn from nationally representative pools of the majority Dutch population. The response rate of the different studies was around 55% which is similar to other research in the Netherlands (Stoop, 2005). The samples covered various segments of the Dutch public in terms of age, gender, education, household size, and the region of residence. The samples were selected by research consultancy companies which maintain a database of majority Dutch people who regularly participate in surveys in return for remuneration.

In Study 1, 469 majority Dutch participants in the 2014 dataset (Study 1) completed all seven key measures and were retained for analysis. Participants were identified as majority Dutch based on self-identification and if both of their parents were born in the Netherlands. Participants who identified as Muslim were excluded in the analysis (two participants each in Studies 2 and 3). Similarly, 800 participants in the 2015 dataset (Study 2), 590 in the 2017 dataset (Study 3), and 563 participants in the 2018 dataset (Study 4) completed all key measures and were retained for analysis. The participants were split relatively evenly along gender lines (2014: 52.2% male; 2015: 50.0% male; 2017: 54.56% male: 2018: 50.3% female), and from a wide range of ages (2014: M = 50.26, SD = 16.98, range = 18–88; 2015: M = 50.65, SD = 17.16, range = 18–87; 2017: M = 55.46, SD = 14.60, range = 18–87;2018: M = 51.08, SD = 17.52, range = 18–91), education levels and political orientation (descriptive statistics reported below).

Materials

The materials used are discussed below and the English (translated) versions of all items can be found in the Electronic Supplementary Material (ESM 1). Complete data and analytic scripts can be found on the Open Science Framework at https://osf.io/7j2zm/.

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Out-Group Feelings

Respondents were presented with feeling thermometers to indicate their coldness or warmth to members of the two main Muslim minority groups in the Netherlands: Turks and Moroccans. Both groups are over 95% Muslim, and are widely recognized as the two prototypical Muslim groups in the Netherlands. Using feeling thermometers with wider ranges of responses than Likert-type scales generates a more reliable measure (Alwin, 1997), and these explicit measures tend to correlate with subtler methods of assessing prejudice (Dovidio, Kawakami, & Beach, 2001). Overall and reflecting the ethnic hierarchy in the Netherlands (Schalk-Soekar, van de Vijver, & Hoogsteder, 2004), feelings toward Turks hovered around the midpoint of the 1–11 scale, neither warm nor cold (M_{Study1} = 6.05, SD_{Study1} = 2.29; M_{Study2} = 6.20, SD_{Study2} = 2.20; M_{Study3} = 6.45, SD_{Study3} = 2.27; M_{Study4} = 5.13, SD_{Study4} = 2.21), while feelings toward Moroccans were consistently lower (M_{Study1} = 4.36, SD_{Study1} = 2.23; M_{Study2} = 4.28, SD_{Study2} = 2.12; M_{Study3} = 4.66, SD_{Study3} = 2.47; M_{Study4} = 4.54, SD_{Study4} = 2.24). More negative attitudes toward Turks in 2018 (Study 4) may be due to a 2017 political and diplomatic crisis between Turkey and the Netherlands. Across all three studies, the two measures were positively correlated (r ranging from .49 to .61) and in a recent study among a Dutch representative sample, the feelings toward Turks and Moroccans correlate strongly with the feeling toward Muslims as a category (.73 and .77, respectively).

Acceptance of Muslim Practices

The specific practices presented to the respondents were partially adapted from previous research (e.g., Gieling, Thijs, & Verkuyten, 2010; Smeekes, Verkuyten, & Poppe, 2011). These relate to different types of civil liberties that, however, are subject to much debate in Dutch society.[2] Using 7-point scales, one item asked participants' agreement that Muslims can express their faith in public ("Muslims in the Netherlands must be able to show and experience their own faith in public life"; M_{Study1} = 4.31, SD_{Study1} = 1.60; M_{Study2} = 4.44, SD_{Study2} = 1.53; M_{Study3} = 4.16, SD_{Study3} = 1.77; M_{Study4} = 4.72, SD_{Study4} = 1.54), the second referred to Muslim women's ability to wear the headscarf ("Muslim women should have the opportunity to wear a headscarf anywhere in the Netherlands"; M_{Study1} = 3.34, SD_{Study1} = 1.77; M_{Study2} = 3.78, SD_{Study2} = 1.80; M_{Study3} = 3.63, SD_{Study3} = 1.92; M_{Study4} = 3.87, SD_{Study4} = 1.77), the third asked about Muslim's rights to celebrate their festivals in public ("Muslims in the Netherlands should not only be able to celebrate their Islamic holidays at home, but also in public"; M_{Study1} = 4.04, SD_{Study1} = 1.64; M_{Study2} = 4.13, SD_{Study2} = 1.60; M_{Study3} = 3.99, SD_{Study3} = 1.82; M_{Study4} = 4.46, SD_{Study4} = 1.62), the fourth about the right to build mosques ("Muslims must have the right to build mosques in the Netherlands"; M_{Study1} = 3.72, SD_{Study1} = 1.76; M_{Study2} = 3.94, SD_{Study2} = 1.71; M_{Study3} = 3.91, SD_{Study3} = 1.87; M_{Study4} = 4.29, SD_{Study4} = 1.72), and the fifth about the right to establish Islamic schools ("Muslims must have the right to establish Islamic schools"; M_{Study1} = 3.09, SD_{Study1} = 1.65; M_{Study2} = 3.08, SD_{Study2} = 1.65; M_{Study3} = 2.84SD_{Study3} = 1.71; M_{Study4} = 3.35, SD_{Study4} = 1.75). Thus, these items range from the less objectionable (showing and experiencing faith in public) to the more objectionable (establishing Islamic schools that may perceived to prevent social and cultural integration).

Predictor Variables

Educational background was measured using a single-item in which participants indicated their highest educational achievement on a scale ranging from 1 (= *no higher education*) to 8 (= *doctorate or advanced masters*) in Studies 1 and 3, and 1 (= *no higher education*) to 7 (= *doctorate or advanced masters*)in Study 2 and 4 (Study 1: 23.4% low, 48.9% middle, 27.7% high, M_{study1} = 5.14, SD_{study1} = 1.68; Study 2: 25.4% low, 49.7% middle, 25.0% high, M_{study2} = 5.03, SD_{study2} = 1.68; Study 3: 23.5% low, 45.1% middle, 31.4% high,M_{study3} = 4.24, SD_{study3} = 1.75; Study 4: 17.1% low, 47.4% middle, 35.5% high, M_{study4} = 4.45, SD_{study4} = 1.72). The distinction between these levels of achieved education is comparable to the International Standard Classification of Education (ISCED)-measure that is used, for example, in the European Social Survey. Similar to other research in the Netherlands (e.g., De Graaf, De Graaf, & Kraaykamp, 2000; Van de Werfhorst & Van Tubergen, 2007), education was treated in the analysis as a continuous variable which allows us to investigate the difference between lower and higher educated participants.

Political orientation was measured with the well-known self-placement question (Jost, 2006). A 5-point scale was used ranging from politically left, center-left, center, center-right to right. In all four samples, the overall mean for political ideology fell in the center (M_{study1} = 3.00, SD_{study1} = 1.09; M_{study2} = 2.92, SD_{study2} = 1.05; M_{study3} = 3.02, SD_{study3} = 1.18; M_{study4} = 2.93, SD_{study4} = 1.27) with an equal distribution to the political right and to the political left.

A measure of *social dominance orientation* was available in two datasets. Eight (in Study 2; M = 3.24, SD = 0.86; α = .76), and six (in Study 3; M = 3.20, SD = 1.02; α = .72) items of a short version of SDO were used that

[2] In Studies 1–3, the five practices listed here are the only practices included in the surveys. In Study 4, one additional practice asked about the right for Muslims to create political parties. As that item measures political rather than social tolerance, and as that item differed from the other three studies, it was not included in the analyses.

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was validated and translated to Dutch by Duriez and Van Hiel (2002) and that has been used in other research (Duriez, Soenens, & Vansteenkiste, 2007; Meeus, Duriez, Vanbeselaere, Phalet, & Kuppens, 2009).

Right-wing authoritarianism (RWA) was measured using a short measure previously utilized in the Netherlands (Sniderman & Hagendoorn, 2007) and that focuses on the conformity aspect of RWA. This measure was used in Study 2 (3 items, $M = 4.61$, $SD = 1.12$, $\alpha = .70$) and Study 3 (4 items; $M = 5.09$, $SD = 0.98$; $\alpha = .70$).

Perceived out-group threat was measured in three datasets in a reliable but not identical way. In Studies 1 and 2, symbolic threat was measured using four and three items, respectively, that asked about Muslims in the Netherlands undermining the Dutch identity and way of life (2014: $M = 4.00$, $SD = 1.49$, $\alpha = .90$; 2015: $M = 3.99$, $SD = 1.64$, $\alpha = .96$). In Study 3, in light of the refugee crisis, the four threat items were expanded to refer to refugees who are from predominantly Muslim nations rather than Muslims as a group ($M = 3.97$, $SD = 1.89$, $\alpha = .97$).

Study 3 also included a measure of *internal motivation to control prejudice* (Plant & Devine, 1998), with the goal of better understanding the motivations of people who might not explicitly declare prejudice but nonetheless be unaccepting of Muslim practices. The scale was comprised of four items measured on a 1–7 scale ($M = 5.01$, $SD = 1.09$, $\alpha = .97$).

Cognitive sophistication was also measured in Study 3 using a 4-item scale ($M = 5.18$, $SD = 0.99$, $\alpha = .83$) that focused on whether and how frequently participants sought to understand alternative perspectives on issues that they felt strongly about.

Behavior intentions. Following previous research in the Netherlands (Verkuyten, 2017), in Study 3 respondents were asked how likely it is that they would engage in a set of actions in response to discrimination against immigrants in the Netherlands ($M = 2.34$, $SD = 0.95$, $\alpha = .85$).

Analyses

For the analysis we used a latent profile analysis approach (e.g., Oberski, 2016) which we conducted using R software. Latent profile analysis is analogous to latent class analysis (LCA; Muthén & Muthén, 2000) and identifies patterns across a set of continuous variables that can be identified as different profiles of individuals. We included the two feeling thermometers measuring attitudes toward Muslims (Turks and Moroccans) and the five items about Muslim practices into latent profile analysis to determine how general feelings and acceptance of specific practices combine to best identify different profiles of individuals in Dutch society. We conducted this same analysis across four separate datasets to triangulate in on a set of profiles that best represent Dutch society.

The LPA analyses performed identify the best models of profiles using different sources of empirical information about the appropriate number of profiles. First, the Bayesian information criteria (BIC) and Akaike information criteria (AIC) indicate how well a model with the selected number of profiles fits the data, with the lowest numbers indicating the best fit. Secondly, the Bootstrappped Likelihood Ratio Test (BLRT) conducts significance tests comparing the selected model with a model including one fewer profile (comparison between k and $k - 1$ profiles). Lastly, entropy scores indicate how uniquely the datapoints belong to one profile and not others. Low entropy scores indicate that datapoints (participants) could be classed into more than one profile, whereas high entropy scores indicate that participants are uniquely classed into one profile and not others. A final component of determining profiles is based on interpretability. One important nuance of interpretability is the need not to overfit the data by generating a model with profiles that represent specific variances unique to the dataset rather than generalizable profiles of participants. The suggested ways to avoid this is to ignore small classes and to inspect the profiles that emerge for interpretability. In our analysis, we used all of these approaches to arrive at the best profile fit for the data.

Results

Latent Profiles

Table 1 shows that across four independent datasets collected over a 4-year period, the combination of respondents' general feelings toward Muslims minorities and their support for specific Muslim practices reveal four groups of individuals. Investigation of the best type of model across the four datasets revealed that a model that allowed for varying means while holding variance and covariance equal best fit the data. As can be seen in Table 1, while the BIC criteria in Studies 2–4 indicate that six profiles provide a slightly better fit, in both cases the improvement over the previous iteration is relatively small, and the criterion of theoretical interpretability suggests a four profile solution across the four datasets. Specifically, when we investigated profiles generated by the six-profile solution, the resultant profiles were inconsistent across the studies, generating profiles of less than 50 people (n ranging from 31 to 45 in Studies 2 and 4), and somewhat larger, though still inconsistent profiles in Study 3 (n = 61–78). In all studies, the six-profile solution returned groups consistent with the Like, Dislike, and Prejudiced Intolerant groups (see below), and returned fractured sections of what we term the Principled

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Table 1. Model fit indices across three studies

Study #	Profile #	BIC	AIC	BLRT	Entropy
Study 1 (2014)	3	11,644.66	11,432.98	65.56, $p < .001$	0.93
	4	**11,563.52**	**11,318.633**	**130.35, $p < .001$**	**0.92**
	5	11,697.80	11,419.71	−85.07, $p = .999$	0.87
	6	11,650.53	11,339.23	NA	0.87
Study 2 (2015)	3	19,845.43	19,606.42	57.85, $p < .001$	0.88
	4	**19,689.68**	**19,413.29**	**209.23, $p < .001$**	**0.86**
	5	19,698.98	19,385.11	44.18, $p < .001$	0.84
	6	19,663.19	19,311.85	89.26, $p < .001$	0.89
Study 3 (2017)	3	14,842.74	14,619.35	142.92, $p < .001$	0.89
	4	**14,719.83**	**14,461.41**	**173.95, $p < .001$**	**0.88**
	5	14,712.99	14,419.52	57.89, $p = .001$	0.90
	6	14,712.11	14,383.61	51.91, $p = .002$	0.86
Study 4 (2018)	3	13,773.84	13,552.84	63.82, $p < .001$	0.90
	4	**13,615.86**	**13,360.20**	**208.64, $p < .001$**	**0.92**
	5	13,625.82	13,335.49	40.71, $p < .001$	0.91
	6	13,628.64	13,303.65	47.84, $p < .001$	0.90

Note. Bootstrapped Likelihood Ratio Test (BLRT) tests whether each number of profiles represents an improvement over identifying one fewer profile. BIC = Bayesian Information Criteria; AIC = Akaike Information Criteria. Bolded rows indicate the four-profile solution that was indicated across all four datasets.

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Intolerant group, usually in which some participants displayed greater or lesser concern about individual practices like wearing the headscarf or opening Islamic schools. These differences, while likely reflecting a range of concerns among that group, did not consistently return distinct profiles across the datasets, so we elected to use the more interpretable four profile solution. Thus, while the six profile solution performed better statistically, there was no consistent pattern across the six-profile solutions that improved interpretability above the four-profile model.

Figure 1 (panels A–D) presents the mean levels of Muslim group feelings and acceptance of the specific practices across the four studies for the four profiles identified. Note that for ease of interpretation these scores were subtracted from the neutral midpoints of the scales of the items used. Thus, positive scores indicate attitudes that are higher than the midpoint and negative scores indicate attitudes below the midpoint. This allows us to differentiate between relative differences in attitudes that indicate negativity as compared to neutrality.

A first group consists of individuals with relatively positive feelings toward the two Muslim minority groups combined with relatively high levels of acceptance for all the Muslim practices ("liking"; between 27.8% and 36.5% across the datasets).The second ("disliking") consists of individuals with negative feelings toward Muslim minorities combined with a tendency to reject the different Muslim practices (between 13.8% and 22.0%). In addition, the latent profile analysis indicates that almost half of the participants do not appear to be less prejudiced against Muslim minority groups than the disliking group but nonetheless are unwilling to tolerate Muslim practices. This category of individuals emerges as two profiles: one in which all or almost all Muslim practices are rejected without apparent distinctions made between them, and another in which some practices are rejected but not others. The first of these subgroups we label as "prejudiced intolerance" (between 28.5% and 34.5%). While they do not appear to have particularly negative feelings toward Muslims, displaying neutral to slightly positive attitudes toward the Turkish minority group for example, their rejection of all Muslim practices (most evident in Studies 2 and 3) without differentiating much between them suggests that this rejection may be driven by a general dislike of Muslims. In contrast, members of the second of these subgroups show substantial differences in attitudes toward distinct practices. For example, in Study 1 they are opposed toward the headscarf, and across all of the studies they are especially opposed to Islamic schools. At the same time, their support for religious freedom (i.e., building Mosques) and public expression is usually very similar to that of the "liking" group. Thus, this fourth group is distinguished both from those that are generally positive across the board and those that are neutral or negative across the board, and we label this group "principled intolerant" (between 17.6% and 29.2%). Their intolerance of some Muslim practices but not others suggests a rejection based on specific objections rather than a generalized dislike. Thus, while responses on some of the less controversial practices are broadly similar in pattern to those of other groups, it is precisely the differences in one or two

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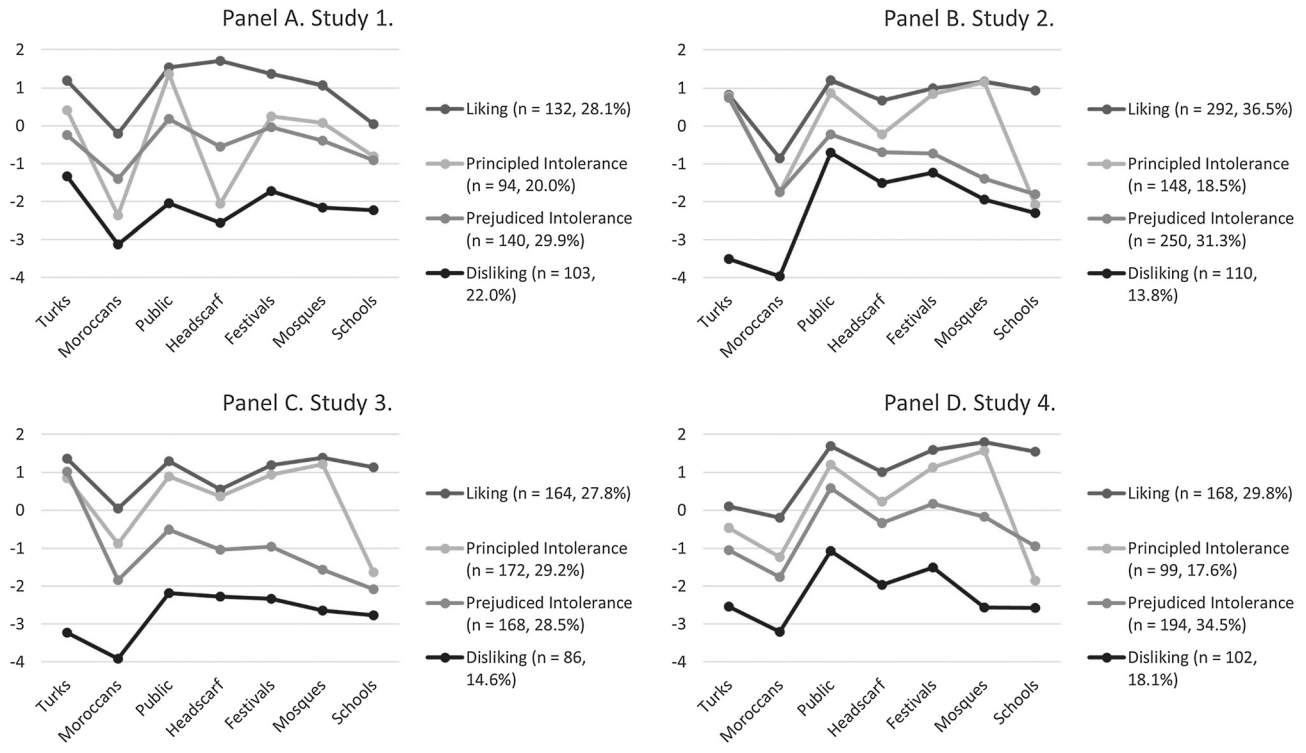

Figure 1. Panels A–D: Mean scores for the attitude and tolerance of practices variables for the four profiles in each study.

practices that differentiate the groups. Across all four studies, the group we identify as principled intolerant deviates from the pattern found among the other three groups, specifically by displaying positivity or neutrality to most practices, but a strong and consistent objection to specific other practices. Thus, these findings support our expectation of the existence of four specific profiles, with two subgroups of intolerance without prejudice emerging: one which appears to disapprove of all practices and thus seems to more closely fit the description of prejudiced intolerance, and another which is intolerant of some practices but not others and thus appears to resemble a more principled intolerance.

Predictors of the Groups of Individuals

Further evidence for the construct validity of these groups is provided by investigation of differences in important correlates. Therefore, we next looked at whether the four groups of people identified across these datasets differ on key demographic, social psychological, and behavioral variables. To do this, we generated multinomial logistic regression models using SAS software to predict membership in these categories. We created the model using three steps. In the first step, we looked at education and, the related concept of cognitive sophistication, as well as political orientation as key characteristics. In the second step, we included SDO, RWA, and internal motivation to control prejudice to see whether and how ideological world view differences predicted membership in these four profiles. Then we added in out-group threat and the behavioral measure of willingness to act regarding discrimination against immigrants in the Netherlands, to see how those added to our understanding of how the types of people who fall into these profiles differ. We used the three-step model since we expect there to be a substantial overlap between demographic and world view predictors and with threat and behavioral predictors. By conducting the analysis in three steps we are able to see the effects of demographic variables independent of the other predictors, as well as demographic and world view predictors independent of threat and behavioral predictors. In the third step, we can also see which predictors play a meaningful unique role when included with all other predictors. For the analyses and following our predictions, the principled intolerant group was set as the referent. All variables included in these analyses were rescaled from 0 to 1 which means that the log odds beta coefficients and the odds ratios indicate the relative change in likelihood of belonging to each of the groups as a function of a full scale increase in a given predictor.

Table 2 shows how, while there is variation across the four datasets, there also appears to be a distinction between unique predictors for the four profiles. Overall the principled intolerant group appears to be distinct from the prejudiced intolerant and disliking groups, and in some cases appears to be slightly more similar to the liking profile

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Table 2. Multinomial logistic regression results of a demographics-only and demographics and personality predictors models with the principled intolerant group as the referent

	Liking		Prejudiced intolerant		Disliking	
Reference = principled intolerant	Log odds β (*SE*)	Odds ratios	Log odds β (*SE*)	Odds ratios	Log odds β (*SE*)	Odds ratios
Panel A: Demographic predictors						
Education						
Study 1	−0.19 (0.50)	0.83	−0.69 (0.49)	0.50	−1.99*** (0.54)	0.14
Study 2	1.20** (0.38)	3.31	−1.26** (0.40)	0.29	−1.58*** (0.48)	0.21
Study 3	0.39 (0.38)	1.48	−1.87*** (0.42)	0.15	−1.86*** (0.52)	0.16
Study 4	0.64 (0.50)	1.89	−0.80† (0.48)	0.45	−2.16*** (0.59)	0.12
Cognitive sophistication						
Study 3	1.54† (0.83)	4.67	−2.08** (0.79)	0.13	−4.79* (0.91)	0.01
Political orientation						
Study 1	−1.57** (0.51)	0.21	−0.44 (0.50)	0.65	1.13* (0.56)	3.10
Study 2	−0.27 (0.39)	0.76	1.30** (0.41)	3.68	0.83† (0.50)	2.30
Study 3	0.44 (0.41)	1.55	2.56*** (0.46)	12.90	2.66*** (0.57)	14.36
Study 4	−0.84† (0.45)	0.43	1.09* (0.44)	2.97	2.94*** (0.57)	18.97
Panel B: Demographic and personality predictors						
Education						
Study 2	1.14** (0.39)	3.13	−0.89* (0.41)	0.41	−0.83 (0.51)	0.44
Study 3	0.23 (0.41)	1.26	−1.40** (0.45)	0.25	−1.02† (0.58)	0.36
Cognitive sophistication						
Study 3	1.21 (0.93)	3.35	−1.30 (0.93)	0.27	2.02† (1.10)	0.13
Political orientation						
Study 2	−0.22 (0.43)	0.80	0.71 (0.45)	2.04	0.14 (0.54)	0.87
Study 3	0.57 (0.43)	1.77	2.33*** (0.47)	10.30	1.81** (0.61)	6.08
SDO						
Study 2	0.92 (0.76)	2.52	2.88** (0.80)	17.78	4.61*** (1.01)	100.20
Study 3	0.82 (0.77)	2.28	1.18 (0.83)	3.25	2.96** (1.09)	19.22
RWA						
Study 2	−0.82 (0.62)	0.44	1.01 (0.67)	2.75	3.18*** (0.87)	24.07
Study 3	−1.18† (0.70)	0.31	1.98* (0.80)	7.22	3.81*** (1.05)	45.11
Internal motivation to control prejudice						
Study 3	1.21 (0.91)	3.37	−0.71 (0.88)	0.49	−3.19*** (1.06)	0.04

Notes. All variables were feature scaled (0–1) before inclusion for ease of comparison. Coefficients and odds ratios therefore represent changes due to movement from the lowest to the highest value on the scale. Coefficient betas represent log odds with standard errors in parentheses. Odds ratios represent odds of category belonging compared to the referent group. Only Studies 2 and 3 were included in the second step, as Studies 1 and 4 did not include any ideological predictors. SDO = Social Dominance Orientation; RWA = Right Wing Authoritarianism; *SE* = Standard Error. †$p < .09–.05$; *$p < .05$; **$p < .01$; ***$p < .001$.

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than the prejudiced intolerant profile. First, across the four studies and as expected, people in the principled intolerance group tended to be among the best educated: the disliking and prejudiced intolerant groups are less well educated than the principled intolerant group and the liking group. Second, consistent with the education variable, the principled intolerant group was more likely to engage in cognitively sophisticated thought than the prejudiced intolerant or disliking groups. Third, members of the principled intolerant group are politically more left-leaning than people in the prejudiced intolerance and disliking groups. Fourth, the principled intolerant group has consistently RWA and had lower SDO tendencies than the disliking and prejudiced intolerant groups, with mixed differences from the liking group. Lastly, the principled intolerant group has marginally less motivation to control prejudice than the liking group and (marginally) more than the prejudiced intolerance group and the disliking group.

Table 3 shows how the addition of threat and behavioral predictors adds to our understanding of the different groups. Individuals in the principled intolerant group tend to perceive somewhat less threat from Muslims than the prejudiced intolerant group, further differentiating them, although those in the principled intolerant group nonetheless perceived greater threat than those in the liking group. Importantly, the distinctions between the two

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Table 3. Multinomial logistic regression results of a full predictor model with principled intolerant as the referent group

	Liking		Prejudiced intolerant		Disliking	
Reference = Principled intolerant	Log odds β (*SE*)	Odds ratios	Log odds β (*SE*)	Odds ratios	Log odds β (*SE*)	Odds ratios
Education						
Study 1	−1.06† (0.55)	0.35	−0.93† (0.51)	0.40	−1.33* (0.60)	0.26
Study 2	0.97* (0.39)	2.64	−0.82* (0.41)	0.44	−0.63 (0.53)	0.53
Study 3	0.14 (0.42)	1.16	−1.08* (0.46)	0.34	−0.38 (0.63)	0.69
Study 4	0.64 (0.50)	1.89	−0.80† (0.48)	0.45	−2.16*** (0.59)	0.12
Cognitive sophistication						
Study 3	1.23 (0.95)	3.43	−1.18 (0.96)	0.31	−1.67(1.16)	0.19
Political orientation						
Study 1	−0.56 (0.58)	0.57	−0.23 (0.53)	0.80	0.16 (0.61)	1.17
Study 2	0.11 (0.45)	1.12	0.51 (0.46)	1.67	−0.78 (0.56)	0.46
Study 3	0.68 (0.47)	1.97	1.73*** (0.50)	5.66	0.81 (0.65)	2.25
Study 4	−0.84† (0.45)	0.43	1.09* (0.44)	2.97	2.94*** (0.57)	18.97
SDO						
Study 2	1.78* (0.82)	5.94	2.44** (0.84)	11.47	3.05** (1.05)	21.22
Study 3	0.91 (0.78)	2.48	0.66 (0.87)	1.94	1.82 (1.17)	6.15
RWA						
Study 2	−0.27 (0.65)	0.76	0.54 (0.71)	1.71	0.99 (0.95)	2.68
Study 3	−0.98 (0.72)	0.38	1.06 (0.83)	2.88	2.09† (1.15)	8.11
Motivation to control prejudice						
Study 3	0.99 (0.89)	2.69	0.36 (0.94)	1.43	−1.26 (1.14)	0.28
Threat						
Study 1	−4.69*** (0.76)	0.01	−1.14† (0.69)	0.32	5.76*** (0.96)	317.23
Study 2	−1.67*** (0.51)	0.19	0.94† (0.53)	2.56	4.27*** (0.77)	71.59
Study 3	−0.50 (0.51)	0.61	1.55** (0.50)	4.72	3.80*** (0.74)	44.81
Anti-discrimination						
Study 3	0.00 (0.60)	1.00	−1.49* (0.67)	0.23	−3.36*** (1.01)	0.04

Notes. All variables were feature scaled (0–1) before inclusion for ease of comparison. Coefficients and odds ratios therefore represent changes due to movement from the lowest to the highest value on the scale. Coefficient betas represent log odds with standard errors in parentheses. Odds ratios represent odds of category belonging compared to the referent group. SDO = Social Dominance Orientation; RWA = Right Wing Authoritarianism; *SE* = Standard Error. †$p < .09–.05$; *$p < .05$; **$p < .01$; ***$p < .001$.

intolerant groups also emerged for behavioral intentions. A measure of willingness to engage in anti-discrimination activities showed that while people in the liking group were equally likely to engage in anti-discrimination activities as those in the principled intolerant group, those in the prejudiced intolerant and disliking groups were less likely to engage in such activism compared to the principled intolerant group (see ESM 1 for additional tables).

Discussion

The accommodation of Muslim practices in Western societies tends to evoke much political and public debate whereby some sections of the population argue for the acceptance of these practices and others are in favor of banning them (Carol & Koopmans, 2013; Morin & Horowitz, 2006). While previous research has examined how the public evaluates these sorts of practices, this research tends to consider the rejection of these practices as an expression of anti-Muslim feelings (e.g., Helbling, 2014; Saroglou et al., 2009; Van der Noll, 2014). However, while people can reject certain practices because of their prejudicial feelings toward Muslims as a group, they might also be opposed to these practices because of more principled objections (e.g., Gustavsson et al., 2016; Imhoff & Recker, 2012; Van Bohemen et al., 2011). Individuals can be intolerant of specific practices while having either negative or positive feelings toward a group (Hagendoorn & Poppe, 2012; Hurwitz & Mondak, 2002; Sniderman et al., 1989).

We examined majority Dutch reactions to the Muslim minority group and different Muslim practices. Drawing on data from four large datasets covering 4 years, we used latent profile analysis to identify groups of individuals

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across the datasets, and then tested the construct validity of those groups by considering important demographic, psychological, and behavioral correlates of these groups. The advantage of these analyses is that the varying levels of anti-Muslim feelings and rejection to a range of Muslim practices are taken into account and that the heterogeneity of the population is identified.

Across the four datasets and similar to research on political tolerance in different national contexts (McCutcheon, 1985; McIntosh et al., 1995), we identified four latent profiles. In addition to those who generally like and generally dislike Muslims and their practices, we found evidence of two distinct groups of intolerant people that do not explicitly appear motivated by strong negative feelings toward both Muslim groups (but less negative toward Turks compared to Moroccans). One group was intolerant of all or almost all Muslim practices and we labeled this group "prejudiced intolerant." The second one was intolerant of some but not all practices and was labeled "principled intolerance." The principled and prejudiced intolerant groups appeared to represent large sections of society with up to half of the participants in our samples falling into these two groups, indicating the importance of investigating (in)tolerance when seeking to understand intergroup attitudes (Verkuyten & Yogeeswaran, 2017).

We considered several demographic and social psychological variables to examine the meaningfulness of the distinction between the groups. The findings supports the expectation that there are individuals whose rejection of specific practices seems to be guided more by principled considerations than a general dislike toward Muslims. For example, the principled intolerant group is higher educated and more likely to engage in cognitively sophisticated thought than the prejudiced intolerant group. Interestingly, an additional analysis which looked only at the predictive power of cognitive sophistication above and beyond education indicated that adding cognitive sophistication as a predictor had no effect on the role of education in predicting category membership (see ESM 1). In light of past research suggesting that education improves intergroup attitudes through increased cognitive sophistication, this might suggest that education may improve intergroup attitudes by conveying liberal and accepting values or by decreasing feelings of intergroup competition. Level of education is also known as a strong correlate of political sophistication (Highton, 2009; Hillygus, 2005; Tedin, 1987) and of being (in)tolerant as a matter of principle (Sniderman et al., 1989). Similarly, compared to the prejudiced intolerant group, the principled intolerant group is more left-wing politically and research in different Western countries has found that political orientation organizes people's values and beliefs about equality and social deviance (Jost, 2006, 2017; Piurko et al., 2011). Moreover, the principled intolerant group did not only endorse social dominance and authoritarianism less than the prejudiced intolerant group but was also more willing to address the unjust treatment of minority groups.

Our research offers greater nuance than the common distinction between more or less prejudice that is typically used as an underlying continuum for understanding people's attitude toward Muslim minority groups and the different practices they engage in. By making a distinction between people's feelings toward the group of people and toward a range of out-group practices, it is possible to identify a more complex constellation of evaluations. For some individuals, their (un)acceptance of Muslim practices corresponds to their anti-Muslim feelings, but for others it does not. For the principled intolerant group, generally positive group feelings are associated with positivity toward some Muslim practices with disapproval of other practices (i.e., founding Islamic schools and, to a lesser extent, wearing the headscarf). Moreover, not all Muslim practices are rejected to the same extent which indicates that a relative interpretation of rejection is more appropriate than an interpretation in terms of generalized rejection. These findings indicate that rejection of a particular practice (headscarf or Islamic schools) cannot simply be taken to indicate prejudice toward Muslims, and that acceptance of a particular practice does not have to indicate non-prejudicial feelings. Research on anti-Muslim attitudes has examined the extent to which these attitudes reflect prejudice or specific forms of critique based on the endorsement of secularism, and Enlightenment and universalistic values (e.g., Breton & Eady, 2015; Elchardus & Spruyt, 2014; Van Bohemen et al., 2011). Indeed, our analyses indicate that while some people may not openly express prejudice, their objection appears to be guided by hidden prejudice, while others do not express prejudice and appear to be rejecting specific practices as a function of principled objections. Following research on political tolerance, we have tried to argue and demonstrate that it is also useful and important to consider group-based attitudes together with the acceptance of group-specific practices. One can tolerate certain practices of a disliked minority group and when the practice itself is controversial one can be intolerant toward the group one likes or dislikes (Hurwitz & Mondak, 2002; Sniderman et al., 1989).

By also evaluating a range of predictors, we were able to identify differences in attitudes, background, and behavioral intentions between the principled and prejudiced intolerant groups. Thus, despite the group identified as prejudiced intolerant showing a relative absence of prejudice in explicit attitudes toward the Muslim outgroup, their consistent rejection of Muslim practices coupled with differences in predictive attitudes suggests that they may harbor implicit prejudice or have prejudice that they are aware of but are unwilling to express (e.g., Pearson, Dovidio, & Gaertner, 2009).

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However, although we found similar patterns in four different datasets we need to be careful about generalizing the specific content and the size of the different profiles. The findings of latent profiled analyses and person-centered approaches are context-specific and sensitive to the practices that are considered. The practices used in this research reflect issues that are broadly debated in Western Europe and the profiles indicate how people tend to group these issues. However, different profiles might emerge if different practices were considered. For instance, the consideration of more demanding issues (e.g., arranged marriage, Sharia ruling in the Netherlands) could result in very skewed distributions of answers with different profiles as a consequence, such as the "liking" and the "principled intolerant" groups not sustain their overall acceptance of various practices (Gibson, 2005). Further research is needed to test whether this pattern is broadly replicated across a range of other Muslim minority practices.

Furthermore, although we considered familiar predictors of prejudice and tolerance, it is important to note that our indicators were not explicitly developed for the current analysis. Additionally, while our analysis of predictors of membership in the different groups drew on a wide range of predictors, these predictors did not always use identical items and were not present in all of the four datasets. Therefore, while some predictors have evidence from across multiple datasets, others are present in only one dataset and thus provide weaker evidence. Further research using similar items in new datasets may allow us to further update these findings. In addition, future research could consider other important predictors such as intergroup contact, need for closure, and cultural diversity beliefs as these might provide a further understanding of the differences between the groups of individuals.

Lastly, it is important to note that we focused our discussion on the more stable patterns we found in our datasets and not on the differences between the datasets. While differences are relevant and may be informative, it can be difficult to know with any degree of certainty whether they are due to random noise or whether they reflect sociopolitical factors (see, e.g., our discussion on attitudes toward Turks in Study 4 in the Out-Group Feelings section). Therefore, we focus on between-sample similarities that we find across multiple datasets collected over a 4-year period. This allows us to assess the probability that the results we discuss represent more general patterns of population characteristics.

Conclusion

In light of the important academic and societal debates on the acceptance and accommodation of Muslims in Western societies, it is critical to parse between forms of intolerance of particular practices that represent more principled positions on complex matters of policy and those which tend to justify the disliking of Muslim minorities. In this research, we found support for both sides of competing perspectives on the source of intolerance and we have explained in more detail how these groups differ. Many majority members are struggling with questions around immigration and Muslim minorities, and the acceptance of dissenting minority practices in particular. Psychologically various types of feelings, beliefs, norms, and values come into play and the weighting and balancing of these considerations against each other is not easy (Verkuyten & Yogeeswaran, 2017). A social psychological perspective that tries to understand the rejection of specific minority practices only in terms of prejudicial attitudes is limited, as is a perspective that ignores the justification of prejudicial feelings and negative beliefs (Crandall & Eshleman, 2003). Ordinary people are influenced by their group-based likes and dislikes but are also capable of considering different principles and values, including the importance of tolerance. Using a person-centered approach makes it possible to identify unobserved groups of individuals who differ in the particular ways in which they try to combine their out-group feelings and evaluations of a range of out-group practices (McCutcheon, 1985; Meeusen et al., 2018). These groups cannot be placed on a unidimensional prejudice continuum but rather form latent classes of majority group members who differently combine their general feelings toward Muslim groups and their acceptance of Muslim practices. In this way a more nuanced understanding of majority members' evaluation of minority groups and minority practices can be provided which is critical for the continuing social and theoretical debates.

Electronic Supplementary Material

The electronic supplementary material is available with the online version of the article at https://doi.org/10.1027/1864-9335/a000380

ESM 1. Questionnaires, Tables, and Mokken analysis

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History
Received July 10, 2018
Revision received December 12, 2018
Accepted January 3, 2019
Published online August 20, 2019

Authorship
Maykel Verkuyten collected the data; both authors were involved in all other parts of the research.

Open Data
Complete data and analytic scripts can be found on the Open Science Framework at https://osf.io/7j2zm/.

Funding
This contribution was supported by a European Research Council (ERC) Advanced Grant under the European Union's Horizon 2020 research and innovation programme (grant agreement No 740788).

ORCID
Levi Adelman
https://orcid.org/0000-0002-8936-9036

Levi Adelman
ERCOMER
Sjoerd Groenmangebouw
Padualaan 14
3584 CH Utrecht
The Netherlands
l.y.adelman@uu.nl

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Original Article

Perceived Biological and Social Characteristics of a Representative Set of German First Names

Tillmann Nett[1], Angela Dorrough[2], Marc Jekel[2], and Andreas Glöckner[2]

[1]Department of Psychology, FernUniversität in Hagen, Germany
[2]Faculty of Human Sciences, University of Cologne, Germany

Abstract: We provide ratings for a representative set of 2,000 German first names with regard to perceived sex, foreign origin (yes/no), and familiarity. In two studies participants (N = 736 and N = 237) estimated intelligence, education, attractiveness, religiousness, age, warmth, and competence of persons with the respective name. Descriptive results show strong stereotypes in society in that most of the top-rated names on intelligence, competence, and religiousness were male, whereas all top-rated names on attractiveness and warmth were female. The reliability of most ratings is satisfactory. We provide correlations between the rated dimensions to give an overview of the internal structure of the dataset. To enhance usage of the dataset, we provide an R-package, which allows querying subsets of names depending on experimental requirements.

Keywords: first names, word norms, social perception, stereotypes, German language

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A common experimental manipulation in the area of social psychology is to present first names to signal group membership (e.g., gender: Brosi, Spörrle, Welpe, & Heilman, 2016; Heyder & Kessels, 2015; Moss-Racusin, Dovidio, Brescoll, Graham, & Handelsman, 2012; Steinpreis, Anders, & Ritzke, 1999; ethnic groups: Bertrand & Mullainathan, 2004; Lütkenhöner, 2011; different ages: Kuhlmann, Bayen, Meuser, & Kornadt, 2016), to manipulate perceived characteristics, such as intelligence or attractiveness (e.g., Gebauer, Leary, & Neberich, 2012; Greitemeyer & Kunz, 2013; see also Newman, Tan, Caldwell, Duff, & Winer, 2018) or to allow participants to follow a narrative in multiple vignettes (Newman et al., 2018). For a successful manipulation, name carriers must actually be perceived to belong to the intended group and names need to be indeed associated with the intended characteristics. Furthermore, other characteristics associated with the respective names must be comparable and thus unconfounded with group membership (e.g., Böhm, Schütz, Rentzsch, Körner, & Funke, 2010; Brosi et al., 2016; Heyder & Kessels, 2015; Moss-Racusin et al., 2012; Schulz, Rudolph, Tscharaktschiew, & Rudolph, 2013; Steinpreis et al., 1999; I. Winkler, Jonas, & Rudolph, 2008, all controlled for one or multiple perceived characteristics of the given names). Typically, researchers in previous studies using first names either generated small ad hoc samples of names that were rated by a small number of participants in a pilot study (e.g., Bertrand & Mullainathan, 2004; Lütkenhöner, 2011; Stevens, Volstorf, Schooler, & Rieskamp, 2011), re-used names from previous studies with similar research questions (e.g., Gebauer et al., 2012; Moss-Racusin et al., 2012; Steinpreis et al., 1999), or referred to existing validated sets of first names (e.g., Böhm et al., 2010; Brosi et al., 2016; Greitemeyer & Kunz, 2013; Heyder & Kessels, 2015; Kuhlmann et al., 2016; Schulz et al., 2013; I. Winkler et al., 2008). For German names, the dataset provided by Rudolph, Böhm, and Lummer (2007; see also Rudolph & Spörrle, 1999) is most frequently used. This dataset includes the 60 most common German first names (30 male and 30 female), rated by 149 participants in terms of age, intelligence, attractiveness, and religiousness. For names from the United States a dataset is provided by Newman et al. (2018), which includes 400 names (200 male and 200 female), with the names rated by 497 participants in terms of age, competence, and warmth. In some of the existing datasets also other characteristics such as topicality or sex are included. Typically, these characteristics have been measured based on demographic statistics. For example, Rudolph et al. (2007) categorized German first names with regard to topicality (modern, old-fashioned, or ageless) and sex based on demographic statistics about the allocation of

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these names to newborns in the years between 1960 and 2004. Similarly, Bertrand and Mullainathan (2004) used statistics about the ethnicity from birth certificates to determine the ethnic membership of a name. While those measures by definition reflect the true relation between name and name carriers (e.g., frequency of names for different ethnic groups), people may have incorrect beliefs about the true relations. Thus, those measures might not be a valid indicator for the potentially distorted perceived relations between names and characteristics. In addition, the definition of the characteristics underlying these measurements may be different from how people actually perceive these characteristics (e.g., problems with construct validity).

In addition to these potential limitations for the use of past validation studies, the number of names included in those study was typically low. Experiments on decision making, for example, often require a large number of trials (e.g., Dorrough, Glöckner, Betsch, & Wille, 2017; Stevens et al., 2011) for which a larger number of names would be desirable to avoid repeating the same names across different trials. Repeating the same name may introduce undesired effects such as increased liking due to mere exposure (Moreland & Zajonc, 1982). Furthermore, the experimental design may require different information to be conveyed to the participants in different trials. Another experimental constraint, which can increase the number of required names, is that some names may not be usable in some studies. For example, Steinpreis et al. (1999) investigated the impact of stereotypes among psychologists using different first names for otherwise equivalent CVs. To avoid confusion with any real existing psychologists and the ones given in the CVs, they ensured that the names used in the study did not appear in the APA membership directory. For small sets of names (e.g., Rudolph et al., 2007), removing names may be impossible or may result in a set of names that are not comparable with respect to other perceived characteristics. Similarly, Rentzsch, Schütz, and Schröder-Abé (2011) specifically mentioned that they did not use any names in their study, as the current existing norms for German first names did not allow them to identify enough names rated similarly in terms of intelligence and attractiveness. By providing a much larger set of names, researchers can remove a larger number of names, which may be problematic for their design, and still be left with a usable number of validated names.

Furthermore, but related to the previous issue, a name set representative of a certain reference set often needs to be selected for generating internally and externally valid results (Brunswik, 1955; see also Newman et al., 2018, for this argument specifically applied to names). Generating such a subset requires a representative set and ratings on relevant selection criteria to begin with. For names, such a set has not been provided so far. Ad hoc selections of names based on experimenters' intuition or mental simulation as well as some kinds of piloting can artificially increase estimates of the true effect and should, therefore, be avoided (Fiedler, 2011). Even more so, previous studies investigating the names used in psychological experiments have shown a systematic tendency to use names, which can strongly bias the findings in the direction required by the researcher (Kasof, 1993). For example in research on stereotpyes, male names were often associated with higher intelectual competence than the female names, with which they were contrasted.

Finally, although ratings of names in prior studies cover plenty of perceived characteristics, ratings on two fundamental dimensions of social perceptions, namely warmth and competence, are usually missing (i.e., Stereotype Content Model, SCM; Fiske, Cuddy, Glick, & Xu, 2002). In research on stereotypes, the SCM has been shown to be a valuable tool for predicting the attitude and behavior toward members of a group. According to this model, the attitude toward a member of a group is governed by the perceived warmth and competence of that group (Fiske, Cuddy, & Glick, 2007). To fill this gap, we also included items from a German questionnaire of warmth and competence (Asbrock, 2010). In previous research, these items have been only used to analyze the perception of social groups, such as immigrants, women, or homeless people but not for names. We will thus also analyze if we can measure these variables reliably and furthermore if we are able to identify the two factors of warmth and competence also for name ratings. In a similar approach, Newman et al. (2018) also included items for warmth and competence when collecting ratings of names for use in the United States. However, Newman et al. (2018) only used single items for collecting the competence and warmth ratings, wheras we used a set of six validated items (Asbrock, 2010).

In sum, we extend prior studies on name sets in three ways. First, we provide a large set of 2,000 representative German male and female names rated by 973 participants. Second, we provide direct ratings on the perceived topicality of names instead of using demographic statistics. Third, we provide additional ratings on name characteristics such as warmth and competence (Asbrock, 2010; Fiske et al., 2002, 2007) that have not been systematically assessed in most other name studies before.

Methods

The software for conducting this research can be retrieved from the Open Science Framework (OSF) at https://osf.io/jepzp/. The software is put under an open license (MIT open source license) such that it may be freely adapted an re-used for future research. While the current implementation is in German to generate a German name set, the software is written such that it may easily be

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adapted for other countries or languages. For example, all questionnaires are defined in an easy to understand XML format that may be translated into other languages even with little programming knowledge. In addition, the format used to define the questionnaires provides a simple method to add or remove questions.

Participants

In the first study, we collected data from 736 participants who rated subsets of names. These participants were recruited using a participant pool of students from the FernUniversitt in Hagen as well as through social media. Participants studying psychology at the FernUniversität in Hagen received partial course credit for taking part in the survey. Each participant rated $M = 54.08$ names on average ($SD = 29.06$) and thus we collected about 20 ratings per name ($M = 19.90$, $SD = 0.72$). With only 20 ratings per name, however, the distribution of the collected ratings may not sufficiently reflect the real distribution. Thus, for example, the mean ratings for each name may have a large measument error (e.g., difference between estimated mean and true population mean), which may pose a problem when selecting names for other studies based on these ratings. Thus in a second study, we recruited 237 additional participants. These participants did not receive course credit but instead were paid 15 € for taking part in the survey. The names rated by these participants were a subset of the names used in the first study, such that we could collect particularly precise ratings for this subset of names. For all analyses, data from both studies were combined into a single dataset. All participants in both studies indicated that they were fluent in German. One hundred seven participants indicated they were not from Germany or did not provide any information about their origin. The mean age of the participants was 34.24 years ($SD = 10.69$; ages above 78 were imputed as 78 since no selection of birthdates before 1940 was possible). Of the analyzed participants, 73% participants were female, 27% participants were male, and 0.31% identified as neither male nor female. The majority of participants were students (62%) and/or employed (51%) with the majority of the students in the field of psychology (83%).

Materials: Selection of the Initial Nameset

To select a set of names, we started with a large initial set of 8,173 names taken from a German name dictionary (Duden, 2007). This dictionary contains two tables of male and female names, which we scanned and translated to text using optical character recognition (OCR). To check for errors during translations, we checked all names against a corpus of German words generated from newspaper articles from 2011 and 2012, which has been made available as part of the "Leipzig Wortschatz" project (Biemann, Heyer, Quasthoff, & Richter, 2007) and manually corrected where necessary. In cases where two names were very similar to each other, only the more common name (e.g., the one with the highest number of occurrences in the corpus) was kept. The similarity was determined using the following criteria: (1) The names differ only in terms of diacritics (äüößé). For example, "Jérôme" and "Jerome" were considered different forms of the same name and Jerome was kept because of the higher occurrence in the corpus (10 names were removed because they were considered to be the less common form of another name). (2) The names were similar in terms of sound *and* also similarly spelled. The sound similarity was calculated using the "Kölner Phonetik" (Postel, 1969). The Kölner Phonetik translates words or names to soundcodes, which correspond to the perceptual features of the name in German (e.g., by encoding guttural and plosive phones differently). For all names which were similar in terms of sound, similarity in spelling was checked using the Jaro-Winkler similarity (W. E. Winkler, 1990; see also W. Cohen, Ravikumar, & Fienberg, 2003) on character sequences ranging from 0 (= *the names are very dissimilar*) to 1 (= *the names are exactly the same*). Names were considered similar if the Jaro-Winkler similarity was above .8. The value of .8 was chosen, such that as many names as possible were removed, while still keeping at least 1,500 male and female names in the generated dataset (4,095 names were removed because they were too similar to another name).[1] In addition to removing similar names, we also removed any name that appeared both in the male and the female table of the name dictionary (210 names were removed due to sex ambiguity) and all names which appeared in neither corpus (2011 or 2012) due to being very uncommon in Germany (e.g., "Ermengard" or "Jodyokus"; 922 names were removed as uncommon). A full list of names and reasons for their removal is provided on OSF. The complete set of initial names, the number of occurrences from the two corpora and the generated soundcodes have also been uploaded to OSF. Furthermore, all python scripts, which were used to query the corpora, generate the soundcodes, and filter the names are provided on OSF for replicability.

This method left us with a set of 1,804 male and 1,524 female names. From these names, the 1,000 names for

[1] For example, the name "Adda" was removed because it was similar sounding and spelled similarly to the more frequent "Ada" (Jaro-Winkler similarity: .92); however, the name "Kimiko" was not removed although it had the same soundcodes as "Chinook," "Kinga," and "Ganga" because all three alternatives differed considerably in spelling (Jaro-Winkler similarity $\leq .60$).

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Table 1. Rated dimensions and derived variables

Dimension	Type	Code	*M* (*SD*)	Reliability [95% CI]	N_{eff}[e]
Sex (weighted)[a,c]			0.30 (6.11)	.99 [0.98, 1.00]	55,955
Sex	Categorical	−1: Female, +1: Male	0.06 (1.00)	.99 [.98, .99]	55,955
Sex (certainty)	7-point Likert		5.85 (1.79)	.92 [.90, .93]	9,891
Topicality	Categorical	1-hot[f]			
Modern[a,c]			0.23 (0.42)	.82 [.79, .85]	2,717
Old[a,c]			0.40 (0.49)	.90 [.88, .92]	4,733
Ageless[a,c]			0.37 (0.48)	.75 [.71, .79]	3,067
Education[c]	7-point Likert		4.24 (1.22)	.72 [.67, .76]	6,812
Age[c]	Multiple Choice	$\leq$ 20: 1, 20–30: 2, …, $\geq$ 61: 6	3.39 (1.45)	.92 [.90, .94]	19,153
Attractiveness[c]	7-point Likert		4.13 (1.26)	.77 [.73, .80]	7,775
Intelligence[c]	7-point Likert		4.26 (1.18)	.66 [.61, .71]	5,680
Religiousness[c]	7-point Likert		3.81 (1.44)	.77 [.73, .80]	8,385
Competence (SCM)[a,c]			4.29 (1.06)	.58 [.51, .64]	3,606
Competent[b]	7-point Likert		4.25 (1.15)	.59 [.53, .65]	4,924
Competitive[b]	7-point Likert		4.18 (1.18)	.53 [.46, .60]	5,677
Independent[b]	7-point Likert		4.43 (1.23)	.48 [.40, .55]	4,410
Warmth (SCM)[a,c]			4.34 (1.07)	.58 [.51, .63]	3,343
Likable[b]	7-point Likert		4.37 (1.19)	.55 [.48, .61]	4,601
Warm[b]	7-point Likert		4.33 (1.17)	.56 [.49, .62]	4,718
Good natured[b]	7-point Likert		4.32 (1.17)	.51 [.44, .58]	4,324
Nationality[c]	Categorical	0: Foreign, 1: German	0.50 (0.50)	.95 [.94, .96]	2,280
Familiarity[c]	7-point Likert		3.02 (1.72)	.93 [.91, .94]	6,626
Associations[d]	Free text				

Notes. [a]Derived variable. [b]Used in the factor analysis of warmth/competence (see Section "Ratings of Warmth and Competence"). [c]Used during dimensionality reduction (see Section "Choosing Similar Names" and https://osf.io/hcx2v/). [d]Provided as is for future research but not included in any analysis here. [e]Effective sample size based on design effect correction using the intracluster correlation coefficient (ICC) with participants as clusters. [f]1-hot coding of categorial variable with three levels as three separate numerical variables; Modern: (1, 0, 0), Old: (0, 1, 0), Ageless: (0, 0, 1).

each sex according to the name dictionary (Duden, 2007), which had the highest total occurrence in the newspaper corpora for 2011 and 2012, were used as representative German names.

Procedure

Ratings were collected using an online survey, which was programmed in oTree (Chen, Schonger, & Wickens, 2016). A translated example of the full survey is provided at https://osf.io/erykn/. The original in German can be found on OSF at https://osf.io/uwdt9/. A full list of dimensions on which each name was rated is summarized in Table 1. To assess these dimensions, we asked participants to indicate the ratings for the average person with this name (e.g., whether the average person with this name is female or male; not at all vs. very educated/intelligent; etc.).

Participants were asked to agree with a statement of consent about data collection and usage before starting with the main part of the study and provided demographic data. They were then directed to the main survey, in which each participant was asked to rate a subset of the names. Each name and the associated ratings were presented on a separate page. Most items were taken from the study by Rudolph et al. (2007), currently the most extensive existing validated name set for German first names. Furthermore, as outlined above, we also included questions about the perceived sex of the name and its topicality (modern, old, or ageless) and items to measure perceived warmth and competence (Asbrock, 2010). In addition, participants indicated how certain they were about the associated sex, whether they considered this name to be a German name and how common they believed this name to be in Germany. Finally, to also collect open-ended perceived characteristics with the names, we provided a text field in which participants could provide any association they had with that name. We do not analyze these open answers in the current article but they might be used in future research to extract potential stereotype dimensions for names (cf. Koch, Imhoff, Dotsch, Unkelbach, & Alves, 2016). All ratings except for sex, age, age category, origin, and the free written associations were collected using a 7-point Likert scale with labels only at the endpoints of the scale (e.g., *not intelligent at all* vs. *very intelligent*). The ratings for age were collected using a 6-point

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scale with age ratings between 20 years and 60 years in intervals of 10 years (1 = *less than 20*, 2 = *20–30*, 3 = *31–40*, 4 = *41–50*, 5 = *51–60*, 6 = *more than 61*; Rudolph et al., 2007). The ratings for sex, age category, and origin were collected using drop-down lists, from which the participants could select the appropriate response.

To generate the stimulus material for participants in the first study, we constructed sets of 75 different names from all 2,000 names, such that each name was used exactly 15 times in each set (400 sets in total). These sets were then used in the first round of the survey. However, since some of the initial 400 participants did not finish the survey, the frequency of ratings for each name differed at this point. Therefore, after the first phase of data collection, we created novel sets of 75 names, in which the names that previously had received a lower number of ratings were included more often. As before, participants never rated the same name twice. This process was repeated until we had at least 15 ratings for each name. The order in which the names were presented was randomized during trial generation. In the first study, we were able to achieve about 20 (M = 19.90, SD = 0.72) ratings per name. To collect more ratings per name for some names, in the second study we selected 200 names which were rated by new participants. These 200 names included 45 names that were also included in the study by Rudolph et al. (2007; see Table 2 for a complete list). In addition, we included names based on the following procedure: First we assigned sex and topicality categories to all names, such that each name was assigned the sex and topicality category that was chosen most often by participants in the first study. Based on these sex and topicality categories, we split our dataset into six groups (3 Topicality Categories × 2 Sex Categories). From each of the six groups we selected those names rated as most familiar on average in the first study, such that an approximately equal number of names was selected from each of the groups. Participants in the second phase were given random sets of 75 names sampled from these 200 names only. The participants included in the analysis (both studies) rated M = 57.51 names on average (SD = 27.65). Because we also included data from participants, who did not complete the survey, the number of names rated are less than 75 for some of the participants. Each of the 2,000 names was rated between 17 and 103 times in total (M = 27.98, SD = 23.9) for a total of 55,955 name ratings.

After rating the names, participants were thanked and redirected to a page, where they could collect the course credit for the survey and send an email to receive additional information about the goals of this study. The goals of this study were not disclosed before all data were collected.

Results

Analyses were conducted using the R programming language version 3.4.1 (R Core Team, 2017) with the tidyverse set of packages (Wickham, 2017) for data preparation and wrangling. The original document for this paper used knitr (Xie, 2018; see also Xie, 2014) to embed R code into the document to ensure reproducible research (De Leeuw, 2001) and to prevent transcription errors of the computed values (Nuijten, Hartgerink, van Assen, Epskamp, & Wicherts, 2016). Figures are generated using the ggplot2 package (Wickham et al., 2018). The complete set of scripts, seed values for the random number generator, and original raw data files used to compute the analyses is provided on the OSF.

Since our analyses aim to provide insights into the structure and quality of the collected dataset and are not meant to test any scientific research hypotheses, we present descriptive statistics, effect sizes, and the confidence intervals of these effect sizes. For all analyses, which were conducted multiple times (e.g., for the reliability of multiple ratings), we adjusted individual confidence intervals such that an aggregate confidence of 95% is assured (Bonferroni corrected confidence intervals).

Descriptive Results

Table 3 provides descriptive results for the 10 highest and lowest rated names for the dimensions intelligence (Table 3A), education (Table 3B), attractiveness (Table 3C), religiousness (Table 3D), familiarity (Table 3E), and age (Table 3F). The descriptive results show strong prevailing gender stereotypes in German society that are attributed to the average persons with male versus female names. Within the top rated names for intelligence and religiousness, there were almost exclusively male names, with the only exception of the name "Mitsuko" among names rated as most educated and the name "Aygül" among the names rated as most religious.[2] The female name rated as most intelligent was Viktoria with an average rating of M = 5.3 (rank 11). The reversed picture emerges for the dimension attractiveness in which the top names included only female names. For attractiveness, the male name with the highest rating was Raul with an average rating of M = 5.25 (rank 13). These observations can mainly be confirmed for the complete dataset. For all names the ratings for sex (weighted) (scale: −1 = *certainly female* to 1 = *certainly male*; sex rating weighted by confidence, details below) and intelligence correlated significantly even after controlling for

[2] The appearance of these names is most likely caused by stereotypes about the "efficient Asian" (Asbrock, 2010; Fiske et al., 2002) and "religious Muslim" (Koch et al., 2016), which may overrule the "women" stereotype for these two names.

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Table 2. Demographic topicality from Rudolph et al. (2007) and topicality ratings from this study for all names used in both studies

			Ratings (this study)		
Name	*N*	Topicality Rudolph et al. (2007)	Ageless (%)	Modern (%)	Old-fashioned (%)
Alexander	100	Ageless	81	6	13
Andreas	101	Ageless	65	8	27
Christian	100	Ageless	80	8	12
Claudia	100	Ageless	49	9	42
Cornelia	99	Old	32	4	64
David	100	Modern	74	13	13
Dirk	98	Old	38	4	58
Felix	100	Modern	55	32	13
Florian	99	Modern	63	26	11
Frank	100	Old	33	8	59
Heike	100	Old	34	2	64
Heiko	101	Old	27	9	64
Holger	100	Old	17	3	80
Ines	98	Old	51	14	35
Jan	100	Modern	68	27	5
Jens	100	Old	55	15	30
Johanna	101	Modern	72	9	19
Jörg	98	Old	21	8	70
Katharina	99	Modern	79	7	14
Kerstin	100	Old	43	9	48
Laura	101	Modern	62	35	3
Lea	100	Modern	45	54	1
Lena	98	Modern	56	41	3
Leon	100	Modern	40	60	< 1
Leonie	99	Modern	33	62	5
Luca	100	Modern	28	69	3
Lukas	100	Modern	73	19	8
Manuela	99	Old	38	4	58
Maria	99	Ageless	70	3	27
Mario	99	Old	54	20	26
Matthias	100	Ageless	71	6	23
Maximilian	99	Modern	68	14	18
Michael	101	Ageless	75	4	21
Mike	100	Old	37	55	8
Olaf	100	Old	26	5	69
Paul	100	Modern	74	5	21
Peter	101	Old	43	3	54
Petra	99	Old	37	2	61
Sabine	99	Old	37	8	55
Sarah	100	Modern	82	15	3
Silke	99	Old	32	6	62
Susanne	102	Ageless	40	6	54
Thomas	99	Ageless	69	6	25
Tim	100	Modern	59	37	4
Uwe	100	Old	9	5	86

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Table 3. Highest and lowest rated names concerning (A) intelligence, (B) education, (C) attractiveness, (D) religiousness, (E) familiarity, and (F) age (scale: 1 (= *not at all*) to 7 (= *very*))

Name	*M*	*SD*
(A) Intelligence		
Chen	5.65	0.88
Primus	5.63	1.01
Augustinus	5.60	1.19
Bartholomäus	5.45	1.43
Graham	5.42	1.12
Amadeus	5.35	1.09
Aristoteles	5.35	1.63
Cornelius	5.35	0.88
Fitzgerald	5.35	1.23
Justus	5.35	1.18
...	...	...
Igor	3.05	1.39
Chantal	3.05	0.83
Cindy	3.01	1.29
Mandy	2.91	1.20
Fifi	2.90	1.07
Dolly	2.89	1.15
Kevin	2.86	1.22
Cheyenne	2.79	0.92
Jacqueline	2.76	1.18
Candy	2.74	0.87
(B) Education		
Bartholomäus	5.75	1.48
Nathan	5.60	1.14
Primus	5.58	1.12
Amadeus	5.55	1.10
Augustinus	5.55	1.36
Laurentius	5.55	1.19
Graham	5.47	1.43
Cornelius	5.45	1.15
Mitsuko	5.42	1.26
Jacques	5.40	1.14
...	...	...
Aga	2.89	1.20
Cindy	2.84	1.29
Jacqueline	2.79	1.36
Mandy	2.78	1.37
Kevin	2.76	1.27
Destiny	2.70	1.34
Cheyenne	2.68	1.00
Dolly	2.58	1.17
Fifi	2.50	1.10
Candy	2.42	0.90
(C) Attractiveness		
Flora	5.60	1.27
Liz	5.55	1.10
Fleur	5.50	1.19
Grace	5.48	1.12
Aurora	5.40	1.35
Giulietta	5.40	1.39
Viktoria	5.40	0.99
Serena	5.35	1.27
Victoria	5.35	0.88
Laetitia	5.30	1.13
...	...	...
Fritz	2.80	1.15
Winifred	2.78	1.22
Ottmar	2.75	1.02
Hartmut	2.75	1.07
Ekkehard	2.75	1.16
Gottwald	2.70	1.17
Arnulf	2.70	1.17
Adolf	2.65	1.42
Igor	2.58	1.22
Ottfried	2.53	1.17
(D) Religiousness		
Evangelist	5.75	1.33
Hakan	5.75	1.16
Jesus	5.75	1.12
Aygül	5.70	1.13
Moses	5.70	1.26
Abraham	5.65	1.50
Franziskus	5.65	1.09
Josefa	5.65	1.31
Khalid	5.65	1.35
Paulus	5.60	1.23
...	...	...
Tilly	2.55	1.36
Roxy	2.55	1.19
Kelvin	2.55	1.19
Bibi	2.55	1.39
Kevin	2.55	1.31
Torben	2.47	1.22
Guy	2.35	1.04
Jacqueline	2.31	1.28
Dexter	2.28	1.41
Chanel	2.15	1.14
(E) Familiarity		
Michael	5.50	1.39
Christian	5.47	1.37
Stefan	5.39	1.26
Andreas	5.34	1.28
Alexander	5.27	1.44
Martin	5.27	1.32
Lisa	5.17	1.56
Daniel	5.16	1.42
Peter	5.16	1.32

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Table 3. (Continued)

Name	*M*	*SD*
Sabine	5.15	1.34
...	...	...
Eitel	1.19	0.60
Kraft	1.17	0.51
Andere	1.17	0.38
Quincy	1.15	0.49
Hai	1.15	0.49
Focke	1.15	0.37
Winnetou	1.14	0.48
Arpad	1.10	0.31
Solange	1.05	0.22
Guadalupe	1.05	0.22
(F) Age		
Klothilde	5.85	0.37
Edelgard	5.80	0.41
Gerhild	5.80	0.41
Sigismund	5.80	0.41
Friedewald	5.79	0.54
Brunhild	5.78	0.55
Irmhild	5.72	0.96
Gertrud	5.68	0.60
Adalbert	5.67	0.73
Ewald	5.65	0.49
...	...	...
Justin	1.71	0.76
Faith	1.70	0.92
Destiny	1.70	0.92
Janelle	1.67	0.77
Vanilla	1.65	0.81
Jara	1.65	0.93
Cinderella	1.65	0.81
Emily	1.62	1.02
Finn	1.62	0.83
Fia	1.55	1.23

Notes. (A) Correlation with sex (weighted): $r(1{,}998) = .10$ [.01, .18], $p < .01$; (B) Correlation with sex (weighted): $r(1{,}998) = .12$ [.03, .20], $p < .01$; (C) Correlation with sex (weighted): $r(1{,}998) = -.35$ [−.42, −.27], $p < .01$; (D) Correlation with sex (weighted): $r(1{,}998) = -.03$ [−.11, .05], $p = .20$; (E) Correlation with sex (weighted): $r(1{,}998) = -.01$ [−.09, .08], $p = .82$; (F) Correlation with sex (weighted): $r(1{,}998) = .14$ [.06, .22], $p < .01$.

multiple comparisons,[3] confirming that males are assumed to be more intelligent than females. This effect was not moderated by the gender of the participants (see see https://osf.io/b6kfn/). Similarly, there also was a significant negative correlation between ratings of sex (weighted) and attractiveness, confirming that females are rated to be more attractive than males. Again, this effect was not moderated by the gender of the participants (see https://osf.io/b6kfn/). Only for religiousness the effect did not hold in an overall analysis but was only apparent in the extremes.

Reliability of Ratings

Since this study is based on a repeated measurement approach, ratings from the same participant may be correlated with each other, and thus the number of 55,955 collected ratings only insufficiently reflects the amount of collected information. Similarly, the number of ratings per name, and the number of names also only insufficiently reflect the amount of collected information. To estimate the actual information contained in the provided dataset, we calculated the effective sample sizes N_{eff} (Hox, Moerbeek, & Van de Schoot, 2017) for each measurement, which are reported in Table 1.[4] The names included in the first study only have about 20 ratings for each attribute. Since low sample sizes in correlational studies are often linked to unreliable findings (Schönbrodt & Perugini, 2013) and inflated effect sizes (Gelman & Carlin, 2014), one might expect results from statistical analyses on these names to be unreliable. We circumvented this probelm by conducting most analyses on the complete dataset either after calculation of the mean ratings, or without aggregation. While the low number of ratings for names only included in the first study may still cause individual means to deviate from the true population mean, this effect vanishes when all names are included in an analysis, and the effective sample sizes then give a much more realistic impression of the reliability of the results. Since all effective sample sizes are above 2,000, the chances that any of the results presented here are unreliable and would disappear with larger sample sizes (Schönbrodt & Perugini, 2013) can be considered neglible.

In addition, to test how well ratings from different participants for the same name corresponed to each other, we calculated split-half reliabilities using the formula for the two-part alpha reliability coefficient ($r_{2\alpha}$) and the corresponding confidence intervals for perceived characteristics to assess in how far different participants perceive names in a consistent way (Charter, 2000; see also Kristof, 1969). For this, we split all individual ratings for the same name randomly into two sets and calculated the mean

[3] We tested all possible correlations between the collected ratings (78 comparisons). Therefore, significance was tested at $\alpha = .00064$. See Table 3 for *r* and *p*-values.

[4] The effective sample size for a repeated measurement design is an indication of the numbers of samples required to gather the same amount of information without repeated measures. While the effective sample size can also be used to determine the *df* of a statistical test, for most tests we instead chose a conservative approach by only basing the *df* on the number of names.

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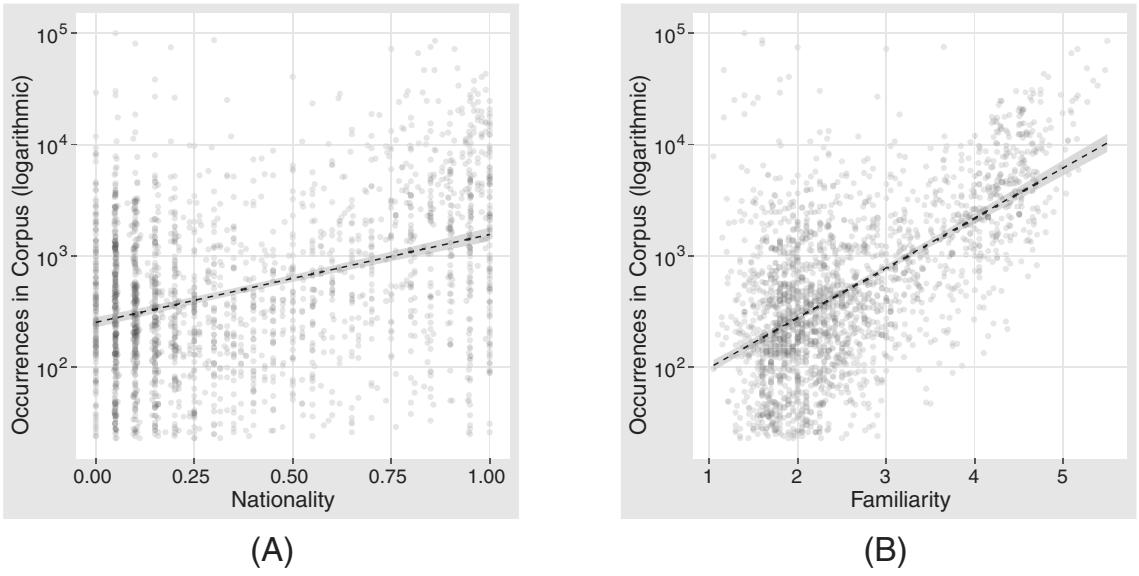

Figure 1. Correlation between the frequency of occurrences and nationality ratings (A) and familiarity ratings (B).

ratings for each name separately. The paired-sample correlations (r_{XY}) of the mean ratings for each name were then used to derive maximum likelihood estimates of the reliability coefficient alpha ($r_{2\alpha}$) using the formula provided by Charter (2000). We used maximum likelihood estimators for two-part alpha instead of Spearman-Brown corrections for split-half reliability since two-part alpha, in general, provides more reliable estimates and confidence intervals (Charter, 2000). We repeated this random splitting 1,000 times and averaged the resulting reliability scores and confidence intervals.[5] For sex ratings, to also include the confidence participants had in their choice, we multiplied the recoded sex variable (−1 for female and +1 for male) by the numeric confidence variable (weighted sex ratings) to achieve ratings that can be interpreted as more or less certainly female/male. Higher positive values thus reflect more certain ratings of male sex while lower negative values indicate more certain ratings of female sex and values close to zero uncertain sex ratings. For the categorical topicality variable, we recoded all ratings using 1-hot coding.[6] Overall, reliabilities differed largely between items (Table 1), ranging from $r_{2\alpha} = .48$ for the item independency to $r_{2\alpha} = .99$ for the weighted sex ratings. All variables, which described objective characteristics of a person (age, sex, and nationality) but also familiarity of the name in Germany (which is directly related to nationality) showed excellent reliability scores ($r_{2\alpha} \geq .92$). Subjective ratings of attractiveness, intelligence, education, and religiousness were of moderate reliability ($.66 \leq r_{2\alpha} \leq .77$). The reliability of the warmth and competence variables (Asbrock, 2010) showed poor reliability ($.48 \leq r_{2\alpha} \leq .56$), indicating that different participants rated the same name very differently on these items. Also, variables for warmth and competence were similar to each other with regard to their reliability. Finally, topicality showed acceptable reliability ($.75 \leq r_{2\alpha} \leq .90$).

Validity of Ratings for Familiarity and Sex

To test the convergent validity of the ratings for familiarity and sex, we correlated those variables to external criteria that should be related. For sensible familiarity ratings, the frequency of occurrence of a name in a German text corpus should be correlated with participants' ratings of familiarity. Additionally, more frequent names in a German text corpus (Biemann et al., 2007) are more likely of German than of foreign origin. As predicted, a comparison between the ratings of nationality (German or foreign name) and familiarity of the names with the frequency of occurrences in the text corpus showed a medium correlation between

[5] Separate confidence intervals were computed for each split using the formula provided by Charter (2000). The values from all these estimates were then averaged to reduce the influence of each single split. Since we only varied the random splits of the datasets while keeping all ratings, the method we used cannot be considered a bootstrap, and therefore the individual estimates may not be used when determining the confidence intervals.

[6] 1-hot or one-of-many coding recodes a categorical variable with K categories into K separate variables. This is similar to dummy coding, which, however, only uses $K - 1$ variables and takes one of the categories as a reference category. We chose 1-hot coding instead of dummy coding, because it does not require choosing a reference category, to which all other categories are compared. Also 1-hot coding allows us to report data for all three topicality categories and does not require omission of the reference category.

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the ratings for the nationality and the logarithm of the occurrence count ($r(1,998) = .36$ [.31, .42], $p < .01$; see Figure 1A) and a strong correlation between the familiarity ratings and the logarithm of the occurrence count ($r(1,998) = .60$ [.55, .64], $p < .01$; see Figure 1B).[7]

For sensible sex ratings, participants' classification of names regarding a name carrier's sex should correspond to some extent to the classification in the name dictionary (Duden, 2007). An independent sample *t*-test of the sex ratings with the names split according to the sex provided by the name dictionary and female coded as −1, male coded as +1 showed a large difference of the mean ratings, $t(1,583) = 96.75$, $d = 4.33$ [4.14, 4.51], $p < .01$ ($M = -0.74$, $SD = 0.46$ for female names and $M = 0.88$, $SD = 0.26$ for male names). This shows that participants rated the names listed as male names in the name dictionary more often as male compared to female and the other way around for female names. Cohen's *d* and the confidence intervals for this and the next analysis were computed using the effsize package (Torchiano, 2018), with Bonferroni corrections on the confidence levels. Degrees of freedom are corrected using the Welch modification, as the variances in both groups may differ. In addition to the sex ratings, we also collected ratings of the confidence that participants had in their sex ratings. We expected some of the names to be more or less ambiguous than others. An independent sample *t*-test of the weighted sex ratings produced similar results, showing a large difference of the mean ratings, $t(1,741) = 94.39$, $d = 4.22$ [4.04, 4.41], $p < .01$ ($M = -4.55$, $SD = 2.73$ for female names and $M = 5.25$, $SD = 1.82$ for male names). This result shows that participants were also more certain with their sex rating if their rating corresponds to the Duden sex classification. In sum, the results demonstrate a large correspondence between our collected ratings and ratings provided from other sources. Nevertheless, a manual inspection of the names, which were most strongly assigned to a different sex compared to the source material, showed that some names were consistently rated as belonging to a different gender. However, this comparison indicated that the difference between the ratings and the source material can mostly be explained by errors in the source material or changes in the usage of the names since the source material was collected.

To conclude, the comparison of the datasets with other sources of the same or similar variables demonstrates a reasonably high validity for ratings of demographic characteristics. This matches the analysis of the internal reliability from the previous section, which also found excellent reliability for all ratings of demographic characteristics. This demonstrates that ratings can be used to manipulate or control sex, nationality, or familiarity of a name in future studies.

Ratings of Warmth and Competence

In addition to the items used by Rudolph et al. (2007), we also included a German version of warmth and competence items, which can be used to predict the perception of a name, most importantly the attitude of a participant toward a carrier of that name, according to the SCM (Asbrock, 2010). To test if the included first names can be also located along the dimensions of warmth and competence as used for the stereotype content model, we first examined the number of meaningful factors that can be extracted from ratings using a principal component analysis (PCA). For this PCA we only used the six warmth and competence ratings (see Table 1 for details). For this analysis, we averaged all ratings for each name and scaled and centered the resulting variables, then we computed a PCA on these averaged ratings to identify the number of factors underlying the ratings of all names. An inspection of the scree plot (see https://osf.io/v5fsy/) showed that two principal components can capture a substantial portion of the variance of the ratings. Since a manual inspection of the scree plot is highly subjective and therefore open to debate, we also confirmed results of two components using a parallel analysis (Horn, 1965)[8] and bootstrapping. Together, these two components were able to account for 90% of the total variance. We thereby confirm the hypothesis of Asbrock (2010) that these items can be organized along two separate dimensions.

To extract two factors from the six PCA components and to confirm that these dimensions indeed correspond to the concepts of warmth and competence, we performed a factor analysis by computing a PCA followed by dropping the four components with lowest variance explanation and a promax rotation of the retained two components (Asbrock, 2010) using the psych R-package (Revelle, 2019). The resulting loadings showed that the variables corresponding to competence loaded strongly and almost exclusively on a single factor with all other variables corresponding to warmth

[7] We transformed the occurrence counts using a logarithmic scale since word occurrences tend to follow a Zipf distribution, which is essentially an exponential distribution in nature, and also their psycholinguistic properties tend to follow an exponential law (e.g., van Heuven, Mandera, Keuleers, & Brysbaert, 2014). Using untransformed occurrence counts, we found somewhat weaker but still reliable correlations ($r(1,998) = .23$ [.17, .28], $p < .01$ for nationality and $r(1,998) = .33$ [.28, .39], $p < .01$ for familiarity; confidence intervals were corrected to achieve simultaneous 95% confidence across all four correlations). Significance tests are done with $\alpha = .01250$ (four simultaneous tests).

[8] To match the (unknown) distribution of the data, we used bootstrapping. To remove the correlations, we sampled all variables independently of each other. To retain the between-subject differences in the random datasets we separately bootstrapped the data for each participant, similar to methods commonly used for bootstrapping multi-level models (Van der Leeden, Busing, & Meijer, 1997). We performed 1,000 bootstraps.

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Table 4. Factor loadings for the stereotype content model variables

Item	Competence	Warmth
Competent	.91	
Competitive	.99	
Independent	.94	
Likable	.21	.83
Warm		.99
Good natured		.98
Variance explained	46%	44%

Note. Factor loadings < .20 ommited.

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loading on the other factor (see Table 4). The only exception was the item "Likable," which was also somewhat correlated with the competence variables, albeit much lower than with the warmth variables. To include the factors competence and warmth from the SCM in the provided dataset, we then averaged the ratings for the three competence items to calculate a total competence score and the three warmth items to calculate a total warmth score. Furthermore, we checked whether averaging of the variables increased the overall low reliability of the SCM variables. The reliability, however, remained low ($r_{2\alpha}$ = .58 [.51, .64] for competence and $r_{2\alpha}$ = .58 [.51, .63] for warmth).

As before, we provide lists of the ten names rated the highest and lowest on these aggregate factors in Table 5. This table shows a similar prevailing gender stereotype as for the ratings of intelligence and attractiveness. Among the ten most highly rated names for competence, the only female name is again the name "Mitsuko." In contrast, the ten names rated highest for warmth are exclusively female, with the name "Giovanni" as the highest rated male name for warmth (M = 5.12, rank 12). This is in line with other research on stereotype content, which frequently finds women to be rated as warmer but less competent compared to men (Asbrock, 2010; Fiske et al., 2002). Both competence as well as warmth correlated significantly with the sex (weighted) ratings.[9] Neither the correlation between weighted sex ratings and competence nor the correlation between weighted sex ratings and warmth was moderated by the gender of the participants (see https://osf.io/b6kfn/).

Exploratory Analyses: Item Inter-Correlations

To identify relationships between the collected variables, we calculated pairwise correlations between all variables in an exploratory analysis. For this, we averaged the ratings for the same name from all participants who rated the name

Table 5. Highest and lowest rated names for the factors (A) Competence and (B) Warmth

Name	*M*	*SD*
(A) Competence		
Chen	5.53	0.83
Jacques	5.27	0.93
Cornelius	5.27	0.73
Erasmus	5.25	0.99
Primus	5.18	0.93
Neil	5.17	0.96
Mitsuko	5.16	1.08
Augustinus	5.15	0.95
Aristoteles	5.15	1.29
Clemens	5.14	1.01
...	...	...
Cindy	3.31	1.06
Mandy	3.29	1.07
Chantal	3.23	0.80
Cinderella	3.18	1.43
Destiny	3.13	1.11
Kevin	3.13	1.17
Cheyenne	3.09	1.08
Candy	3.05	0.90
Fifi	3.05	1.11
Jacqueline	3.03	1.17
(B) Warmth		
Gretchen	5.32	1.16
Bruni	5.23	0.96
Lisbeth	5.22	1.08
Betty	5.20	0.96
Jolanda	5.20	0.68
Maria	5.19	1.04
Rosalinde	5.15	1.16
Lucia	5.13	1.18
Anneli	5.13	0.94
Bea	5.13	1.24
...	...	...
Haider	3.33	1.12
Achmed	3.31	0.87
Zdenek	3.3	0.75
Erdogan	3.24	0.91
Igor	3.21	1.24
Hussein	3.21	1.01
Hassan	3.15	1.11
Arnulf	3.10	1.17
Etzel	3.05	1.24
Adolf	2.77	1.31

Notes. (A) Correlation with sex (weighted): $r(1{,}998)$ = .23 [.15, .30], p < .01; (B) Correlation with sex (weighted): $r(1{,}998)$ = −.37 [−.43, −.29], p < .01.

[9] We tested all possible correlations between the collected ratings (78 comparisons). Therefore, significance was tested at α = .00064.

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Table 6. Correlations between the three topicality categories and all other dimensions

Rating	Modern		Ageless		Old-Fashioned	
	r	95% CI	*r*	95% CI	*r*	95% CI
Sex (weighted)	−.15	[−.23, −.07]*	< .01	[−.08, .08] *ns*	.10	[.02, .18]*
Education	−.27	[−.34, −.19]*	.12	[.04, .20]*	.10	[.02, .18]*
Age	−.74	[−.78, −.70]*	−.56	[−.61, −.50]*	.89	[.86, .91]*
Attractiveness	.42	[.35, .49]*	.53	[.47, .59]*	−.65	[−.70, −.60]*
Intelligence	−.25	[−.32, −.17]*	.12	[.04, .20]*	.09	[.00, .17]*
Religiousness	−.53	[−.58, −.46]*	−.15	[−.22, −.07]*	.46	[.39, .52]*
Competence (SCM)	−.25	[−.33, −.18]*	.19	[.11, .26]*	.04	[−.04, .12] *ns*
Warmth (SCM)	−.04	[−.12, .05] *ns*	.17	[.09, .25]*	−.10	[−.18, −.01]*
Nationality	−.40	[−.47, −.33]*	−.35	[−.42, −.28]*	.52	[.46, .58]*
Familiarity	−.31	[−.38, −.23]*	.16	[.08, .24]*	.10	[.02, .18]*

Note. All df = 1,998. *p < .00064 (equivalent to p < .05 after Bonferroni correction for 78 simultaneous tests).

Table 7. Correlations between all variables with Bonferroni corrected confidence intervals

	Age	Attractiveness	Education	Familiarity	Intelligence	Warmth (SCM)
Attractiveness	−.58 [−.64, −.52]		.32 [.24, .39]		.35 [.27, .42]	.50 [.43, .56]
Competence (SCM)		.34 [.26, .41]	.85 [.83, .88]		.89 [.86, .91]	.37 [.30, .44]
Intelligence		.35 [.27, .42]	.92 [.90, .94]			.43 [.36, .49]
Nationality	.54 [.48, .59]	−.32 [−.39, −.25]		.67 [.62, .72]		
Religiousness	.41 [.34, .48]					
Warmth (SCM)		.50 [.43, .56]	.38 [.31, .45]	.31 [.24, .39]	.43 [.36, .49]	

prior to calculating the correlations. Since the categorical variable "topicality" was coded as three separate variables (1-hot coding), we performed individual correlations for each of the three topicality categories. The aggregated topicality variables measure the proportion of the participants who rated the name in each category. The correlations between the three topicality categories and all other ratings are given in Table 6. The correlations between sex (weighted) and all variables are reported in Tables 3 and 5. Other correlations that were at least of moderate size ($|r| > .3$; J. Cohen, 1988) can be found in Table 7. All correlations in Table 7 are also significant (all $ps \leq .00064$; Bonferroni correction for 78 simultaneous tests). To keep all tests conservative, the degrees of freedom of the test statistics were estimated based on the number of names in the study. The number of names was below the estimated effective sample size for all characteristics (see Table 1). In addition, we put a strong focus on correct positive results, by only providing correlations of at least moderate size in Table 7 instead of providing all statistically significant correlations. In line with Rudolph et al. (2007)[10] we found a significant negative correlation between both the topicality "modern" as well as "ageless" with age ratings, showing that the perceived age of name carriers decreases, the more frequently their names were rated as modern or ageless names. In contrast, for the topicality "old" we found a significant positive correlation, showing that name carriers were rated as older, the more frequently a name is rated as old-fashioned. For attractiveness, the results differed somewhat from the pattern found by Rudolph et al. (2007). The strongest correlations between attractiveness and topicality were found for the topicality "ageless," with somewhat reduced correlations for the topicality "modern," showing name carriers were rated as more attractive the more often their names are rated as ageless or modern. In contrast, we found that the "old" topicality was negatively correlated with attractiveness, such that names were rated as less attractive the more often they were also rated as old-fashioned. For intelligence, we could not confirm the results found by Rudolph et al. (2007). Other than in the previous study, modern names were not rated as more intelligent, but rather as less intelligent, whereas ageless names were rated as more intelligent. Also, intelligence ratings were generally higher the more often a name was rated as old-fashioned, whereas Rudolph et al. (2007) found old-fashioned names to be rated as less intelligent. Similarly, the results for religiousness from Rudolph et al. (2007) could not be replicated. Instead of the modern and ageless names being rated as more religious, we found that names were rated as more religious, the more often they were also rated as

[10] Since Rudolph et al. (2007) used demographic statics to define topicality variables instead of including these variables in their ratings, we can only conceptually replicate the statistical tests. Instead of using an ANOVA, we will perform correlations with each coded topicality.

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old-fashioned. For the relationships between the other ratings also tested by Rudolph et al. (2007) we replicated the negative correlation between age and attractiveness ($r(1,998) = -.58$ [−.64, −.52], $p < .01$) and the positive correlation between attractiveness and intelligence ($r(1,998) = .35$ [.27, .42], $p < .01$). The latter of these can most likely be attributed to some kind of halo effect (Nisbett & Wilson, 1977). However, for our dataset, the correlation between age and intelligence was reversed ($r(1,998) = .18$ [.10, .25], $p < .01$) showing that older name carriers were rated as more intelligent and not as less intelligent. In addition to these results presented for comparison with the results by Rudolph et al. (2007), we also found the correlations between gender and intelligence, attractiveness, warmth, and competence, which we already reported in Descriptive Results and Ratings of Warmth and Competence sections. In addition, names rated as warmer on average were also rated as more attractive, better educated, more intelligent, and more competent. The same was true for competence, which also showed a correlation with attractiveness, education, and intelligence. The correlation between warmth and competence found for this dataset was atypical, as other studies on the stereotype content model found these two scales to be mostly uncorrelated (e.g., Asbrock, 2010; but see also Koch et al., 2016). Finally, the nationality ratings correlated negatively with attractiveness and positively with age, showing that carriers of German names were rated as less attractive and older than those with foreign names.

Comparison of Our Data With Rudolph et al. (2007)

We observed statistically significant correlations that differed in sign in comparison to correlations reported by Rudolph et al. (2007). Differences in the methodology that may explain these discrepancies are discussed below. First, instead of using demographics to determine the topicality, in our study participants rated names in terms of perceived topicality. Therefore, the variables representing the topicalities in our analysis could differ from the ones used by Rudolph et al. (2007). Second, the names we used in our study come from a much larger set, including many less popular and unusal names. For example, our dataset included some modern names that follow short lived trends and are mostly associated with lower social and educational class, such as "Destiny" or "Cheyenne" (see also the lower part of Table 3A) Similarly, we included many less popular old fashioned but highly religious names, such as "Moses" or "Abraham." Since these names were not included by Rudolph et al. (2007), the inclusion in our dataset may have caused the differences.

To test these two possible explanations, we specifically focused on the subset of names also used by Rudolph et al. (2007). Due to our method of selecting the names from the original dataset, only 45 of the 60 names used by Rudolph et al. (2007) were included in our dataset. Since we selected the names for our second study such that all of these 45 names were included in both studies reported here, we combined the data from both our studies for this analysis. Thus, all analyses are based on about 100 ratings per name ($M = 99.73$, $SD = 0.89$). To investigate whether differences between the topicality attributes in our study and the study by Rudolph et al. (2007) may explain the different results, we analyzed how strongly the topicality ratings by our participants differed from the topicality categories that were assigned to names by Rudolph et al. (2007) based on demographic statistics (demographic topicalities). Figure 2A shows the aggregated percentages each topicality category was chosen by our participants split by the demographic topicalities (see Table 2 and Figure 2B for a direct comparison). This comparison shows that the perception of the topicalities does not coincide well with the demographic topicalities. This effect was particular strong for names classified as modern based on demographics, with only around 31% of our participants also rating these names as modern and most participants rating these names as ageless (61%). Similarly, names classified as old-fashioned based on demographics were only rated as old-fashioned by 55% of our participants and rated as ageless by 35%. Thus, either participants have incorrect beliefs about the true demographics of these names, or the definitions of the topicalities used by Rudolph et al. (2007) do not reflect how our participants interpreted these terms. In addition Figure 2A also shows the total percentage each topicality category was chosen by our participants for all 45 names used by Rudolph et al. (2007) and us ("Total") as well as for all 2,000 names used in our study ("Total"). A direct comparison shows, that the name set used by Rudolph et al. (2007) contains a disproportionaly large amount of names our participants perceived as ageless, whereas both old fashioned and modern names were underrepresented. Since topicality was used as a variable in most analyses performed by Rudolph et al. (2007) and us, the differences in the variables may explain the different findings.

To further analyze the differences between our findings and the ones presented by Rudolph et al. (2007), we repeated the analyses restricted to the 45 names contained in both datasets. Since we only calculated the correlations corresponding to the analyses by Rudolph et al. (2007) instead of correlating all variables, we only performed Bonferroni corrections for 15 simulataneous tests (e.g., $\alpha = .003$). In addition, since we found a large difference between the topicality ratings from our participants and the topicalities assigned by Rudolph et al. (2007), we also replicated their analyses on our dataset using an analysis of variance (ANOVA) with the topicality categories derived

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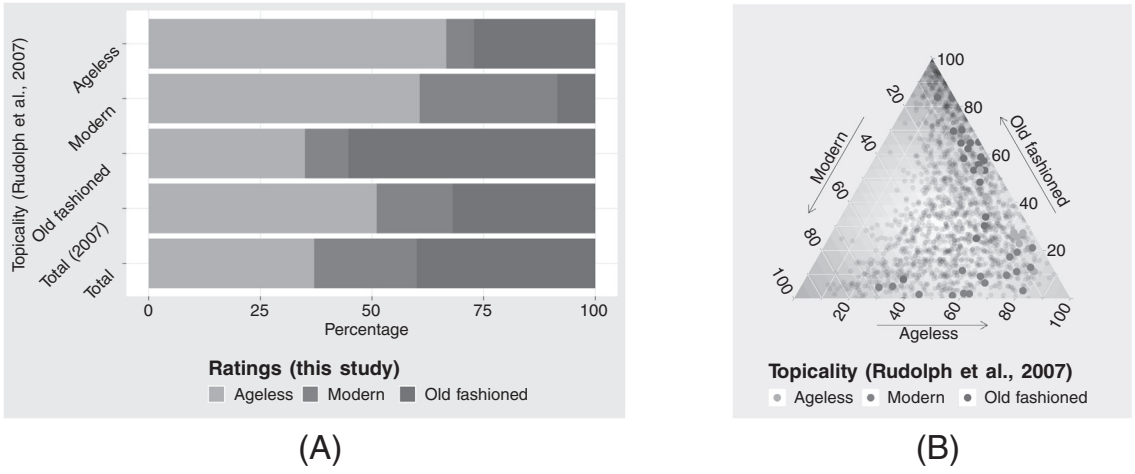

Figure 2. Distribution of the topicality ratings for the names used by Rudolph et al. (2007) aggregated by the topicality assigned in their study (A) and for individual names (B). Axes in (B) indicate the percentage each topicality category was chosen for a name, that is, the sum of all three values for each point is equal to 100% (ternary plot). Gray dots in (B) indicate names not used by Rudolph et al. (2007).

from demographics (demographic topicality) as independent variables. In line with Rudolph et al. (2007), the correlations between the age ratings and the modern and old-fashioned topicality ratings remained statistically significant for the reduced dataset, whereas the ageless topicality ratings were not statistically significant after Bonferroni corrections ($r(43) = -.83$ [−.93, −.62], $p < .01$ for "modern," $r(43) = -.41$ [−.71, .03], $p = .01$ for "ageless," and $r(43) = .90$ [.77, .96], $p < .01$ for "old"). Similarly, the age ratings also differed for demographic topicality, $F(2, 42) = 58.83$, $p < .01$, $\eta^2 = .74$. For the correlations between attractiveness and the topicality ratings on the reduced dataset, we also found the same pattern as before, with names being rated as more attractive the more often they were rated as "ageless" or "modern" and names being rated as less attractive the more often they were rated as "old." Again, the correlation between "ageless" ratings and attractiveness was stronger than between "modern" ratings and attractiveness ($r(43) = .54$ [.14, .79], $p < .01$ for "modern," $r(43) = .66$ [.32, .85], $p < .01$ for "ageless," and $r(43) = -.89$ [−.96, −.74], $p < .01$ for "old"), thus showing the same discrepancy between the findings by Rudolph et al. (2007), and our findings. An ANOVA using the demographic topicality also showed statistically significant differences between the three topicality variables, $F(2, 42) = 51.73$, $p < .01$, $\eta^2 = .71$. Most importantly, using the demographic topicality, we found the same pattern reported by Rudolph et al. (2007) ($M = 4.40$, $SD = 0.26$ for "ageless"; $M = 4.68$, $SD = 0.25$ for "modern"; $M = 3.82$, $SD = 0.26$ for "old"). The correlations between topicality ratings and intelligence ratings for the reduced dataset was neither in line with the correlations on the complete dataset nor the ones reported by Rudolph et al. (2007). Just as on the complete dataset, we found a positive correlation between "ageless" ratings and intelligence ratings, showing that names were rated as more intelligent, the more often they were also rated as ageless ($r(43) = .72$ [.42, .88], $p < .01$). For "old" ratings and intelligence, the direction was now reversed compared to the previous analysis. Thus, on the reduced dataset, names were descriptively rated as less intelligent the more often they were rated as old-fashioned ($r(43) = -.56$ [−.80, −.17], $p < .01$). This was more in line with findings by Rudolph et al. (2007), who also found old names being rated as less intelligent. The correlation between "modern" ratings and intelligence was not statistically significant anymore on the reduced dataset ($r(43) = .01$ [−.42, .44], $p = .94$). Again, an ANOVA with the demographic topicality also showed statistically significant differences between the three categories, $F(2, 42) = 20.12$, $p < .01$, $\eta^2 = .49$. Most importantly, using the demographic topicality we found the same pattern as Rudolph et al. (2007), with the old-fashioned names being rated as least intelligent ($M = 4.61$, $SD = 0.26$ for "ageless"; $M = 4.67$, $SD = 0.21$ for "modern"; $M = 4.22$, $SD = 0.22$ for "old"). For religiousness, we neither could replicate the pattern on the complete dataset nor the findings by Rudolph et al. (2007). Of the original correlations, only the one between modern topicality ratings and religiousness remained significance on the reduced dataset ($r(43) = -.45$ [−.74, −.02], $p < .01$ for "modern"; $r(43) = .31$ [−.13, .66], $p = .04$ for "ageless"; and $r(43) = .08$ [−.36, .49], $p = .61$ for "old"). An ANOVA with the demographic topicality also showed no statistically significant differences in religiousness ratings, $F(2, 42) = 2.41$, $p = .10$, $\eta^2 = .10$. Furthermore, the negative correlation between age and attractiveness remained on the reduced dataset ($r(43) = -.84$ [−.94, −.65], $p < .01$). The same was true for the positive correlation between attractiveness and intelligence ($r(43) = .74$ [.45, .89], $p < .01$). More importantly, on the reduced dataset the relationship between age and intelligence differed from the complete

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dataset, thus older names were rated as less intelligent ($r(43) = -.45$ [$-.74$, $-.03$], $p < .01$) in line with the findings by Rudolph et al. (2007).

In conclusion, this more direct comparison shows that both the larger set of names, which also included more uncommon names, as well as the different methodological approach to determine topicality caused the differences between our results and the ones reported by Rudolph et al. (2007). When we reduced the dataset to the names also used by Rudolph et al. (2007) the differences partially disappeared. Most importantly, the correlation between age and intelligence switched signs and was now in line with previous findings, although it was not statistically significant anymore. For the topicality ratings, the discrepancies also partially disappeared. In addition, when we switched from topicality ratings to demographic topicality, the pattern was much more in line with previous findings. The differences in our findings when using ratings versus when using demographics in combination with the initial comparison between these two sources supports our initial notions that demographics may sometimes differ strongly from participants' beliefs about these demographics.

Guidelines for Using the Provided Dataset

In this section, we provide guidelines on how to select names from our dataset, methodological pitfalls that may arise, and how to circumvent those. We also describe an R-package that may assist researchers in the process.

Choosing Similar Names

In a study on sex stereotypes in job interviews, a researcher might want present information on a job candidate who is either male or female and either competent or warm in an experimental design. Using our dataset, what is the most efficient method to select male or female names that differ most on the independent variables "competence" and "warmth" and that match on the many other variables that may relate to the dependent variable (e.g., perceived intelligence)? High dimensionality datasets often suffer from an effect referred to as the "curse of dimensionality" (Aggarwal, Hinneburg, & Keim, 2001; Beyer, Goldstein, Ramakrishnan, & Shaft, 1999). Without going into much detail, this term refers to a number of unexpected properties of high dimensionality spaces. Most importantly for the research presented here, in such a dataset the most similar (best match) and most dissimilar (worst match) to any given query (e.g., another name in the dataset) show only minor differences in terms of their similarity. Hence, in "such a case, the nearest neighbor problem becomes ill defined, since the contrast between the distances to different data points does not exist. In such cases, even the concept of proximity may not be meaningful from a qualitative perspective" (Aggarwal et al., 2001, p. 421). Thus, the high dimensional nature of the dataset makes a search for similar names to any name ill defined. However, the curse of dimensionality can be avoided in case the variables show high correlations and the underlying dimensionality of the dataset is much lower (Beyer et al., 1999). In this case, the matching should be performed on a dataset of lower dimensionality, which approximates the original dataset. We constructed and tested such a dataset (details and quality metrics are given in https://osf.io/hcx2v/), which reduces the dimensionality to five dimension. The lower dimensionality variables are given as PC1 to PC5 in the dataset. Researchers who need to calculate the similarity of one or more names to each other are strongly advised to use these variables instead of the original variables.

R-Package for Name Selection

To give researchers a simple method for selecting names for their studies, we provide an open source R-package that allows to define criteria for the selection of names. The package can be downloaded at https://github.com/aggloeckner/GerNameR. This section shortly sketches the main features of the package, interested readers should refer to the documentation included with the package for detailed examples. This package can either directly extract subsets of names based on the percentiles, for example, the 10% most familiar names, or the names which are, for example, both above the median in competence and intelligence. In addition, this package allows creating matched pairs of names from two different groups (e.g., male and female) based on their difference in ratings. The matching is based on the reduced dimensionality variables, but can also be tailored to include other ratings, to ensure that the names are both generally similar but more similar on a given dimension such as competence or warmth. To include any other characteristic, the weight with which this characteristic should be used can be set by the researcher. To match the names, the distance between all pairs is calculated with the given weighting, and then the names are paired such that the total distance between all pairs is minimized. The minimal weighted matching is identified using the Hungarian algorithm for bipartite matching (Hornik, 2018; see also Munkres, 1957).

In addition to creating a set of pairs of matching names, we also allow extracting of a set of names, with the same number of members from two groups. Again this set is created such that the overall difference between all names (not only between the two groups) is minimized, with the

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additional possibility to give more weight to some characteristics if required for the experimental design. To find such minimal distance sets, a genetic algorithm is used with the distance used as the fitness function (Scrucca, 2019; see also Holland, 1975).

Using the Collected Variables as Control Variables

Variables may be used as control variables, for example, in a regression model to account for differences on dimensions for single names in a study. Including many or all variables that we report in this article may result in a failure of fitting regression weights due to high multicollinearity up to the point of exact multicollinearity if all variables are used. This multicollinearity reflects the fact, that the variables contain less information than one would expect given the number of variables (Goldberger, 1991). This again indicates that the actual number of meaningful dimensions we collected is much lower than the number of originally collected variables. The solution to problems of multicollinearity is therefore exactly the same as before, instead of using the original variables, researchers are advised to use the variables labeled PC1 to PC5 as control variables in any regression analysis.

Conclusion

We provide ratings on perceived demographic and social characteristics (e.g., sex, origin, familiarity, education, and intelligence) for a large set of 2,000 representative German names. The split-half reliability indicates that the reliability of these ratings ranges from very high values for more objective characteristics (sex, origin, familiarity, and age) to lower values for more subjective ratings such as warmth and competence. In addition, the correlation with similar ratings provided by other sources for sex and origin show that these ratings relate to external criteria in a meaningful way. Furthermore, a factor analysis on a subset of the ratings taken from a questionnaire about warmth and competence could show that these ratings collected for German names have a similar factor structure as the one that was found in previous studies using the same items for ratings of social groups (e.g., Asbrock, 2010; Fiske et al., 2007).

Considering the high number of names tested and the time-constraints of an online study, the number of items per name was limited. In the study we therefore focused on measures that we think are useful for research on stereotypes (Fiske et al., 2007). To give some more insight on which kind of association people typically have when presented with a name, we nonetheless included an open ended question. The answers to this question can further inform researchers to plan which items to include in a possible follow-up study.

Due to collecting a large number of different names we were only able to collect relatively few ratings for most of the names. This may lead to the estimated means differing substantially from the population means for these names. In addition, low sample sizes are associated with inflated effect sizes (Gelman & Carlin, 2014) and false-positive results of hypothesis tests (Schönbrodt & Perugini, 2013). However, concerning the tests performed in this study, the small number of ratings per name is less problematic, because most of these tests were done on averaged ratings for each name. Thus, the degrees of freedom for these tests should be based on the number of names, not on the number of ratings per name. In addition, since averaging serves to remove noise, each value entered in the analysis carries less error than a single rating, thus leading to even higher true degrees of freedom. In fact, Table 1 shows that the effective sample sizes of all variables are much higher than the number of names. Therefore, by estimating the *df*s for our tests based on the number of names, we could achieve conservative testing.

To our knowledge, the provided set of ratings is the most extensive to date. Therefore, this set of names may not only be used for studies where only a few names are given (e.g., Bertrand & Mullainathan, 2004; Moss-Racusin et al., 2012; Steinpreis et al., 1999), but also in studies that require a large number of trials with different names (Dorrough et al., 2017; Stevens et al., 2011). The representative total set of names furthermore allows generating representative subsets by random sampling with or without constraints (e.g., only names that are similar with respect to some dimensions).

Furthermore, since associated characteristics with names are subject to change (e.g., due to celebrities or other prominent figures carrying those names), it is important to repeatedly re-validate ratings of first names (Newman et al., 2018). By providing the complete source code to our survey software as well as all analysis scripts, we hope to provide an easy starting point for other researchers who are interested in replicating or extending our results.

Since we only collected ratings for German names by German native speakers, the collected dataset is specific to Germany and so its use for studies in other countries may be limited. Nevertheless, the methods and the software we use and provide as part of this research is created such that it may easily be adapted to other countries or languages as well.

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History
Received June 27, 2018
Revision received January 23, 2019
Accepted January 23, 2019
Published online August 20, 2019

Authorship
All authors were involved in all parts of the research. Tillmann Nett, Angela Dorrough, and Marc Jekel designed the study and collected the data. Tillmann Nett performed the data analysis and implementation of the R-scripts.

ORCID
Tillmann Nett
https://orcid.org/0000-0001-7816-8944

Open Data
All materials used in the survey, Tests for gender differences in stereotype effects, a scree plot for 3 warmth and 3 competence items, as well as a construction of a lower dimensionality approximation of the original variables can be found in https://osf.io/jepzp/.

Tillmann Nett
FernUniversität in Hagen
Office C011
Universitätsstr. 33
58084 Hagen
Germany
tillmann.nett@fernuni-hagen.de

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Original Article

Feeling Bad and Doing Good

Forgivability Through the Lens of Uninvolved Third Parties

Shoko Watanabe and Sean M. Laurent

Department of Psychology, University of Illinois at Urbana-Champaign, IL, USA

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Abstract: Previous forgiveness research has mostly focused on victims' forgiveness of transgressors, and offenders' post-transgression efforts intended to promote victim forgiveness have been collectively branded as apology. However, decisions concerning forgiveness frequently occur outside of dyadic contexts, and the unique roles of repentance and atonement in determining forgivability of offenders, despite their preeminence in theology and law, have received little empirical attention. Across five experiments (N = 938), we show that repentance and atonement independently influence third-party perception of forgivability for a variety of harms, even in disinterested contexts. Our findings provide a systematic examination of decisions about forgivability disentangled from direct personal involvement, demonstrating that components of apology known to facilitate forgiveness in victims also increase perceived forgivability from unharmed observers.

Keywords: third-party, forgivability, repentance, atonement, moral judgment, cooperation

Forgiveness among relatives and friends is commonly depicted in classical and modern literature (e.g., *King Lear*, *The Brothers Karamazov*). Religious scholars and contemporary psychologists have also widely investigated forgiveness. For victims and transgressors, forgiveness helps repair damaged relationships, but people also evaluate whether strangers who have harmed other strangers deserve forgiveness. This question of *perceived forgivability* permeates distance and time. Upon hearing about school shootings, hate crimes, or international conflicts, perceivers outside of harmed communities ponder, even generations later, whether offenders deserve forgiveness. *One Love Manchester*, for example, attracted worldwide support – reminding us that even when revenge and punishment seem adaptive, third parties desire healing and forgiveness.

Forgiveness has been described as the process by which negative reactions toward offenders (i.e., avoidance and revenge) are transformed into prosocial motivations (McCullough, Bellah, Kilpatrick, & Johnson, 2001). Supporting this, empirical evidence has demonstrated when and why victims forgive transgressors, including physiological and social benefits of forgiveness (e.g., Harris & Thoresen, 2005; Lawler et al., 2003; Witvliet, Ludwig, & Vander Laan, 2001). However, disinterested third-party observers also make moral judgments about interpersonal transgressions despite having no personal connections to victims or offenders. For example, people experience negative emotions (e.g., moral outrage) even when they are not directly or indirectly victimized (Montada & Schneider, 1989; Skarlicki, Ellard, & Kelln, 1998). Third parties even boycott or protest against offenders in response to mistreatment of others (e.g., #MeToo). Despite detachment from immediate harm, strong negative reactions may have unhealthy consequences for third parties' well-being, just as ruminating or grudge-holding deleteriously affects victims (Witvliet et al., 2001). Given how transgressions have impacts beyond victim-transgressor dyads, understanding how third parties evaluate whether offenders should be forgiven (rather than punished) is an understudied topic of research.

Apology, remorse, and restitution are post-transgression factors that facilitate forgiveness (e.g., McCullough et al., 1998; Zechmeister, Garcia, Romero, & Vas, 2004) and can have positive effects on victims. In victim-transgressor dyads, decisions to forgive hinge on future exploitation risk and offenders' relationship value. Offenders who successfully display relational commitment (e.g., conciliatory behavior) and reduce the perceived likelihood of future threat (e.g., sincere apology) attain forgiveness (McCullough, Kurzban, & Tabak, 2013). Yet, little is known about whether these gestures – which provide no direct benefit for third parties – also influence uninvolved observers' forgivability judgments.[1] When relational value and future harm are not at stake, can post-transgression offender efforts such as repentance and atonement still restore their damaged reputations? Using a person-perception approach, we examine whether offenders' post-transgression attitudes (e.g., repentance) and actions (e.g., atonement) impact uninvolved third parties' perceptions of forgivability – the

[1] We acknowledge that transgressions may affect third parties symbolically (Okimoto & Wenzel, 2008). Thus, the term "uninvolved third party" references observers not personally known to victims or transgressors.

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https://doi.org/10.1027/1864-9335/a000390

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extent to which third parties believe forgiveness is deserved.

Third-Party "Forgiveness" Versus "Forgivability"

Most past interpersonal forgiveness research has concentrated on victim-transgressor dyads (e.g., Boon & Sulsky, 1997; McCullough et al., 1998; McCullough, Worthington, & Rachal, 1997). One exception is third-party forgiveness research where offenders seek forgiveness from victims' family members or communities. Learning that someone close has been harmed can be painful; indeed, friends and relatives of victims are less forgiving than victims, despite not being directly harmed (Green, Burnette, & Davis, 2008). Yet, judgments regarding deservingness of forgiveness are sometimes made by people unconnected to transgressions. For example, people evaluate the behavior of athletes (e.g., Lance Armstrong), actors (e.g., Kevin Spacey), politicians (e.g., Bill Clinton), and criminals (e.g., mass-shooters) and decide whether these people deserve forgiveness for their (alleged) misdeeds.

Unlike victims and their close others, unharmed parties arguably lack "standing" to grant forgiveness. Nonetheless, third parties' decisions regarding forgivability can have real-world consequences for transgressors (e.g., loss of sponsorships and television deals, impeachment, and death sentences vs. life imprisonment). We refer to this impersonal judgment as *forgivability* – the extent to which an offender deserves forgiveness – to distinguish it from *forgiveness*, which denotes a personal decision to forgive an offender. This distinction also applies to victims, who may choose to forgive despite believing that offenders are undeserving of their forgiveness. However, deserved forgiveness (i.e., when transgressors apologize or make amends) is understandably more beneficial for victims than undeserved forgiveness (Strelan, McKee, & Feather, 2016).

When transgressions occur, third parties likely evaluate whether or not offenders *should be* forgiven or punished. Whereas personally unharmed third parties can and do punish offenders even in anonymous interactions (Fehr & Fischbacher, 2004; Henrich, Ensminger, et al., 2010), actually *forgiving* offenders may not be a relevant concept for unharmed third parties. Relatedly, offenders might be instrumentally punished for deterrence yet be seen as deserving forgiveness, or retributively punished while remaining unforgiven. Despite conceptual differences, studies of perceived *forgivability* have been surprisingly neglected in the field, and gaining insight into how uninvolved third parties decide whether forgiveness is deserved is informative beyond what we know about punishment and forgiveness from victims.

Unlike victims and their close others, post-transgression apologies or compensation provide no apparent benefit to unharmed observers. Although forgiveness depends on desire for reconciliation, costs of retaliation, and avoiding further harm for involved parties (McCullough et al., 2013), uninvolved parties should be less concerned with these issues. On what basis, then, will third-party observers decide that offenders deserve forgiveness? Apart from work on public confession (Cerulo & Ruane, 2014; Gold & Weiner, 2000; Weiner, Graham, Peter, & Zmuidinas, 1991) and victim-observer asymmetries in discriminating apology sincerity (Hashimoto & Karasawa, 2012, 2016; Risen & Gilovich, 2007), no studies to our knowledge have tackled this subject.

Exploring third-party perceptions of forgivability allows a clear view of how people think about forgiveness when no reconciliation concerns exist (Fincham, Paleari, & Regalia, 2002; Finkel, Rusbult, Kumashiro, & Hannon, 2002). Without personal revenge motivation, unharmed observers may perceive offenders who display remorse and/or offer restitution to victims as worthy future cooperation partners who deserve rehabilitation instead of punishment (Petersen, Sell, Tooby, & Cosmides, 2012). That is, if forgiveness is a cognitive adaptation for maintaining existing cooperative relationships between involved parties (McCullough et al., 2013), then recognizing repentance and atonement from offenders should be advantageous not only for harmed parties but also for observers seeking to build cooperation. Pointing to third-parties' sensitivity to post-transgression offender efforts, Gromet and Okimoto (2014) found that organizational peers preferred to work with forgiving victims (i.e., who accepted offender amends) more than unforgiving victims. Considering that repentance and atonement directly benefit involved parties in achieving reconciliation and that deserved forgiveness results in improved well-being of victims (Strelan et al., 2016), we argue that the same factors help transform third parties' negative perceptions of offenders into positive beliefs that they should be forgiven.

Repentance and Atonement

Repentance and atonement often co-occur with apology, a topic that has been studied alongside forgiveness (e.g., Carlisle et al., 2012; Darby & Schlenker, 1982). Positive effects of apology on forgiveness are found for past transgressions (e.g., Davis & Gold, 2011; McCullough et al., 1997), experiments with hypothetical transgressions (e.g., Ohtsubo & Watanabe, 2009; Weiner et al., 1991), staged offenses (e.g., Ohbuchi, Kameda, & Agarie, 1989), and economic games (e.g., Fischbacher & Utikal, 2013; Ho, 2012). Despite links between apology and forgiveness, one limitation is

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that conceptualizations of apology have varied considerably across studies (see Lewicki, Polin, & Lount, 2016 for a review). Thus, rather than introducing another definition, we focus directly on repentance and atonement – two components of apology that reflect offenders' post-transgression mental states and observable behavior.

Although these variables have conceptual overlap, crucial differences exist between feeling bad about one's actions (i.e., repentance) and efforts to make amends (i.e., atonement). Repentance is operationalized here as negative emotions like regret, guilt, and remorse that are associated with offender acknowledgment of responsibility for a transgression (Eaton, Struthers, & Santelli, 2006; Schlenker & Darby, 1981). On the other hand, we operationalize atonement as offenders' concrete actions directed toward improving victims' well-being, encompassing behavioral *attempts* to "make things right" and *consequences* of such efforts that result in restitution/compensation. In sum, we use the terms repentance and atonement to represent divergent forms of post-transgression offender efforts that have been uniformly referred to as "apology" in past work. By treating these components of apology as distinct, we examine the unique contributions of each in increasing perceived forgivability.

Evidence indicates that repentance leads to forgiveness by validating victims and dissipating self-threat arising from devaluation caused by a transgression (Eaton et al., 2006; Scobie & Scobie, 1998). As such, conveying the lack of intention to impose further harm through repentance may function as an impression management strategy (Darby & Schlenker, 1982; Ohbuchi et al., 1989). Alternatively, repentance may have diminished value for third parties because they do not directly experience threat. Thus, we hypothesize that repentance will have a weak yet positive effect on forgivability.

Atonement also has positive effects on forgiveness (e.g., Carlisle et al., 2012; Drell & Jaswal, 2016; Jeter & Brannon, 2017). Offenders' post-transgression behavior to make amends typically results in favorable outcomes for victims. However, victims value costly apologies even absent material compensation (Ohtsubo & Watanabe, 2009). Evidence from organizational, ethnographic, and animal behavior research also suggests that substantive penance or conciliatory gestures, even when they do not fully compensate, can rebuild cooperation (Boehm, 1987; Bottom, Gibson, Daniels, & Murnighan, 2002; de Waal, 1989). Although atonement provides no material or emotional benefit for uninvolved observers, it signifies offenders' commitment to the well-being of others (McCullough et al., 2013) and symbolically redresses the values violated by the offense (Okimoto & Wenzel, 2008). Therefore, we hypothesize that atonement will have a strong positive effect on forgivability.

The Current Research

Five experiments, using a variety of harms and relationships between victims and transgressors, tested the hypothesis that repentance and atonement independently increase forgivability. Experiment 1 examined whether communicating repentance would increase forgivability. Experiment 2 investigated the effects of atoning behavior on forgivability. Experiment 3's transgression featured a physical harm and manipulated both repentance and atonement. Experiment 4 used a repeated-measures design allowing us to track how forgivability unfolded across an event and tested whether costliness of restitution mediated the effect of atonement on forgivability. Experiment 5 compared how victims, involved others, and uninvolved third parties perceive forgivability as a function of repentance and atonement.

General Method: Participants

We report how we determined sample size, all data exclusions, manipulations, and measures used. Study 1's sample size was determined based on a pilot study reported in the Electronic Supplementary Material (ESM 1). In Studies 2–5, sample sizes were based on the criterion of having 80% power (α = .05) to detect medium-sized effects (d = 0.50). Participants were excluded from analyses for unusually short reading times or incorrectly responding to attention check items.[2] Final sample sizes were n = 191 (Experiment 1), n = 111 (Experiment 2), n = 141 (Experiment 3), n = 158 (Experiment 4), and n = 337 (Experiment 5). All experiments were between-participants with random assignment to conditions. Research was approved by the Institutional Review Board where data were collected. All participants provided informed consent prior to participation and demographic information after responding to primary measures. Participants were U.S. residents recruited from Amazon Mechanical Turk with above 97% HIT approval ratings. Table 1 lists demographic information for all studies. Additional demographics are reported in the ESM 1 (Table S1).

[2] In Experiment 4, two participants with incomplete responses and 13 participants who had participated in a pilot study were excluded. For all studies, analyses retaining all participants did not differ substantively from those reported, except the effect of repentance on T2 forgivability in Experiment 4 did not reach significance, and its effect on recovery was marginally significant (see Table S7 in ESM 1).

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Table 1. Demographics (Experiments 1–5)

	Experiment 1	Experiment 2	Experiment 3	Experiment 4	Experiment 5
Final sample size	191	111	141	158	337
Exclusion					
Short reading time	11	3	0	6	4
Attention check miss	17	9	4	7	19
Gender (% female)	46.6%	48.6%	57.4%	43.0%	43.6%
Age (*M* and *SD*)	35.97 (12.05)	38.86 (12.92)	38.28 (13.35)	36.41 (10.45)	37.33 (11.14)
Ethnicity					
Asian American	12.3%	7.2%	6.4%	8.9%	6.5%
African American	8.0%	5.4%	4.3%	4.4%	9.2%
Hispanic/Latino(a)	4.8%	7.2%	10.6%	5.1%	6.2%
European American	70.1%	78.4%	75.2%	79.1%	75.1%
Other	4.8%	1.8%	3.5%	2.5%	3.0%

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Experiment 1

Experiment 1 tested whether expressing remorse to a victim (i.e., repentance) versus not doing so would influence forgivability. Although remorse and apology naturally co-occur, to isolate the effects of repentance from verbal apology, Experiment 1 tested whether an offender's communication of remorse can facilitate forgivability without an explicit statement of "I'm sorry." We hypothesized that forgivability would be higher for a repentant offender than a non-repentant offender.

Method

Procedure

All vignettes are available in the ESM 1. Participants read about a senior in college who was failing a required course and submitted an extra-credit assignment that was then lost by a teaching assistant (TA). Participants read about a senior in college who was failing a required course and submitted an extra-credit assignment that was then lost by a teaching assistant (TA). Participants then read one of two email responses from the TA. In the No-Repent condition, the TA inadvertently left the student's assignment in the copy room. In the Repent condition, the TA additionally acknowledged that it could affect the student's grade and articulated remorse, writing, "I feel very bad about it." Participants then responded to dependent measures. Unless noted, all items in all experiments used 7-point scales ranging from 1 = *entirely disagree* to 7 = *entirely agree*.

Measures

Agreement with four statements that the TA "was repentant," "felt guilty," "felt bad," and "regretted what happened" assessed perceived remorse (α = .94), which served as manipulation check. Three items (α = .86) adapted from existing forgiveness measures (McCullough & Hoyt, 2002; Rye et al., 2001) measured forgivability: "Jamie (the student) should forgive the TA," "Despite what the TA did, Jamie should have compassion for him," and "Jamie should let go of any anger she may feel toward the TA." To explore whether participants inferred verbal apology or atonement from the repentance manipulation, we asked two binary-response questions: "Did the TA…" "apologize to Jamie for losing her assignment?" and "make amends to atone for losing Jamie's assignment?" One item assessed transgression severity: "How severe was the impact of what the TA did?" (1 = *not at all severe* to 7 = *very severe*).

Results and Discussion

R codes and data for all studies are available at https://osf.io/6jqky/. As expected, remorse was higher in the Repent condition (M = 4.86, SD = 1.32) than the No-Repent condition (M = 3.63, SD = 1.52), $t(189) = 5.94$, $p < .001$, $d = 0.86$, demonstrating that the repentance manipulation was successful. As hypothesized, forgivability was higher in the Repent condition (M = 4.33, SD = 1.34) than in the No-Repent condition (M = 3.82, SD = 1.53), $t(189) = 2.43$, $p = .016$, $CI_{.95} = [0.10, 0.92]$,[3] $d = 0.35$, showing that repentance promoted a belief among observers that a transgressor deserved forgiveness. Offense severity did not differ significantly across conditions, (M_{repent} = 6.21, SD_{repent} = 0.88; $M_{no\text{-}repent}$ = 6.06, $SD_{no\text{-}repent}$ = 1.24), $t(179) = 0.89$, $p = .376$, ruling out the possibility that the observed difference in forgivability was due to condition-based differences in perceived severity of the offense.[4] When asked whether

[3] CIs represent lower and upper bounds of the difference between means.

[4] Due to a technical error, *df* for offense severity was 179 because responses from 10 participants to the transgression severity item were not recorded.

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the TA apologized, significantly more participants in the Repent condition (74.4%) relative to the No-Repent condition (29.9%) responded "yes," $\chi^2(1) = 35.32$, $p < .001$. However, the proportions of participants indicating that the TA atoned (Repent: 13.3%; No-Repent: 14.4%) were similar, $\chi^2(1) = 0.00$, $p = .996$. Experiment 1 showed that repentance promotes forgivability. Additionally, although the TA's email did not contain explicit verbal apology, people inferred apology (but not atonement) from expression of repentance, suggesting that uninvolved third parties differentiate repentance from atonement.

Experiment 2

Experiment 2 explored the role of post-transgression behavior directed toward alleviating the consequences of an offense (i.e., atonement) on forgivability. We believe that concrete actions *aimed at* repair are what primarily influence forgivability rather than outcome differences that naturally result from these actions. Experiment 2 tested this idea by manipulating an agent's attempt to atone while holding constant the negative outcome resulting from the transgression. We hypothesized that participants would view an offender who tries but fails to atone as more deserving of forgiveness than an offender who does not attempt to make amends.

Method

Procedure

Participants read about an employee who did not get promoted because her supervisor failed to submit a promised recommendation. Two versions of the story's ending were used. Atone: The supervisor explained her oversight to the hiring manager and asked that the employee's application be reconsidered. After review, the employee did not get the promotion. No-Atone: Despite conversing with the hiring manager, the supervisor did not seek to fix the situation. The employee did not get the promotion. No mention of repentance or verbal apology was made, and no description of the supervisor's feelings about the transgression was given. Participants then responded to dependent measures.

Measures

A manipulation check, attempted restitution ($\alpha = .98$) was measured with four items: "Kayce (the supervisor)…" "tried to atone for not submitting Maya's (the employee's) letter on time," "tried to 'make things right' after failing to send the hiring committee her letter," "attempted to correct her mistake of not sending the letter for Maya," and "wanted to fix the problem her oversight had caused." Forgivability ($\alpha = .87$) was measured using the same three items from Experiment 1 with names changed to match the new vignette.

Results and Discussion

Attempted restitution was higher in the Atone condition ($M = 6.27$, $SD = 0.93$) than the No-Atone condition ($M = 3.77$, $SD = 1.77$), $t(109) = 9.34$, $p < .001$, $d = 1.77$. Forgivability was also higher in the Atone condition ($M = 5.63$, $SD = 0.96$) than the No-Atone condition ($M = 4.35$, $SD = 1.38$), $t(109) = 5.71$, $p < .001$, $CI_{.95} = [0.84, 1.73]$, $d = 1.08$. The large effect of atonement on forgivability suggests a robust connection between trying to "make things right" and deservingness of forgiveness. Consistent with the idea of displaying cooperation commitment, this suggests that even failed attempts at atonement make transgressors seem more forgivable to unharmed observers. Though attempts at repair may typically result in positive outcomes, the outcome here was unfavorable in both conditions. Thus, Experiment 2 showed that *attempts* to atone are sufficient for influencing forgivability.

In Experiments 1 and 2, both offenders had power over victims. People may be compelled to forgive offenders if the cost of not forgiving is amplified by power status differences (Aquino, Tripp, & Bies, 2001). Forgivability may have been influenced by the consideration that not forgiving could further disadvantage the victim. In subsequent studies, the transgressor and victim have equal status.

Experiment 3

In Experiment 3, repentance and atonement were jointly manipulated. To focus solely on the offender's internal response, their repentant thoughts were revealed to participants but not communicated to the victim. We hypothesized that repentance and atonement would both increase forgivability but that the effect size for atonement, which indicates offenders' behavioral commitment to cooperation, would be descriptively larger. We had no prediction regarding whether the manipulations would work synergistically or exert additive effects.

Method

Procedure

Participants read a two-part vignette. Part 1: A college student (Jesse), while riding his bike, was hit by a car driven

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by a classmate (Chris). After Jesse claimed to be unhurt, Chris drove away. Later, Jesse realized he was seriously injured and received emergency surgery. The next day, Chris learned about Jesse's injury. The repentance manipulation was embedded in the narrative:

Repent

Hearing this, Chris felt terrible about himself. He thought to himself, "Poor Jesse. It was my fault this happened, wasn't it? [. . .] Jesse would be here right now if I was driving more carefully."

No-Repent

Hearing this, Chris didn't feel particularly bad. He thought to himself, "I don't know why he's blaming me for what happened [. . .] and it's not my fault I couldn't stop in time."

Part 2: Chris encountered Jesse at a mall a few months after the accident. In the Atone condition, Chris bought a new bike for Jesse by denying himself the purchase of a wanted item. In the No-Atone condition, Chris bought his desired item and Jesse bought the bike himself. Perceived remorse was measured between Part 1 and Part 2. Remaining measures were collected after Part 2.

Measures

The four remorse items from Experiment 1 (α = .97) were used to check the repentance manipulation. Four restitution items (α = .97) assessed the atonement manipulation: "Chris. . ." "atoned for the damage he caused Jesse," "tried to make amends to Jesse," "repaired the harm he had caused Jesse," and "made up for his earlier actions." Forgivability (α = .85) was measured with three items: "Jesse should forgive Chris for what happened," "Jesse should let go of any anger he may feel toward Chris," and "Chris deserves to be forgiven for what he did." Several related constructs were measured in Experiments 3 and 4; associated analyses are reported in the ESM 1 (Tables S11–S14).

Results and Discussion

Manipulation Check

Because atonement was manipulated in Part 2 after the measure of remorse was collected (and thus, atonement could not influence remorse), a *t*-test was used to examine differences in remorse as a function of repentance. Predictably, remorse was higher (M = 6.18, SD = 0.74) in the Repent condition than in the No-Repent condition (M = 2.16, SD = 1.25), $t(139) = 23.22$, $p < .001$, $d = 3.91$. For all other measures, 2 (No-Repent/Repent) × 2 (No-Atone/Atone) ANOVAs with 1, 137 *df* were used. Perceived restitution was higher in the Atone condition (M = 5.70, SD = 1.00) than the No-Atone condition (M = 1.67, SD = 0.99), $F = 572.05$, $p < .001$, $d = 4.05$. No main effect of repentance (p = .650) or interaction of atonement and repentance (p = .198) was found on restitution.

Forgivability

Forgivability was higher in the Repent condition (M = 4.83, SD = 1.33) than the No-Repent condition (M = 4.39, SD = 1.63), F = 10.26, p = .002, $CI_{.95}$ = [0.05, 0.83], d = 0.30. Similarly, forgivability was higher in the Atone condition (M = 5.48, SD = 1.03) than the No-Atone condition (M = 3.72, SD = 1.39), F = 83.75, $p < .001$, $CI_{.95}$ = [1.42, 2.20], d = 1.44. The interaction was not significant (p = .098).

Experiment 3 confirmed the findings of Experiments 1 and 2, further showing that repentance and atonement independently influence forgivability. Corroborating the conclusion that atonement effects are not driven solely by outcome (Experiment 2), Experiment 3 demonstrated that atonement increases forgivability even without fully restoring the victim to a pre-transgression state. Notably, repentance influenced forgivability even though the offender's remorse was not communicated to the victim, highlighting uninvolved third parties' sensitivity to offenders' mental states. Suggesting that atonement might exert a greater influence on forgivability than repentance, the effect size for atonement was 480% larger than the effect size for repentance. Finally, these effects were demonstrated in a new context where harm was physical, fairly severe, and described people similar in power status.

Experiment 4

Experiment 4 used a repeated-measures design that allowed us to track how forgiveness changes as a function of repentance and atonement and to conceptually replicate the results of Experiment 3 using a new workplace transgression. To assess how forgivability unfolds across an event and is increased by repentance and/or atonement, a vignette was presented in three parts. The negative event was first described (Part 1), followed by a description of the transgression (Part 2), followed by manipulations of repentance and atonement (Part 3). This design (Figure 1) allowed us to measure forgivability post-transgression/pre-manipulations at Time 1 (T1) and post-manipulations at Time 2 (T2), to assess "repair" in perceived forgivability as a function of repentance and atonement.

Beyond predicted main effects of repentance and atonement on forgivability at T2, we hypothesized that forgivability would be higher at T2 than at T1 (i.e., positive difference score for T2 − T1), indicating recovery from baseline forgivability as a function of repentance and atonement. Because the cost of making amends should be relevant for forgiveness (e.g., Ohtsubo & Watanabe, 2009), we also measured perceived costliness as a possible mediator

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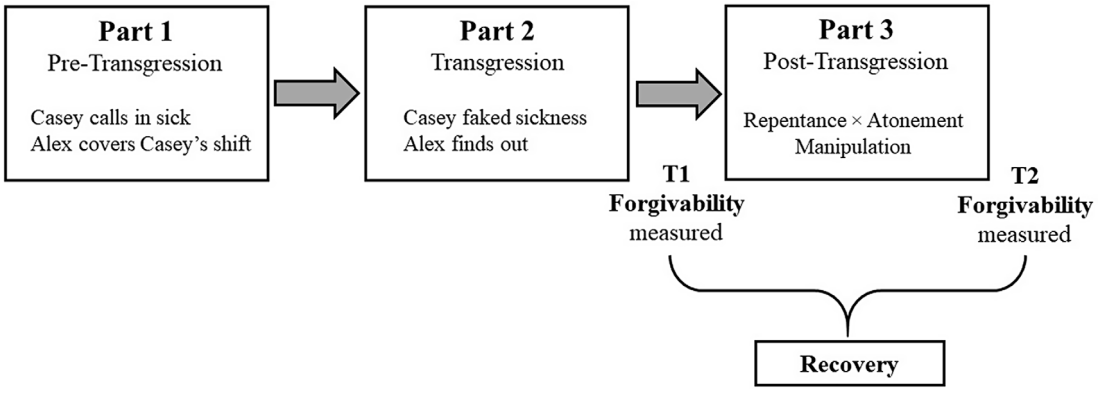

Figure 1. Diagram showing the repeated-measures design of Experiment 4. Forgivability was measured after Part 2 and Part 3.

between atonement and forgivability and predicted that atonement would affect forgivability through perceived costliness, which might itself be mediated by restitution.

Method

Procedure

Participants read a three-part story. Part 1: Alex was forced to cover Casey's shift, which led Alex to cancel plans to celebrate his girlfriend's birthday with her. Part 2: Alex (victim) learned that Casey (offender) faked sickness to attend a concert. Part 3: Casey communicated or denied his repentance to Alex and atoned or did not atone a few weeks later when Alex needed a favor. Repentance was manipulated as follows:

Repent

Casey looked troubled and said, "I feel really bad about this. I never even considered that someone would have to cover my shift, but I should have and should have shown up to work yesterday. I know it doesn't change what happened, but just so you know, I feel pretty bad about it."

No-Repent

Casey said, "To be honest, I don't really feel bad about this. Maybe I should have asked for the night off ahead of time, but you could have said no to coming in. I really enjoyed the concert and I don't regret calling in."

In all versions, Casey initially declined Alex's later request to return the favor by covering Alex's shift, saying he couldn't because a friend was visiting him. Atonement was manipulated as follows:

Atone

"We already have plans to just hang out and relax," Casey said, "so tomorrow really doesn't work." At that point, Casey paused then said, "You know what, though? My friend will be in town for a few days, so I can cover for you."

No-Atone

"We already have plans to just hang out and relax tomorrow," Casey said. "I really can't. My friend is only going to be in town for a few days, so tomorrow really doesn't work for me."

Forgivability was assessed after Part 2 and again after Part 3, and other measures were assessed only after Part 3. Although forgivability was also assessed after Part 1, the meaning of perceived forgivability prior to awareness that a transgression has been committed is conceptually unclear. We therefore do not discuss this further.

Measures

The same items (with names/transgressions changed) from Experiments 1 and 3 respectively assessed remorse (α = .98) and restitution (α = .99). Forgivability was measured with two items: "Alex should forgive Casey," and "Alex should let go of any anger he may feel toward Casey" (T1 r = .73; T2 r = .87). At T2, two additional forgivability items were used. To maintain consistency in measurement across time points, we report only the analyses using the 2-item measure here. Analyses using the full measure are reported in the ESM 1 (Tables S13–S14). To capture "recovery," we subtracted T1 forgivability from T2 (higher numbers indicate greater recovery). Three items measured perceived costliness (α = .83): "Casey tried hard to help Alex," "Covering Alex's shift required a lot of effort on Casey's part," and "To what extent did Casey sacrifice other plans to help Alex?" (1 = *not enough at all*, 7 = *more than enough*).

Results and Discussion

Primary hypotheses were examined using 2 (No-Repent/Repent) × 2 (No-Atone/Atone) ANOVAs with 1, 154 *df*.

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Table 2. Experiment 4: Means and standard deviations as a function of repentance and atonement

	No repent				Repent			
	No Atone		Atone		No Atone		Atone	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Remorse	1.49	1.00	4.06	1.83	2.81	1.60	5.95	1.03
Restitution	1.39	0.97	5.05	1.50	1.70	1.05	6.11	1.02
T1 forgivability	2.33	1.43	2.29	1.51	2.61	1.57	1.96	1.46
T2 forgivability	3.14	1.45	4.93	1.72	3.54	1.76	5.54	1.39
Recovery (T2 − T1)	0.81	1.20	2.64	1.86	0.93	1.23	3.57	1.94
Costliness	1.97	0.85	3.83	1.53	2.12	0.92	4.50	1.28
Cell *N*	39		40		38		41	

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Manipulation Check

For remorse, main effects of repentance (F = 50.97, p < .001, d = 0.82) and atonement (F = 161.20, p < .001, d = 1.82) were found. The interaction was not significant (p = .204). For restitution, main effects of atonement (F = 478.92, p < .001, d = 3.32) and repentance (F = 13.87, p < .001, d = 0.32) were found, as well as a significant interaction (F = 4.11, p = .044, η_p^2 = .03) that suggested the effects of atonement were slightly stronger when repentance was also present. Table 2 provides *M* and *SD* for all variables.

Of interest, the effect size for remorse was descriptively larger as a function of atonement than of repentance, suggesting that actions aimed at making amends imply feeling bad about what one has done. To clarify these relationships, we examined the correlation between remorse and restitution (r = .84, p < .001) and then respectively tested the effects of repentance and atonement on remorse and restitution while controlling for the other variable using 2 (No-Repent/Repent) × 2 (No-Atone/Atone) ANCOVAs with 1, 153 *df*. When controlling restitution, atonement no longer significantly predicted remorse (p = .245; interaction p = .934). Repentance remained significant, F = 35.43, p < .001. Similarly, while controlling remorse, repentance and the interaction no longer predicted restitution (respectively, ps = .252, .120), but atonement remained significant, F = 156.97, p < .001. This confirmed that although remorse and restitution responses were strongly associated, each manipulation worked to influence the linked construct above and beyond that of the other.

Forgivability

Atonement strongly predicted forgivability at T2 (F = 56.04, p < .001, $CI_{.95}$ = [1.39, 2.39], d = 1.19). Repentance also predicted T2 forgivability (F = 4.00, p = .047, $CI_{.95}$ = [0.03, 1.03], d = 0.29), although this effect size was descriptively much smaller. The interaction was not significant, p = .674. The effects of repentance and atonement on recovery from transgression (T2 − T1) were both significant, respectively, Fs = 4.33 and 76.69, p = .039 and p < .001, $CI_{.95}$s = [0.07, 1.07] and [1.73, 2.74], ds = 0.29 and 1.38. The interaction was not significant, F = 2.51, p = .115. This demonstrates that repentance and atonement are independently associated with increases in forgivability from baseline. Single sample t-tests of each cell against zero demonstrated recovery in each cell of the design, ts(37–40) > 4.22, ps < .001, ds > 0.68.

It is somewhat puzzling that there was some recovery even in the No-Repent/No-Atone cell. Speculatively, Casey might have been seen as somewhat forgivable because (a) his offering of a counterfactual (i.e., Alex could have said he couldn't cover Casey's shift) created doubts about the severity of the offense, and (b) people considered it reasonable that Casey didn't want to commit another offense by canceling plans with his friend. To address this, future research might describe a more serious offense where atonement does not require the potential commission of another offense against someone else.

Mediation

We first tested whether atonement impacted the putative mediator, perceived costliness. Both repentance (F = 4.83, p = .029, d = 0.28) and atonement (F = 126.23, p < .001, d = 1.77) impacted costliness; the interaction was nonsignificant (p = .174). Because costliness was affected by repentance, we considered examining whether it might statistically mediate the effects of repentance on forgivability but did not because this effect was unpredicted and not theoretically grounded, making explanation of any statistically significant effect necessarily post-hoc. Additionally, absent atonement, repentance should not affect costliness because no effort was expended to help the victim in the No-Atone cells. Confirming this, the simple effect of repentance on costliness when atonement was absent was not significant, t(75) = 0.78, p = .438. Conversely, the simple effects of atonement on costliness were significant at both levels of repentance, ts(77) > 6.64, ps < .001 (see Table 2). Thus, the main effect of repentance likely reflects a slight boost in perceived costliness when the offender not only

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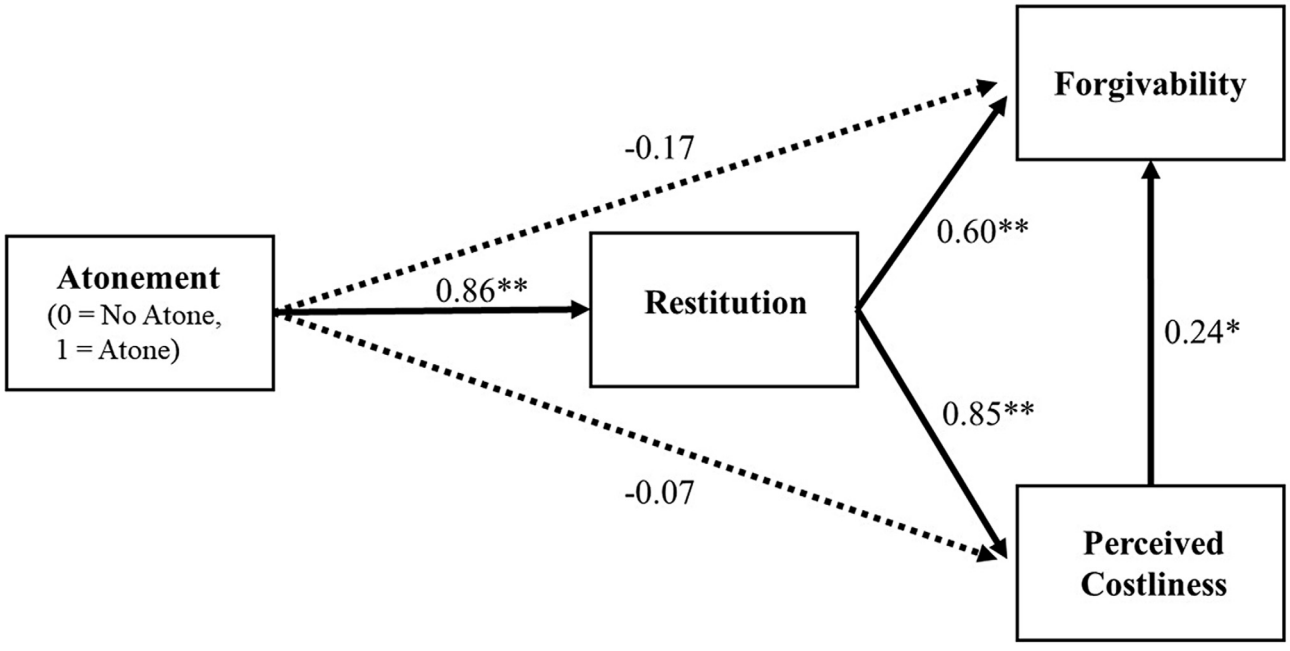

Figure 2. Mediation model predicting forgivability from atonement, restitution, and perceived costliness in Experiment 4. Path coefficients are standardized coefficients. Confidence intervals of path coefficients and significance levels of all indirect effects are reported in text. *$p < .05$; **$p < .001$.

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expended effort but did so because he felt bad about causing harm. Given these findings, mediation tests focused solely on explaining the effect of the atonement manipulation on T2 forgivability.

Costliness was correlated with T2 forgivability and restitution (*r*s = .61, .79, respectively, *p*s < .001), making mediation of atonement on forgivability through costliness possible. Because restitution conceptually represents perceptions that the offender *performed* a concrete action aimed at making amends, and costliness conceptually represents the extent to which this action was *effortful*, we tested a model with atonement (No-Atone = 0; Atone = 1) as an exogenous predictor of restitution, costliness, and forgivability, and restitution as an endogenous predictor of costliness and forgivability, with costliness also predicting forgivability (see Figure 2). In this model (10,000 bootstrap resamples), atonement predicted restitution ($b = 4.04$, $CI_{.95} = [3.67, 4.42]$, $p < .001$), but its direct effect on costliness ($b = -0.21$, $CI_{.95} = [-0.85, 0.45]$, $p = .525$) and forgivability ($b = -0.62$, $CI_{.95} = [-1.62, 0.39]$, $p = .229$) were not significant. Restitution predicted both costliness ($b = 0.58$, $CI_{.95} = [0.43, 0.72]$, $p < .001$) and forgivability ($b = 0.47$, $CI_{.95} = [0.21, 0.75]$, $p < .001$), and costliness predicted forgivability ($b = 0.28$, $CI_{.95} = [0.04, 0.51]$, $p = .018$). The indirect effects of atonement on costliness through restitution ($b = 2.33$, $CI_{.95} = [1.66, 2.99]$, $p < .001$), on forgivability through restitution alone ($b = 1.91$, $CI_{.95} = [0.82, 3.03]$, $p < .001$), and on forgivability through restitution and costliness ($b = 0.66$, $CI_{.95} = [0.03, 1.26]$, $p = .035$), were all significant. Thus, atonement influenced forgivability by increasing perceptions that the offender tried to "make things right," and when perceivers saw this action as more costly, forgivability was further increased.

Experiment 4 replicated the primary findings from Experiments 1 to 3 and provided insight into how repentance and atonement promote recovery from initial damage associated with a transgression. In addition, Experiment 4 demonstrated that efforts aimed at repair, particularly when costly, can impact forgivability, suggesting that third-party observers may notice social cues displayed by offenders even when personal motives for reconciliation are absent. We note, however, that because repentance preceded atonement and forgivability was not assessed between the two manipulations, people in the No Atone condition might have questioned the sincerity of repentance when the subsequent action was inconsistent with the offenders' stated attitudes (Laurent & Clark, 2019).

Experiment 5

Past work has shown that repentance and atonement increase forgiveness from victims and close others. Experiments 1–4 demonstrated how repentance and atonement uniquely contribute to uninvolved observers' perceptions

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of forgivability. One remaining question is whether repentance and atonement have similar or different effects on forgivability from outside observers as compared with victims or victims' close others. A last experiment was conducted to examine this question.

Based on the third-party unforgiveness effect (Green et al., 2008), we hypothesized that involved third parties would perceive the offender to be less forgivable than would victims but were uncertain whether forgivability from uninvolved third parties would differ from that of victims or involved parties. That is, although outside observers – despite having only a symbolic stake in the matter – may believe forgiveness is deserved on the basis of repentance and atonement, we were not certain whether these factors would have a weaker or stronger effect for uninvolved parties than for victims.

Because no interactions of repentance and atonement emerged on forgivability in Experiments 3–4, Experiment 5 focused on their unique effects (i.e., repentance without atonement, atonement without repentance, neither repentance nor atonement). This manipulation was crossed with perceiver role: victim, victim's close friend, or stranger.

Method

Procedure

Experiment 5 used a 3 (Offender-Response: Repent/No-Atone, No-Repent/Atone, No-Repent/No-Atone) × 3 (Role: Victim, Friend, Uninvolved) design. Participants in the Friend condition typed the first name of their closest friend in a textbox, and this name ["friend"] appeared where relevant thereafter in the survey. Participants read a vignette, adapted from Okimoto, Wenzel, and Feather (2009), about a neighbor damaging the victim's car. Participants were told to imagine the transgressor was their own neighbor (Victim), their closest friend's neighbor (Friend), or "Jordan's" (a stranger's) neighbor (Uninvolved). The offender-response manipulation was embedded in the neighbor's reply:

Repent/No-Atone

The neighbor looks regretful and says, "I understand that you're upset and I should've told you as soon as it happened. . .I feel really bad about it." Despite their remorseful attitude, the neighbor does not say they are sorry or attempt to financially compensate you [friend/Jordan] for the damage.

No-Repent/Atone

The neighbor, showing no visible regret, says, "I understand that you're upset and that you think I should've told you as soon as it happened." Despite their apparent lack of remorse and failure to say they are sorry, the neighbor offers to financially compensate you [friend/Jordan] for the damage.

No-Repent/No-Atone

The neighbor, showing no visible regret, says, "I understand that you're upset and that you think I should have told you as soon as it happened." In addition to their apparent lack of remorse, the neighbor does not say they are sorry or attempt to financially compensate you [friend/Jordan] for the damage.

Table 3. Experiment 5: Means and standard deviations as a function of offender-response and perceiver role

	Victim			Friend			Uninvolved		
	None	Repent	Atone	None	Repent	Atone	None	Repent	Atone
	M (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)	*M* (*SD*)
Remorse	1.57 (0.95)	3.85 (1.65)	2.39 (1.24)	1.74 (1.36)	3.76 (1.27)	2.66 (1.59)	1.74 (1.15)	3.88 (1.76)	2.71 (1.42)
Restitution	1.28 (0.78)	1.52 (0.82)	5.09 (1.72)	1.48 (1.15)	2.01 (1.27)	5.42 (1.24)	1.49 (0.99)	1.83 (1.28)	5.38 (1.04)
Forgivability	2.47 (1.47)	3.02 (1.46)	4.28 (1.75)	2.67 (1.32)	3.05 (1.41)	4.33 (1.37)	2.69 (1.60)	3.06 (1.48)	4.30 (1.26)
Cell *N*	33	41	35	40	38	40	37	39	34

Table 4. Experiment 5: Summary of inferential statistics for main and interaction effects of offender-response and perceiver role on remorse, restitution, and forgivability

	Offender-Response			Role			Offender-Response × Role		
	df = (2, 328)			*df* = (2, 328)			*df* = (4, 328)		
	F	*p*	η_p^2	*F*	*p*	η_p^2	*F*	*p*	η_p^2
Remorse	67.07	< .001	.29	0.43	.652	.00	0.19	.944	.00
Restitution	364.65	< .001	.69	2.60	.076	.02	0.16	.960	.00
Forgivability	39.30	< .001	.19	0.15	.858	.00	0.05	.995	.00

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Measures

Four remorse items (α = .97) checked the repentance manipulation: "The neighbor felt…" "guilty," "remorse," "regret about what happened," and "bad about damaging the car." Four restitution items (α = .97) checked the atonement manipulation: "The neighbor…" "tried to atone for the damage they had caused," "tried to 'make things right,'" "attempted to repair the harm they had caused," and "offered to fix the problem they had caused."

Forgivability (α = .88) was measured with three items reflecting participants' assigned roles: "Despite what happened to me [friend/Jordan], I would have compassion for the neighbor," "the neighbor deserves to be forgiven for what they did to you [friend/Jordan]," and "I would let go of any anger I might feel toward the neighbor." Finally, participants responded to, "I was asked to imagine that the neighbor was…" by selecting "my neighbor," "my closest friend's neighbor," or "not related to me in any way."

Results and Discussion

A series of 3 (Offender-Response: Repent/No-Atone, No-Repent/Atone, No-Repent/No-Atone) × 3 (Role: Victim, Friend, Uninvolved) ANOVAs were conducted to examine effects on remorse, restitution, and forgivability. Table 3 provides *M* and *SD* for all variables. Inferential statistics for all analyses below are reported in Table 4.

Manipulation Check

Planned *t*-tests revealed that the repentance manipulation significantly increased offender remorse relative to the No-Repent/Atone, $t(225) = 6.24$, $p < .001$, $d = 0.83$ and No-Repent/No-Atone conditions, $t(226) = 11.70$, $p < .001$, $d = 1.55$. Similarly, restitution in the No-Repent/Atone condition was higher than in the Repent/No-Atone, $t(225) = 21.15$, $p < .001$, $d = 2.81$ and No-Repent/No-Atone conditions, $t(217) = 24.16$, $p < .001$, $d = 3.26$. No main effects of perceiver role or interactions of role and offender-response were found on remorse or restitution (see Table 4). Regarding their relationship to the neighbor, 27.3% of participants in Uninvolved, 15.3% in Friend, and 7.3% in Victim condition responded incorrectly, and these proportions were significantly different, $\chi^2(2) = 15.94$, $p < .001$. We report the results with the full sample below. Analyses excluding these participants are reported in the ESM 1.

Forgivability

A main effect of offender-response was found on forgivability. Planned *t*-tests revealed that all three offender-response conditions significantly differed in ratings of forgivability. Forgivability was higher in Repent/No-Atone than in No-Repent/No-Atone, $t(226) = 2.21$, $p = .028$, $d = 0.29$, $CI_{.95} = [0.05, 0.80]$, and higher in No-Repent/Atone than in Repent/No-Atone, $t(225) = 6.55$, $p < .001$, $d = 0.87$, $CI_{.95} = [0.88, 1.64]$, and No-Repent/No-Atone conditions, $t(217) = 8.55$, $p < .001$, $d = 1.16$, $CI_{.95} = [1.30, 2.07]$.

Although we expected that perceivers in the Friend role would see the offender as less forgivable than perceivers in the Victim role, our findings did not support that prediction as no main effect of role or interaction of role and offender-response were found on forgivability (see Table 4). The role participants were asked to take had relatively little influence on how forgivable the offender seemed, suggesting that the positive effects of repentance and atonement on forgivability worked similarly in each case.

Speculatively, differences between the current study and those reported in Green et al. (2008) might have emerged for two reasons. First, in the current study, the dependent variable was perceived forgivability rather than actual forgiveness or willingness to forgive. Although involved third parties may be less forgiving than victims, both parties may recognize offenders' forgivability to a similar degree. Second, to ensure that the neighbor's offense was one that could be objectively atoned for, we used a form of harm that solely involved material damage. Because the transgression in Green et al. (2008) was emotional harm through social embarrassment by the victim's romantic partner, additional moral violations (e.g., trust betrayal) may have been inferred. Future research might investigate these possibilities directly.

In sum, Experiment 5 extended previous third-party forgiveness research by demonstrating that for uninvolved observers, as well as victims and their close others, an offender who atoned without repenting deserved forgiveness more than one who repented without atoning, and an offender who repented without atoning deserved forgiveness more than an offender who made no post-transgression efforts.

General Discussion

When blameworthy transgressions occur, offenders' post-transgression responses influence whether they will be forgiven by victims (e.g., Tabak, McCullough, Luna, Bono, & Berry, 2012). The current research examined whether offender efforts extend beyond victims and influence third parties' perceptions of forgivability. Five experiments showed that repentance and atonement each independently increase perceived forgivability from socially distant third parties. By examining forgiveness from this relatively disinterested perspective, this work extends prior research, demonstrating that post-transgression attitudes and actions are important factors in enhancing the perceived forgivability of offenders in the eyes of uninvolved third parties.

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A second contribution of this work regards the disentangling of repentance from atonement, both of which are implied in apology, and demonstrating their independent effects on forgivability. Separating these concepts is a useful endeavor that should spur further research. For example, offenders can apologize without repenting (Ohtsubo et al., 2012) or deceptively express remorse to reduce punishment (Hogue & Peebles, 1997). Similarly, offenders can repent without informing anyone about their mental states, and behaviors aimed at restitution can exist with or without remorse. By empirically isolating repentance and atonement, the current work has taken initial steps in understanding how, why, and what parts of apology function to promote forgivability.

Five studies featuring different categories of harm, victim-offender relationships, and perceiver roles found converging evidence that repentance and atonement individually influence perceived forgivability. In Experiment 1, a TA who communicated repentance was viewed as more forgivable than a non-repentant one. In Experiment 2, a supervisor was seen as more deserving of forgiveness when she tried but failed to make up for her oversight relative to when she did not attempt to atone. By isolating atonement from the positive outcomes typically associated with attempts at restitution, Experiment 2 demonstrated the robust connection between actions directed at "making things right" and forgivability. Experiment 3 manipulated both repentance and atonement, replicating the results of Experiments 1 and 2 and extending them to a situation with severe physical harm and equal power status between the parties. Experiment 3 also showed that offender remorse can increase forgivability even when it is not communicated to the victim (but is revealed to participants). In Experiment 4, we found that both repentance and atonement facilitate recovery from negative judgments associated with a transgression, further demonstrating how each factor worked to repair forgivability from a baseline level. Finally, Experiment 5 demonstrated that at least in the provided context, repentance and atonement worked to increase forgivability in the same way for victims, victims' friends, and outside observers. Together, these results suggest that offenders' post-transgression mental states and behaviors influence perceived forgivability and that costly behavior aimed at repair can redeem offenders from the taint of transgression.

Limitations and Future Directions

Limitations to the present research should be noted. First, each experiment used hypothetical vignettes to describe unintended transgressions. This method allowed control over what information people received about post-transgression attitudes, behaviors, and outcomes and is similar to how third-party perceivers might receive information in real contexts. Yet, this design may have elicited different evaluations than would naturally occur. Second, because participants were asked in most studies to evaluate the extent to which victims should forgive transgressors, participants may have tried to adopt the described victims' perspectives and based their forgivability ratings on what they would have done in the same situation. Future research might explore whether this is the case, perhaps examining whether perspective-taking instructions enhance or decrease perceived forgivability. Third, the current research relied on self-reported measures. Although social desirability might not be as critical as it would be for victims (Risen & Gilovich, 2007), using behavioral or physio-neurological responses could complement our conclusions. Fourth, repentance and atonement may influence forgivability differently in other cultural contexts. Because participants in the current experiments were all US residents recruited online, further research would be needed to test whether our findings would replicate in non-Western populations (Henrich, Heine, & Norenzayan, 2010). We have no reason to believe that the results depend on other characteristics of the participants, materials, or context (Simons, Shoda, & Lindsay, 2017).

Several areas for future research seem promising. Although we have identified repentance and atonement as influential in promoting forgivability, the psychological mechanisms by which these factors exerted effects remain unknown. One possibility involves third parties' feelings of injustice when offenders illegitimately violate shared values and/or achieve power/status over the group with which third parties identify (Okimoto & Wenzel, 2008; Okimoto et al., 2009). Third-party punishment reestablishes social order by invalidating an offender's presumed power/status over the group and the rules. Similarly, repentance and atonement may drive third-party forgivability by reaffirming shared societal values and offenders' commitment to them; future research might examine this hypothesis.

The present research focused on third-party perceptions of offenders. However, victims' reactions to offenders' repentance and atonement can be valuable information for observers in evaluating future cooperation partners. For example, the deterrence hypothesis posits that third-party intervention emerges because mistreatment of a third-party connotes the potential for later mistreatment of oneself (Krasnow, Delton, Cosmides, & Tooby, 2016). Accordingly, being attentive to the retaliatory or forgiving capability of others might be advantageous for third parties (dos Santos, Rankin, & Wedekind, 2011). Recent work has already begun to examine what forgiveness signals to uninvolved observers (Yao & Chao, 2019); future work might contrast how offenders' post-transgression actions influence third-party perceptions of forgiving and unforgiving victims. In addition, future research should address how

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forgivability may be influenced by the degree and type of actions aimed at atonement as well as offenders' motives for atonement. Experiment 4 provided supporting evidence for the role of perceived costliness in facilitating forgivability. Thus, examining the net cost incurred by an atoning agent in light of potential or actual benefits is worth considering. Although smaller offers of penance might be equally effective as larger offers for victims (Bottom et al., 2002), observers may be particularly attentive to the costs offenders are willing to incur to reestablish cooperation. Finally, another important question concerns the mental states motivating harmful actions. Here, all experiments investigated repentance and atonement for unintended harms. Will repentance and atonement affect forgivability for foreseen, reckless, or intended transgressions?

The current research has not answered all of these questions, but it has provided important initial steps in distinguishing the roles of mental states from observable reparative actions and in showing that these factors influence forgivability in disinterested contexts. Examining perceived forgivability can contribute to our understanding of person-perception processes that require some degree of objectivity, such as decisions made in criminal justice contexts. Repentance and atonement may serve as attempts to undo the damage wrought, in hopes of restoration in the eyes of those whom offenders have wronged as well as others who are aware of their misdeeds. As we have discussed, prior research has shown the social function of apology in mending damaged relationships with the direct recipients of such reconciliatory gestures. Understanding third-party responses to offender efforts at repair can illuminate consequences that surpass victim-transgressor dyads, influencing offender reintegration, social harmony, and peace-making.

Electronic Supplementary Material

The electronic supplementary material is available with the online version of the article at https://doi.org/10.1027/1864-9335/a000390

ESM 1. Study materials, supplementary measures and analyses

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History
Received July 16, 2018
Revision received March 10, 2019
Accepted April 5, 2019
Published online August 20, 2019

Authorship
Both authors were involved in all parts of the research.

ORCID
Shoko Watanabe
https://orcid.org/0000-0002-6118-1736

Shoko Watanabe
Department of Psychology
University of Illinois at Urbana-Champaign
603 E. Daniel Street
Champaign, IL 61820
USA
shokow2@illinois.edu

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Original Article

The Effects of Exposure to Positive Gender Stereotypes on Women's and Men's Performance in Counter-Stereotypical Tasks and Pursuit of Agentic and Communal Goals

Rotem Kahalon[1], Nurit Shnabel[1], and Julia C. Becker[2]

[1]The School of Psychological Sciences, Tel-Aviv University, Israel
[2]Institute of Psychology, Osnabrück University, Germany

Abstract: Two studies examined the effects of exposure to positive gender stereotypes on performance in counter-stereotypical domains and pursuit of agentic and communal goals. Exposure to stereotypes about women's communality (Study 1, *N* = 108) led to impaired math performance among women, regardless of their math identification. Exposure to stereotypes about men's agency (Study 2, *N* = 129) led to impaired performance in a test of socio-emotional ability among men high in domain identification. Moreover, among women with high math identification, exposure to the communality stereotype increased the pursuit of agentic goals. Among men, exposure to the agency stereotype tended to decrease the pursuit of communal goals. These results are consistent with accumulating evidence for the "dark side" of positive stereotypes, yet, for women, they also point to active attempts to counteract them.

Keywords: stereotype threat, women in STEM, gender stereotypes, positive stereotypes, interpersonal goals, gender roles

Gender segregation in professions and higher education, such that women are underrepresented in STEM fields (Science, Technology, Engineering, and Math) whereas men are underrepresented in HEED fields (Healthcare, Early Education, and Domestic Roles) is a major barrier to gender equality (e.g., Diekman, Steinberg, Brown, Belanger, & Clark, 2017). The social psychological literature has pointed to negative gender stereotypes (e.g., that women are bad at math, or that men have low emotional intelligence) as one of the key mechanisms responsible for this segregation (Cundiff, 2018; Ellemers, 2018). Not only do negative stereotypes reduce women's and men's engagement in and identification with domains in which they are stigmatized (Hall, Schmader, & Croft, 2015; Kalokerinos, Kjelsaas, Bennetts, & von Hippel, 2017), they also impair their actual performance due to the experience of *stereotype threat,* namely concern about confirming negative stereotypes regarding their gender's inferior ability in stigmatized domains, which causes stress that ultimately undermines actual performance (Steele, 1997; for a review, see Spencer, Logel, & Davies, 2016). Ironically, stereotype threat effects tend to be particularly pronounced among individuals with strong domain identification, who wish to succeed in the field in which their gender is stigmatized (e.g., Keller, 2007).

For women, stereotype threat effects can manifest in various domains (e.g., driving; Yeung & von Hippel, 2008), yet the vast majority of research has focused on math performance. This research has demonstrated that the mere awareness of negative stereotypes about women's math ability can interfere with the performance of female participants in math tests (Spencer, Steele, & Quinn, 1999; see Nguyen & Ryan, 2008 for a meta-analysis). Moreover, stereotype threat was associated with women's reduced interest in pursuing math-oriented occupations later on (Davies, Spencer, Quinn, & Gerhardstein, 2002) and with feelings of exhaustion and psychological burnout when already working in STEM-related fields (Hall et al., 2015).

Stereotype threat effects among men have been relatively understudied. The existing research revealed that exposure to either direct (Leyens, Désert, Croizet, & Darcis, 2000) or

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subtle (Koenig & Eagly, 2005) reminders of the stereotype that men have low socio-emotional skills, impaired men's performance in tests of emotional abilities. Moreover, a field study among men working in female-dominated occupations revealed that these men were often experiencing stereotype threat, which in turn led to negative job attitudes and intentions to resign (Kalokerinos et al., 2017).

But what about positive gender stereotypes? As opposed to negative stereotypes, which are delegitimized in today's Western society, positive stereotypes are robustly used and considered legitimate and even complimentary (Kay, Day, Zanna, & Nussbaum, 2013; Mae & Carlston, 2005). Despite their flattery, positive stereotypes might have negative consequences because, like negative stereotypes, they ascribe traits to individuals based on their group affiliation (Czopp, Kay, & Cheryan, 2015). This leads the targets of such stereotypes to perceive positive comments about their group membership as prejudiced (Czopp, 2008). For example, female participants believed that, compared to a man who expressed no stereotypes, a man who expressed positive stereotypes about women (e.g., "women are nurturing") was also likely to endorse negative stereotypes about women (Siy & Cheryan, 2016) – suggesting that positive stereotypes signal to their targets an underlying negativity toward their group. If so, exposure to positive gender stereotypes might also lead to stereotype threat effects.

A recent study by Kahalon, Shnabel, and Becker (2018) confirmed this possibility, demonstrating that: (a) exposure to the stereotype about women's communality (according to which women are empathic, motherly, etc.) impaired the math performance of female participants with strong domain identification (i.e., who find math rewarding and wish to succeed in it; Smith & White, 2001), and (b) exposure to the stereotype about men's agency (according to which men are assertive, have natural leadership qualities, etc.) impaired the performance in a test of emotional skills of male participants with strong identification with the socio-emotional domain. These findings are consistent with the notion that stereotypes in general (Kervyn, Yzerbyt, & Judd, 2010), and gender stereotypes in particular (Eagly & Wood, 1999), are complementary: women are perceived as communal and warm but also as low on agency, whereas men are perceived as agentic but also as low on communality and warmth (Fiske, Cuddy, & Glick, 2007). Moreover, research on the "innuendo effect" (Fiske et al., 2015; Kervyn, Bergsieker, & Fiske, 2012) tells us that when people receive information about social targets that focuses on their positive traits, they infer complementary negative traits; for example, when learning that a given woman is very nice, people infer that she is not highly intelligent.

Although Kahalon et al.'s (2018) findings are consistent with the literature about the complementary nature of stereotype and the innuendo effect, they have been the sole demonstration to date that activation of positive gender stereotypes can produce stereotype threat effects. Thus, a main purpose of the present research was to test the replicability of these findings – which was critical for establishing the validity of the conclusions about the "darker side" of positive stereotypes. Replicating social psychological results is generally necessary to confirm the accuracy of the empirical findings, clarify the conditions under which an effect can be observed, and estimate the true effect size (Open Science Collaboration, 2012). Yet replication efforts are especially important with regard to stereotype threat effects, in light of recent concerns that – despite evidence that stereotype threat effects represent a real phenomenon (Flore & Wicherts 2015; Nguyen & Ryan 2008) – their size has been overestimated in the literature (i.e., stereotype threat effects are smaller than suggested by previous research; Flore & Wicherts, 2015).

Besides replicating previous findings, we tested some new hypotheses. Theoretically, stereotype threat effects are distinct from de-motivation processes; they occur due to the experience of stress and intrusive negative thoughts (Cadinu, Maass, Rosabianca, & Kiesner, 2005) and despite the motivation to do well (Schmader, Johns, & Forbes, 2008). Yet Kahalon et al.'s (2018) research did not tell us whether exposure to the positive aspects of the gender stereotype influenced women's and men's test motivation – which could be an alternative explanation (rather than stereotype threat) to the observed decrease in their performance. Finding that exposure to positive gender stereotypes impaired women's math performance and men's socio-emotional performance *without* changing their test motivation could provide further support for stereotype threat as the reason for performance decrements. Thus, the second aim of the present research was to examine women's and men's motivation to succeed in the test.

Finally, a third goal of the present study was to explore the effects of exposure to the communality and agency stereotypes on women's and men's interpersonal goals; that is, preferences for certain interpersonal outcomes or modes of conduct (Rokeach, 1973). Interpersonal goals can be organized within one conceptual space, defined by two orthogonal axes, in which each point is specified as a weighted mixture of agency and communion (Locke, 2000). High (vs. low) pursuit of agentic goals reflects the motivation to gain competence and power (vs. submissiveness and helplessness); high (vs. low) pursuit of communal goals reflects warmth and empathy toward others (vs. coldness and detachment).[1]

[1] Although competence and power, or warmth and morality, reflect somewhat different constructs they can be subsumed under the broad "big two" categories of agency and communion (Abele & Wojciszke, 2013).

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The pursuit of agentic and communal goals is influenced by situational cues. To illustrate, after being victimized by others, people pursuit more agentic goals (e.g., being tough), whereas after victimizing others, people pursuit more communal goals (e.g., being nice) (Aydin, Ullrich, Siem, Locke, & Shnabel, 2018). In the present study, thus, we examined how situational exposure to positive gender stereotypes affects the pursuit of agentic and communal interpersonal goals.

The existing literature offers two different (albeit not contradicting) predictions. First, theorizing on the "sweet persuasion" of positive stereotypes (Barreto & Ellemers, 2005) suggests that members of stigmatized groups often embrace and align their behavior with them. Hence, consistent with the notion that positive stereotypes are prescriptive (whereas negative stereotypes are descriptive; Heilman, 2001), participants should align their goals with the positive stereotypes to which they are exposed: exposure to stereotypes about women's communality should increase female participants' pursuit of communal goals, whereas exposure to stereotypes about men's agency should increase male participants' pursuit of agentic goals. If so, positive stereotypes may perpetuate traditional gender roles by leading women and men to fulfill their respective roles as "nice" or "assertive" in interpersonal interactions.

Second, the experience of stereotype threat may lead members of stigmatized groups who highly identify with the domain in which their group is stigmatized (e.g., female STEM students performing a math test, Pronin, Steele, & Ross, 2004; female business students performing a negotiation task; Kray, Thompson, & Galinsky, 2001) to distance themselves from the negative stereotype about their group or try to counteract it. If so, exposure to positive gender stereotypes may lead participants who are high in domain identification to endorse goals that would counteract the negative stereotypes about their group (which are activated along with the positive stereotypes). Thus, women who identify with math should pursue more agentic goals in response to the communality stereotype, and men who identify with the socio-emotional domain should pursue more communal goals in response to the agency stereotypes. Such patterns would indicate that the experience of stereotype threat – induced by exposure to positive gender stereotypes (Kahalon et al., 2018) – can lead to reactance responses (Brehm & Brehm, 1981), in an attempt to rebel against and challenge traditional gender roles (Kray et al., 2001).

The Present Research

The present research consisted of two studies, Study 1 conducted among women and Study 2 conducted among men, which used a two-cell experimental design to test the following hypotheses:

Hypothesis 1 (H1): Exposure to positive stereotypes about one's gender would lead to performance decrements in a counter-stereotypical task (math in Study 1, emotion recognition in Study 2) among women (Study 1) and men (Study 2) with high domain identification. Confirming this hypothesis would replicate Kahalon et al.'s (2018) previous findings.

Hypothesis 2 (H2): Exposure to positive gender stereotypes would not change women's (Study 1) and/or men's (Study 2) test motivation. Confirming this hypothesis would extend Kahalon et al.'s (2018) previous findings by ruling out reduced test motivation as an alternative explanation.

Hypothesis 3 (H3): Exposure to positive gender stereotypes would increase the pursuit of goals that are consistent with these positive stereotypes; namely, communal goals among women (Study 1) and agentic goals among men (Study 2). This exploratory hypothesis tested for changes in goal pursuit that *reinforce* traditional gender roles.

Hypothesis 4 (H4): Exposure to positive gender stereotypes would increase the pursuit of goals that are inconsistent with the negative gender stereotypes that "complement" these positive stereotypes; namely, increased pursuit of agentic goals among women (Study 1) and increased pursuit of communal goals among men (Study 2). This exploratory hypothesis tested for changes in goal pursuit that *challenge* traditional gender roles.

The study conforms to recognized ethical standards, data files can be accessed through the Open Science Framework (OSF) https://osf.io/hktz3.

Study 1

In Study 1, female participants were randomly assigned either to an experimental condition in which they were exposed to the positive stereotype about women's communality, or to a control/no-stereotypes condition. They then completed a math test, a measure of their motivation to succeed in the test, and a circumplex measure of their pursuit of agentic and communal goals in interpersonal interactions. Based on our hypotheses, we expected the experimental condition to have (a) a negative effect on participants' math performance – especially among those with

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high math-identification, and (b) no effect on participants' motivation. We also explored whether the experimental condition would increase the pursuit of (a) communal goals, in alignment with the prescriptive nature of positive stereotypes (i.e., that women should be nice; Heilman, 2001), and/or (b) agentic goals, in an attempt to counteract negative expectations about women's agency (Kray et al., 2001) – especially among participants with high domain-identification (in line with Pronin et al., 2004). The study was preregistered through the OSF https://osf.io/pa9j5.

Method

Participants

A power analysis using G*Power calculator (Faul, Erdfelder, Buchner, & Lang, 2009) revealed that 124 participants were needed to detect the small to medium effect size (f^2 = .08) observed by Kahalon et al. (2018), at a significance of 5% and power of 80%. Data collection was stopped after the recruitment of 115 participants, since there were no new sign-ups. Participants were undergraduate female students majoring in diverse disciplines (e.g., psychology, engineering, business). To avoid disproportional influence of extreme observations on our analysis (Osborne & Overbay, 2004), we excluded seven outliers based on our preregistered decision to exclude participants with high Cook's (1977) distance value (i.e., whose distance was greater than $4/n$; Bollen & Jackman, 1990). Thus, the sample included 108 participants.[2] A sensitivity analysis (Faul et al., 2009) for a sample of 108 participants and power of 80% at α = .05, revealed that for a single predictor, in a multiple regression analysis, effects above R^2 = .069 will be reliably detected. The effect for the main predictor in Study 1 was R^2 = .075. All participants were Israeli Jewish, and their native tongue was Hebrew; 86.3% described themselves as heterosexual, and the rest as either lesbian (6.4%) or bisexual (7.3%); M_{age} = 22.60 (SD = 2.11).

Procedure and Materials

Participants were invited to a laboratory study on "academic tendencies in different domains." All the materials were computerized. Participants first completed a 5-item measure of their math identification (adapted from Smith & White, 2001), which captured their interest and success in math (e.g., "It is highly likely that I will work in a math related field"; 1 = *strongly disagree* to 5 = *strongly agree*), α = .86. To disguise the study's purpose, participants completed additional filler questions about their identification with the domain of verbal ability.

Next, ostensibly presented as a verbal ability task, participants read short texts that constituted the experimental manipulation (see Appendix A in the OSF deposit https://osf.io/hktz3/). In both conditions, participants were presented with a symbol, followed by a short scientific explanation about the meaning of this symbol and three reading comprehension questions. This allowed us to disguise the real purpose of the manipulation as "a test of verbal abilities," and activate the relevant stereotype in a subtle manner. The text in the control condition discussed the meaning of the @ sign. For example, it explained that the sign was originally used to represent a unit of weight (Amphora). The text in the communality stereotype condition discussed the meaning of the circle symbol in the Bender-Gestalt Test as representing femininity. For example, it was explained that this symbol represents women's "innate maternal qualities," such as containment and natural sensitivity.

The texts were conceptually similar to those used by Kahalon et al. (2018), yet with slight modifications intended to improve the original manipulation. Specifically, the original control condition discussed the Yin-Yang symbol, which represents the complementarity of natural elements such as light and darkness; the original experimental condition first mentioned that the Bender-Gestalt test includes both a square and a circle symbols, representing the complementary roles of women and men, and then went on to discuss the circle symbol (explaining that it represents women's "innate maternal qualities," such as sensitivity – as done in the present study). Thus, in the original study both conditions primed participants with the concept of complementarity, yet the experimental condition additionally reminded them with the positive stereotype about women's communality. A limitation of this approach was that it has remained unclear whether a reminder of the positive communality stereotype would have the same effect on women's math performance even when the concept of complementarity is not explicitly activated. The present research addressed this limitation by using a "cleaner," unconfounded manipulation to activate the communality stereotype (for the importance of doing so, see Giner-Sorolla, Amodio, & Van Kleef, 2018).

Next, participants completed a math test (developed by Johns, Schmader, & Martens, 2005), which lasted 15 min and included 30 difficult yet solvable questions. Participants earned one point for each correct answer. They then completed the following measures.

[2] When all 115 participants were included in the analysis, the key main effect of the experimental condition on math performance became marginal (B = −1.18, t = −1.86, p = .066). For motivation and communal goals, all the effects remained nonsignificant, ps > .114. For agentic goals, the Condition × Math Identification interaction remained significant (B = 0.34, t = 2.68, p = .009).

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Test Motivation

We used a shortened version of the Student Opinion Scale (SOS; Sundre & Thelk, 2007), which assessed, using six 5-point items (1 = *strongly disagree* to 5 = *strongly agree*), the importance and effort participants placed on the test (e.g., "Doing well on this test was important to me"; "I engaged in good effort throughout this test"), α = .68.

Circumplex Scales of Interpersonal Values (CSIV)

We used the shortened 32-items (instead of 64-item) version of the CSIV (Locke, 2000). The CSIV assesses a diversity of interpersonal goals (how people want to act or be perceived when interacting with others), reflecting all possible mixtures of agentic and communal tendencies, by asking respondents to rate the importance of various interpersonal outcomes or modes of conduct (0 = *not important to me* to 4 = *extremely important to me*). It consists of eight 4-item scales, such that each scale reflects a different circumplex octant: Agentic (+A; appearing self-confident), Agentic and Communal (+A+C; expressing oneself openly), Communal (+C; feeling closeness to and developing friendships with others), Submissive and Communal(–A +C; seeking others' approval by complying with their opinions), Submissive (–A; avoiding making others angry by pleasing them), Submissive and Separate (−A−C; avoiding social embarrassment), Separate (–C; appearing detached, without revealing one's thoughts and feelings), and Agentic and Separate (+A−C; having no interest in others' opinions).

Testing the circumplex structure using multidimensional scaling (MDS; Gurtman & Pincus, 2000; detailed analysis is available at https://osf.io/hktz3) revealed that the data mapped onto two orthogonal dimensions (stress = .07). This allowed us to calculate Agentic and Communal vectors, which reflect the pursuit of agency (vs. submissiveness) and communion (vs. separateness), respectively (Locke, 2000).

Threat Appraisal

As a manipulation check, adapted from Marx (2012), three 7-point items (1 = *not at all* to 7 = *very much*) evaluated participants' experience of stereotype threat (e.g., "I worry that my ability to perform well on math tests is affected by my gender"). After the exclusion of one item (α = .53 for the 3-item scale),[3] α = .84. Note that although a manipulation check should ideally be employed immediately after the manipulation, we decided to employ it only after measuring the dependent variables because we were concerned that the blatant wording of the threat-appraisal items would reveal the real purpose of the study.

Finally, participants completed a short demographic questionnaire that assessed their pre-existing math ability by asking about their Psychometric score (the Israeli equivalent to the SAT). They were then thanked and debriefed.

Results

Descriptive statistics and correlations for all variables are presented in Table 1.

Threat Appraisal

A *t*-test for independent samples revealed that, as intended, women in the experimental condition reported experiencing a higher level of stereotype threat compared to participants in the control condition, $t(98) = 2.10$, $p = .039$.

Test Motivation

We conducted a regression analysis in which the predictors were the experimental condition, domain identification and their interaction. As expected, none of the effects or interactions reached significance, *p*s > .187. In addition, the data were analyzed by estimating a Bayes factor (BF) using Bayesian Information Criteria (BIC; Wagenmakers, 2007). This procedure examines the fit of the data under the null hypothesis, compared to the alternative hypothesis (such that $BF_{01} > 1$ suggest that there is a support for the null hypothesis). The estimation of the Bayes factor for condition and the interaction between condition and math identification, suggested that the data were in favor of the null hypothesis, $BF_{01range} = 2.45–17.97$ (Jarosz & Wiley, 2014).

Math Performance

To test our main hypothesis, we conducted a regression analysis in which the predictors were the experimental condition, math identification, and their interaction. Consistent with previous research (Kahalon et al., 2018) participants' preexisting math ability (psychometric score) was used as a covariate, to isolate the unique effect of the experimental manipulation on their math performance. As seen in Table 2, consistent with previous research (e.g., Steinberg, Okun, & Aiken, 2012), preexisting math ability and math identification predicted better performance. Importantly, participants' math performance in the communality condition was significantly worse than in the control condition. The Condition × Math Identification interaction was nonsignificant.[4]

[3] The test remained significant even when the 3-item scale was used, $t(96) = -2.23$, $p = .028$.

[4] The main effect of condition remained significant, $B = -1.83$, $t = -2.82$, $p = .006$, such that participants' math performance in the experimental condition ($M = 7.70$, $SD = 3.14$) was significantly worse than in the control condition ($M = 9.25$, $SD = 3.82$), even without using pre-existing math ability as a covariate.

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Table 1. Descriptive statistics and correlations for Study 1 variables

		Communality condition	Control condition						
	Range	*M* (*SD*)	*M* (*SD*)	1	2	3	4	5	6
1. Threat appraisal	1.00–6.50	1.90 (1.42)	1.39 (1.07)	–					
2. Test motivation	2.50–5.00	3.81 (0.66)	3.96 (0.53)	−.06	–				
3. Math performance	1.00–18.00	7.70 (3.14)	9.25 (3.82)	.03	.29**	–			
4. Agentic goals	−1.75–1.00	0.14 (0.67)	0.10 (0.71)	−.05	−.01	.03	–		
5. Communal goals	−1.12–2.98	1.25 (0.72)	1.31 (0.69)	−.02	.71	−.08	.21*	–	
6. Math identification	1.00–5.00	3.90 (0.85)	3.65 (0.90)	−.01	.06	.28**	.10	−.07	–

Notes. N = 108 female participants. For threat appraisal: the scale ranged from 1 to 7. For test motivation and math identification: the scales ranged from 1 to 5. For agentic and communal goals: the scales ranged from −4 to 4. For math performance: the scale ranged from 0 to 30. *$p < .05$; **$p < .01$.

Table 2. Results of regression analysis on performance in the Math Test.

						95% CI	
	B	*SE*	β	*t*	*p*	LL	UL
Intercept	−2.727	2.351		−1.160	.249	−7.390	1.937
Preexisting Math Ability	0.018	0.003	.426	5.231	.000	0.011	0.025
Exposure to Communality Stereotype (vs. control)	−1.960	0.581	−.276	−3.374	.001	−3.112	−0.808
Math Identification	1.469	0.435	.381	3.377	.001	0.606	2.332
Exposure to Communality Stereotype × Math Identification	−0.826	0.632	−.147	−1.307	.194	−2.082	0.428

Notes. N = 108 female participants. R^2 = .33, $F_{change}(4, 102) = 12.68$, $p < .001$.

Interpersonal Goals

We conducted two regression analyses: one with participants' communal goals as the outcome variable and one with agentic goals as the outcome. The predictors in both models were the experimental condition, math identification, and their interaction. For the communion vector, none of the main effects nor the interaction were significant, *p*s > .564. For the agency vector, the effects of condition and math identification were non-significant, *p*s > .306, whereas their interaction, illustrated in Figure 1, was significant, β = .36, $t = 2.52$, $p = .013$. The region of significance, calculated using Preacher, Curran, and Bauer's (2006) online calculator, was $Z_{identification} > 1.07$, indicating that for participants whose standardized level of math identification was higher than 1.07 exposure to the communality stereotype significantly increased the pursuit of agentic goals, and $Z_{identification} < -1.28$, indicating that, for participants whose level of math identification was lower than −1.28, exposure to the communality stereotype significantly decreased the pursuit of agentic goals.

Discussion

Study 1 revealed that the math performance of female participants who were exposed to the positive stereotype about women's communality was worse than that of participants in the control/no-stereotype condition. This result cannot be explained by preexisting differences in participants' math ability, which were controlled for. Whereas in previous research of Kahalon et al. (2018) the negative effect of exposure to the communality stereotype emerged only among participants with high domain identification, Study 1 revealed an even more robust effect, which did not depend on participants' level of math identification. This result underscores the potentially adverse consequences for women of exposure to the seemingly positive stereotype about their "innate" warmth and communal nature.

In line with the assumption that the performance decrements were caused by stereotype threat, the manipulation check revealed that participants had a higher threat appraisal in the experimental compared to the control condition. Moreover, exposure to the communality stereotype did not affect participants' motivation to do well in the test. Stereotype threat is known to occur without a decrease in participants' wish and effort to succeed (Jamieson & Harkins, 2007; Keller, 2007), that is, it is not a motivational phenomenon. Therefore, this finding is consistent with stereotype threat as underlying the observed performance decrements – ruling out de-motivation as an alternative explanation.

The pattern of results for the interpersonal goals, as observed in the analysis of regions of significance, suggests that exposure to the communality stereotype led to an increase in pursuit of agentic goals among women with relatively high math identification, and to a decrease among

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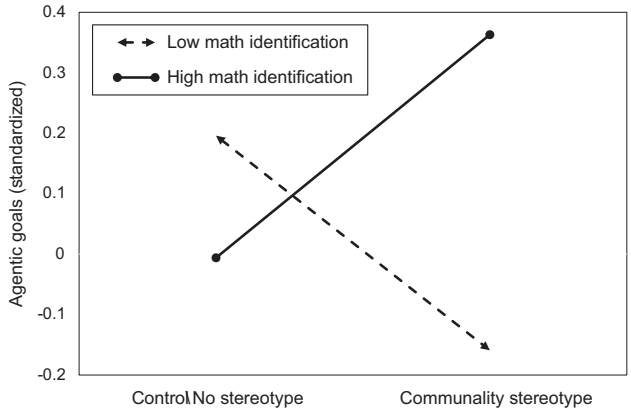

Figure 1. The effect of exposure to the communality stereotype (Study 1, N = 108) on the pursuit of agentic goals among women with high math identification (+1 *SD* above average), B = 0.37 (SE = 0.19), t = 1.92, p = .057, versus low math identification (−1 *SD* below average), B = −0.36 (SE = 0.20), t = −1.80, p = .076.

women with relatively low identification. The results for participants high in math identification can be interpreted as stemming from stereotype reactance (Kray et al., 2001). The pattern of results for women with low domain identification was not predicted a priori and therefore should be treated cautiously. If replicated in future research, it would suggest that the communality stereotype reinforces traditional gender roles not only through impairing women's math performance but also by leading some women to behave more submissively.

Study 2

Study 2 examined the effects of exposure to a reminder of the positive stereotype about men's agency on men's performance in a test of socio-emotional abilities; their motivation to do well in this test; and their pursuit of agentic and communal interpersonal goals. We expected that exposure to the positive stereotype about men's agency would (a) impair men's performance in a test of socio-emotional abilities – especially among those high in domain identification, and (b) have no effect on participants' test motivation. We also explored whether exposure to the agency stereotype would increase men's pursuit of agentic interpersonal goals, in alignment with the prescriptive positive stereotype about their gender, and/or increase their pursuit of communal interpersonal goals, in an attempt to counteract the negative stereotype about men's communality.

Before we move on to describe the method and results, for the sake of transparency in reporting, we disclose that we ran Study 2 twice. In the first time, to facilitate data collection, we ran an on online study, which was preregistered along with Study 1 (https://osf.io/pa9j5). The procedure was identical to that specified below with one exception – participants (N = 132) completed the study in their homes rather than in the laboratory. This study failed to replicate Kahalon et al.'s (2018) findings: the effects of exposure to the agency stereotype, domain identification, and their two-way interaction on participants' performance in the emotion recognition test were nonsignificant, ps > .196 (detailed description and dataset are available in https://osf.io/hktz3). The manipulation check indicated that participants in both the experimental and the control conditions had similar levels of threat appraisal, Ms = 1.72 and 1.70, t = 0.13, p = .896, suggesting that we failed to induce stereotype threat in the positive stereotype condition. This finding is consistent with previous reports that stereotype threat effects are difficult to induce online (Finnigan & Corker, 2016). Hence, we decided to run Study 2 again, this time in the laboratory. We preregistered this study through the OSF https://osf.io/uj5av.

Method

Participants

Based on the effect size observed by Kahalon et al. (2018), a power analysis using G*Power calculator revealed that 137 participants were needed to detect the small to medium effect size (f^2 = .09) at a significance of 5% and power of 80%. We managed to recruit 136 male participants through advertisements placed around the campus and in social networks. Students were compensated by either course credit or money (30 NIS). Seven outliers were excluded based on the preregistered decision to exclude observations whose Cook's (1977) distance is greater than $4/n$ (Bollen & Jackman, 1990). The final sample comprised of 129 participants,[5] M_{age} = 25.20 (SD = 3.53). A sensitivity analysis (Faul et al., 2009) for a sample of 129 participants and power of 80% at α = .05, revealed that for a single predictor, in a multiple regression analysis, effects above R^2 = .058 will be reliably detected. The effect for the interaction in Study 2 was R^2 = .050. All participants were Israeli men; their native tongue was Hebrew (98.5%) or other (1.5%). Most participants (86%) described themselves as heterosexual, and the rest as homosexual (10%), bisexual (2%), or not wanting to indicate their sexual orientation (2%).

[5] When all 136 participants were included in the analysis, the key interaction between the condition and domain identification on test performance remained significant (B = −1.10, t = −2.52, p = .026). For motivation and agentic goals, all the effects remained nonsignificant, ps > .255. The main effect for communal goals became marginal (B = −0.20, t = −1.67, p = .098).

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Table 3. Descriptive statistics and correlations for Study 2 variables

		Agency condition	Control condition						
	Range	*M* (*SD*)	*M* (*SD*)	1.	2.	3.	4.	5.	6.
1. Threat appraisal	1.00–4.67	2.12 (0.95)	1.66 (0.76)	–					
2. Test motivation	1.80–5.00	4.02 (0.71)	4.08 (0.61)	−.04	–				
3. Emotion recognition test performance	11.00–23.00	17.58 (2.52)	17.82 (2.32)	−.12	.03	–			
4. Agentic goals	−1.25–1.94	0.31 (0.55)	0.32 (0.61)	−.25**	.10	.17*	–		
5. Communal goals	−1.56–2.68	0.93 (0.76)	1.17 (0.69)	−.20*	.07	.19*	.05	–	
6. Domain identification	1.50–5.00	4.09 (0.72)	4.21 (0.57)	−.06	.27**	−.02	.07	.25**	–

Notes. N = 129 male participants. For threat appraisal: the scale ranged from 1 to 7. For test motivation and domain identification: the scales ranged from 1 to 5. For agentic and communal goals: the scales ranged from −4 to 4. For the emotion recognition test (DANVA2) performance: the scale ranged from 0 to 24. *p < .05; **p < .01.

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Most of them (98%) were students, majoring in different disciplines (e.g., psychology, exact sciences, law).

Procedure and Materials

The procedure generally matched that of Study 1. Participants first completed a demographic questionnaire, which included the 4-item 5-point measure of their domain identification, adapted from Smith and White (2001) to the domain of socio-emotional abilities (e.g., "It is highly likely that my future career will involve understanding the feelings and expressions of other people"; α = .69).

Next, ostensibly presented as a verbal ability task, participants read the short texts that constituted the experimental manipulation (see Appendix B in the OSF deposit https://osf.io/hktz3/). The control condition was identical to that used in Study 1. The agency-stereotype condition was similar to the communality condition used in Study 1, except that it discussed the square symbol in the Bender-Gestalt test, which represents the qualities of masculinity. For example, it was explained that the angled shape of the square represents men's typical traits – such as assertiveness, strength, and ambitiousness – which makes them especially suitable for leadership positions.

As the primary outcome variable, participants completed the Adult Facial Expressions subtest from the Diagnostic Analysis of Nonverbal Accuracy 2 (DANVA2; Nowicki & Duke, 1994), a well-established measure of the ability to apprehend others' emotions, which is a key component of socio-emotional intelligence (Mayer, Salovey, & Caruso, 2004). The DANVA2 examines emotion recognition and consists of 24 facial photographs. For each photograph, participants are asked to choose the correct feeling expressed in it, out of four options. Participants earned one point for each correct answer.

Next, participants completed the measures of test motivation, α = .76 (one item was excluded due to low reliability, α = .67 for the 6-item scale); the CSIV (the 32-item version), and threat appraisal, α = .52. Testing the circumplex structure of the CSIV using multidimensional scaling revealed that the data mapped into two different dimensions (stress = .036), allowing us to calculate the Agentic and Communal vectors.

Results

Descriptive statistics and correlations for all variables are presented in Table 3.

Threat Appraisal

A *t*-test for independent samples revealed that, as intended, participants reported experiencing a higher level of stereotype threat in the experimental compared to the control condition, $t(127) = 3.04$, $p = .003$.

Test Motivation

A regression analysis was conducted, in which the experimental condition, domain identification and their two-way interaction were the predictors. As expected, none of the effects or interactions reached significance, ps > .148. Using Bayesian statistic to compare the fit of the data under the null hypothesis compared to the alternative hypothesis, suggested that the estimation of the Bayes factor for the experimental condition was in favor of the null hypothesis, $BF_{01} = 4.75$, yet the estimation of the Bayes factor for the interaction between condition and domain identification was close to 1, indicating data insensitivity (support for neither hypothesis; Dienes, 2014), $BF_{01} = .73$.

Emotion Recognition Test Performance

We conducted a regression analysis in which condition, domain identification and their two-way interaction were the predictors. As seen in Table 4, domain identification marginally predicted better performance. Consistent with Kahalon et al.'s (2018) findings, the effect of the experimental condition was not significant, while the Condition × Math Identification interaction reached significance. The region of significance was $Z_{identification} > .74$, indicating that for participants whose standardized level of domain

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Table 4. Results of regression analysis on performance in the Emotion Recognition Test

						95% CI	
	B	*SE*	β	*t*	*p*	LL	UL
Intercept	17.69	.30		58.38	.000	17.09	18.29
Exposure to agency stereotype (vs. control)	−0.10	.42	−.02	−0.23	.815	−0.94	0.74
Domain identification	0.71	.39	.26	1.83	.069	−0.06	1.48
Exposure to agency stereotype × Domain identification	−1.26	.50	−.35	−2.53	.013	−2.24	−0.28

Notes. N = 129 male participants. R^2 = .05, $F_{change}(3, 125) = 2.27$, p = .084.

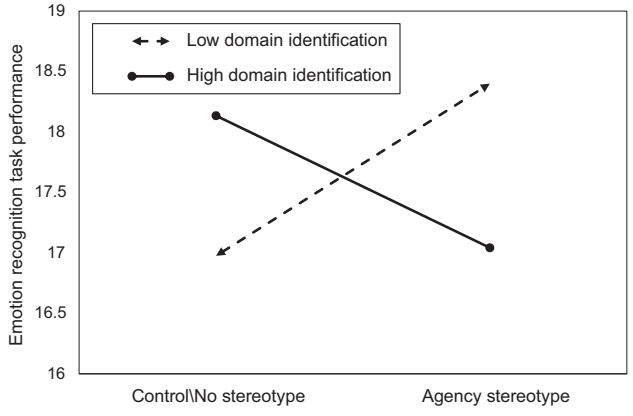

Figure 2. The effect of exposure to the agency stereotype (Study 2, N = 129) on performance in an emotion recognition test among men with high domain identification (+1 *SD* above average), B = −1.36 (*SE* = 0.61), t = −2.24, p = .027, versus low domain identification (−1 *SD* below average), B = 1.16 (*SE* = 0.70), t = 1.66, p = .100.

identification was higher than .74, exposure to the agency stereotype led to significantly worse test performance, and $Z_{identification} < -1.52$, indicating that for participants whose level of domain identification was lower than −1.52, exposure to the agency stereotype led to significantly improved test performance. Figure 2 shows the obtained pattern of results.

Interpersonal Goals

We conducted two regression analyses, such that the outcome variable was participants' communal goals in the first analysis, and agentic goals in the second. The predictors in both models were condition, domain identification, and their interaction. For the communion vector, the effect of the experimental condition was marginal, B = −0.22, β = −.13, t = −1.77, p = .079, such that participants in the agency-stereotype condition reported pursuing marginally less communal goals than in the control condition (see Table 3). The effects of domain identification and the Identification × Condition interaction were not significant, βs < .15, ps > .269. For the agency vector, neither the model, $F(3, 125) = 0.23$, p = .879, nor any of the effects, ps > .505, reached significance.

Discussion

Replicating previous findings, Study 2 revealed that when male participants were exposed to the positive stereotype about men's agency in the laboratory (but, admittedly, not online), they were induced with stereotype threat, leading to impaired performance in an emotion recognition test among those with high domain identification (i.e., men who care about their socio-emotional abilities). In addition, exposure the agency stereotype did not result in decreased test motivation, thus supporting stereotype threat, rather than demotivation, as the reason for the observed decrements in the emotion recognition test.

Exposure to the agency stereotype did not influence participants' pursuit of agentic goals, yet there was a marginal decrease in their pursuit of communal goals. If replicates in future studies, this means that exposure to the positive stereotype about men's agency reinforces traditional gender roles not only by undermining (some) men's socio-emotional performance, but also by decreasing men's efforts to be nice and cooperative – traditionally feminine traits – in interpersonal interactions.

General Discussion

Can positive stereotypes have detrimental effects? Yes, they can. Replicating Kahalon et al.'s (2018) findings, women who were exposed to the positive communality stereotype about their gender had worse math performance compared to women in the control/no-stereotype condition, and men who were exposed to the positive agency stereotype about their gender had worse performance in an emotion recognition test compared to men in the control/no-stereotype condition. For men, this effect was evident among participants with high identification with the socio-emotional domain, while for women, this effect was even more robust – occurring for all participants regardless of their level of math identification. In both studies, exposure to these positive stereotypes increased participants' threat appraisal, with no conclusive evidence for a reduction in their motivation to perform well. These findings establish stereotype

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threat as the cause underlying the observed performance decrements.

Our results point to the importance of considering how subtle social psychological mechanisms, such as positive gender stereotypes, perpetuate gender inequality. Such subtle mechanisms are often harder to recognize than more overtly hostile mechanisms (Barreto & Ellemers, 2005), yet they have far reaching implications. Identifying the subtle barriers for women in STEM is important for both STEM fields, which are losing out potential talented workers and innovators, as well as for women, as STEM jobs offer higher salaries than non-STEM jobs (Langdon, McKittrick, Beede, Khan, & Doms, 2011). The same is true for men who are more communally oriented. These men can benefit from entering HEED fields, as it can improve their well-being (Le, Impett, Kogan, Webster, & Cheng, 2013; Sheldon & Cooper, 2008), and greater gender diversity in HEED fields could benefit society as a whole (Croft, Schmader, & Block, 2015).

Besides performance in counter-stereotypical domains, we also explored whether exposure to positive gender stereotypes affect women's and men's goals in interpersonal interactions. Perhaps surprisingly, women did not increase their pursuit of communal goals when exposed to the communality stereotype. That is, they did not follow the prescription that they should be "nice" and caring for others. Moreover, among women with relatively high math identification, exposure to the communality stereotype led to an increase in pursuit of agentic goals. Possibly, this finding suggests that women who highly identify with a counter-stereotypical domain try to counteract not only negative (Kray et al., 2001) but also positive gender stereotypes – which subjugate women in a seemingly benevolent way, by putting them on a pedestal (Glick & Fiske, 2001). Notably, although women's pursuit of agentic goals can ultimately challenge traditional gender roles, it comes with a personal cost, as women are often socially penalized for pursuing agency (Phelan, Moss-Racusin, & Rudman, 2008).

Among men, corresponding to the findings among women, we did not find evidence that exposure to the agency stereotype led to alignment with the prescription that they should be strong and assertive. However, compared to men in the control condition, men in the agency stereotype condition reported marginally less communal goals – which are traditionally associated with femininity. Besides examining whether this unexpected result replicates, future research may further explore if exposure to the agency stereotype leads men to detach themselves from other things that are associated with womanhood, such as feminine behaviors (e.g., caring and nurturing of the environment; Brough, Wilkie, Ma, Isaac, & Gal, 2016), feminine men (Glick, Gangl, Gibb, Klumpner, & Weinberg, 2007), and products associated with women (White & Dahl, 2006).

Limitations

The main limitation of the present research is that it did not include a comparison group of male participants in Study 1, and female participants in Study 2. We decided not to include these comparison groups because there is no theoretical ground to assume that exposure to the stereotype about women's communality would affect men's math performance, or that exposure to the stereotype about men's agency would affect women's socio-emotional performance. If anything, such exposure can lead to a stereotype lift (Walton & Cohen, 2003) – a performance boost that occurs due to a downward comparison to a stigmatized outgroup. Still, we admit that without the comparison to the non-stigmatized gender in both studies we cannot establish with full confidence that the effects of exposure to the communality and agency stereotypes are indeed unique to women and men (respectively).

Another issue that should be taken in consideration is that we observed stereotype threat effects in the studies conducted in a laboratory setting, but not in the online study. Future research should systematically examine our assumption that stereotype threat effects are more difficult to induce online than in the laboratory, by randomly assigning participants to complete on online versus a laboratory study. Such examination is especially important due to the rapid growth in Internet studies, and the reported consistency of Internet studies with findings obtained using traditional methods (Gosling, Vazire, Srivastava, & John, 2004). It can also point to a potential route for reducing stereotype threat effects; namely, by letting examinees take online tests when possible.

Implication and Future Directions

The present research provides evidence for the idea that not only negative stereotypes, but also positive stereotypes, have the potential to reinforce gender inequality as they might prevent women and men to use their full potential in non-stereotypical domains. Still, our findings also suggest that some women try to counteract these stereotypes, perhaps because they are intuitively aware of their implied negativity (Siy & Cheryan, 2016). Future research could examine the effectiveness of women's strategies for coping with positive stereotypes, and identify means in which the backlash against women's agentic behavior (O'neill & O'Reilly, 2011) can be minimized.

Our results are also relevant for interventions using group affirmation techniques (emphasizing positive ingroup dimensions), which were found to be helpful in improving stereotyped groups' well-being, motivation and performance (Derks, Van Laar, & Ellemers, 2009; Van Laar, Derks, & Ellemers, 2013). Practitioners who use such

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interventions should be careful not to affirm group members' positive identity through the use of positive stereotypes, in order not to arouse the negative outcomes of stereotype threat or undesirable changes in group members' goal pursuit (as we found among men).

Future research is also needed in order to examine the effects of positive stereotype on participants from collectivistic countries. Because collectivistic cultures place a greater focus on relationships, which in turn undermines universal stereotyping principles (Cuddy et al., 2009), it is possible that they will be less affected by positive stereotypes (in compare to participants from individualistic countries).

Finally, as positive stereotypes are relevant to other groups as well, future research could examine the effects of positive stereotypes on racial, ethnic or religious groups. For example, it could test whether exposure to the positive stereotype about Blacks' natural athletic ability (Stone, 2002) impairs Black participants' performance in intelligence tests. Extending our understanding of the effects of positive stereotypes and the way in which people react to them, is critical for eradicating intergroup achievement gaps and restriction to social roles.

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History
Received March 4, 2019
Revision received May 28, 2019
Accepted May 28, 2019
Published online August 20, 2019

Acknowledgments
We hereby declare that we have no potential conflicts of interest with respect to the authorship or the publication of this article.

Authorship
All authors were involved in all parts of the research.

Open Data
We preregistered these studies through the OSF https://osf.io/uj5av, https://osf.io/pa9j5, and https://osf.io/hktz3.

ORCID
Rotem Kahalon
https://orcid.org/0000-0002-4718-6119

Rotem Kahalon
The School of Psychological Sciences
Tel-Aviv University
Ramat Aviv
Tel-Aviv 69978
Israel
rotemkah@mail.tau.ac.il

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Original Article

Perpetrator Religion and Perceiver's Political Ideology Affect Processing and Communication of Media Reports of Violence

Samia Habib[1,2], Levi Adelman[1,3], Bernhard Leidner[1], Shaheen Pasha[4], and Razvan Sibii[1]

[1]Department of Psychological and Brain Sciences, University of Massachusetts, Amherst, MA, USA
[2]Department of Epidemiology, Boston University, MA, USA
[3]Ercomer, Faculty of Social and Behavioral Sciences, Utrecht University, The Netherlands
[4]Journalism Department, University of Massachusetts, Amherst, MA, USA

Abstract: People's interpretations of media reports about crimes may be biased by their motivations to construct and protect their worldviews and, relatedly, by criminals' group membership. Two large-scale experiments (*N*s = 248 and 1,115) investigated how American adults interpret reports of crimes committed by either a Christian or Muslim, and how these interpretations depend on political ideology. Results show liberals attributing crimes more to religion for Christian rather than Muslim offenders, with the opposite effect for conservatives. Importantly, these biases also influenced how people communicated the news report to others. Additionally, evidence suggests that attitudes toward Islam and not toward Muslims may explain these effects. Implications for how political ideology affects interpretation and communication of media portrayals of Muslims are discussed.

Keywords: media, political ideology, motivated reasoning, religion

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In recent years, there has been a series of high-profile mass-casualty attacks across the Western world. In the wake of these tragedies, media outlets and everyday citizens try to understand why these attacks happened, often in the absence of sufficient information. For example, a shooting committed by two Muslims in San Bernardino, California, was quickly attributed to terrorism (Ifill, 2015), whereas a shooting by a Caucasian man in Las Vegas was quickly ruled to have not been an act of terrorism (Weaver & Lartley, 2017). As with many real-world examples taken as anecdotal evidence, these cases differ on many levels. At the same time, they illustrate the possibility that members of the media, and people more broadly, may attribute criminal acts by Muslims to terrorism, while they attribute similar acts by Caucasians either to mental health or forgo an attribution altogether until more evidence can be gathered. Thus, people exposed to the same information can draw wildly diverging conclusions, with important downstream consequences for individual behavior and national policy.

The problem of biased attributions and processing is exacerbated as people tend to encounter and communicate news from within their social circles and selected niche media sources (Mitchell, Gottfried, Barthel, & Shearer, 2016), raising the risk of media "echo-chambers" dividing segments of the population exposed to disparate sources of information (e.g., Barberá, Jost, Nagler, Tucker, & Bonneau, 2015; Van Aelst et al., 2017). The consequences of such social divisions in processing media reports can have severe consequences not only for broader society, but also for the subjects of these media reports and members of their groups – especially when the subjects belong to minority groups. Therefore, it is important to study why these differences in perception and portrayal of similar actions by different social groups occur. Thus, this research seeks to better understand how people's biases, and especially people's political worldviews, shape the processing, interpretation, and communication of news stories about crimes committed by majority and minority perpetrators, respectively. Furthermore, this research investigates some

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competing arguments about the attitudes and beliefs that might help us understand why political worldviews might affect reactions to news reports of crimes committed by majority versus minority perpetrators.

Bias in the Media

Media are not just consumed passively, but they are engaged with, construed and interpreted actively. Many people are simultaneously consuming news themselves and reproducing news for others, and any biases they hold can affect this process. Indeed, research has found that when a crime is attributed to the race of the perpetrator, the outgroup minority members were seen as more dangerous than ingroup members who committed an identical crime (Chen, Purdie-Vaughns, Phelan, Yu, & Yang, 2015). This attribution bias can have consequences for blame and punishment: a perpetrator labeled as a religious terrorist is more likely to be punished than a perpetrator labeled as mentally ill, because mental illness is associated with lack of control and intentionality (Hughes & Trafimow, 2015), thus evoking more sympathy toward the perpetrator (Noor, Kteily, Siem, & Mazziotta, 2019). Research has further shown that people consider whiter faces to be more likely to have committed an act of terrorism due to mental health rather than ideological reasons, and therefore assign them less guilt (Kunst, Myhren, & Onyeador, 2018). Thus, if people blame crimes by Caucasians on mental health, they may then determine that the crime was likely less intentional because of reduced control. In contrast, if people blame crimes by minority members on religion or culture, they may be more likely to see these crimes as intentional. Once people ascribe greater intentionality to crimes by minority members, they may then assign harsher judgments and punishments to the minority actors (Ames & Fiske, 2015).

Media biases can have other downstream consequences. If the media's negative portrayal of a certain group generates negative impressions of that group, whether explicitly or implicitly (e.g., Dovidio, Kawakami, & Gaertner, 2002), these associations could increase prejudicial responses (Persson & Musher-Eizenman, 2005). Therefore, biases in the production of news media do not only harm the factual content of the news, but they might also affect media consumers' perceptions and treatment of the social group of the perpetrator (see also Hoffman & Wallach, 2007).

The unequal representation of minority versus majority group members in the media is especially an issue for Muslims. American media reports about Muslims are often very negative because Americans relate Islam with the 9/11 attacks committed by Islamic extremists (Persson & Musher-Eizenman, 2005). Furthermore, news specifically related to terrorism can increase death-related thoughts, which in turn can increase prejudice (Das, Bushman, Bezemer, Kerkhof, & Vermeulen, 2009). Therefore, an increased emphasis on Islam and terrorism in news reports can increase prejudice toward Muslims, whereas a reduced emphasis can decrease prejudice toward Muslims (Persson & Musher-Eizenman, 2005). The present research will investigate how motivated reasoning grounded in political ideology produces such biased perception and reproduction of the news. Specifically, we aimed to test how liberals and conservatives, respectively, interpret crimes committed by minority Muslim and majority non-Muslim actors, and subsequently communicate these crimes to others.

Political Ideology as Motivated Reasoning

One potential source of bias in how people process and communicate news is their motivated reasoning: their unconscious motivations that affect their reasoning, attitudes, and behavior (e.g., Kunda & Sinclair, 1999). People are generally motivated to only focus on evidence that supports their point and disregard much else (Epley & Gilovich, 2016). Importantly, motivated reasoning can lead to biases and stereotypes toward people and events (Kunda, 1990). Thus, we hypothesized that, based on their worldview and ideological belief system, people would exhibit biased interpretation and attribution styles when processing as well as communicating crimes committed by minority Muslim or majority non-Muslim actors.

One prominent ideology that affects how people view the world is their political ideology (Kiley, 2017). Specifically, people's political worldviews may lead them to process information in the world around them in a way that matches their worldview (Jost, Glaser, Kruglanski, & Sulloway, 2003). Thus, people's liberal or conservative ideologies shape their goals and beliefs, which in turn affect their unconscious motivations. Indeed, some research characterizes conservatives as high inconscientiousness and rigidity, desiring orderliness, and liberals as more open to new experiences, desiring novelty and diversity (Carney, Jost, Gosling, & Potter, 2008). These differences in beliefs and values can lead conservatives and liberals to respond differently in the face of threat, fear, or uncertainty, with conservatives becoming more resistant to change and endorsing inequality (Jost et al., 2003), and liberals becoming more motivated by values of openness and diversity to support equality and social change (Carney et al., 2008).

Importantly, however, both conservatives *and* liberals exhibit biases in motivated reasoning (Kahan, 2013; Kahan, Jenkins-Smith, & Braman, 2011). A recent meta-analysis found that liberals and conservatives show equal amounts of partisan bias (Ditto et al., 2017; see also Crawford, Kay, & Duke, 2015). Indeed, recent research suggests that people adapt attributions of terrorism or mental health to

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criminal actions as a function of their political attitudes (Noor et al., 2019). Across multiple studies, Noor and colleagues (2019) found that people on both sides of the political spectrum interpreted the motivations of criminal actions in ways that protected their desired worldview and group image. Thus, initial findings suggest that political ideology may affect how a person interprets and portrays a crime depending on the race or religion of the perpetrator.

However, the past research leaves a few questions unanswered. First, it is not yet clear whether this bias is mainly or exclusively driven by conservatism (e.g., Jost et al., 2003), or also/equally by liberalism (e.g., Ditto et al., 2017). One perspective is that political conservatism, as an ideology that encompasses intergroup biases which promote ingroup superiority and ingroup dominance, might be expected to lead to situations in which liberals treat members of all groups identically while conservatives demonize and penalize outgroups (Hodson & Busseri, 2012; Jost et al., 2003; Luguri, Napier, & Dovidio, 2012). Alternatively, it may be that both liberals and conservatives are equally motivated to interpret world events in ways that protect their perspective on how the world should work. For liberals who desire and defend diversity (Carney et al., 2008), they may be especially motivated to support minority groups and try to focus on good actions and ignore or deny negative ones by those groups (Adelman, Yogeeswaran, & Lickel, 2019) while being relatively more intolerant toward majority group members (Brandt, Reyna, Chambers, Crawford, & Wetherell, 2014).

Second, while the importance of political ideology seems clear, it is less clear why political ideology would affect judgments about members of majority and minority groups. One perspective is that since political ideology reflects a wide-ranging worldview which includes attitudes toward other groups, liberals might judge a Muslim perpetrator equally or more positively than a Christian perpetrator because they hold more positive attitudes toward Muslims than Christians (Brandt et al., 2014). By contrast, conservatives who hold relatively more negative attitudes toward Muslims than non-Muslims, would judge Muslim perpetrators more negatively than Christian perpetrators. An alternative perspective is that the way people judge Muslim and non-Muslim perpetrators may not be due to attitudes toward the group, but might instead reflect beliefs they have about the content of the religion that might make them more or less likely to interpret that religion as being the source of violent behaviors. This approach reflects research suggesting that stereotypes, which are often perceived as a negative bias, may instead reflect relatively accurate judgments about groups (Jussim, Crawford, & Rubinstein, 2015) or principled observations about other groups (e.g., Sniderman, Tetlock, Glaser, Green, & Hout, 1989). Indeed, people on both sides of the political aisle can argue that their interpretation is the most probable interpretation, and that it is not influenced by bias. Therefore, to better understand the source and nature of biases grounded in political ideology, two large-scale experiments investigated how people with varying political ideologies interpret crimes committed by members of a stigmatized minority group (Muslims) compared to crimes committed by members of a less or non-stigmatized group (Christians).

Hypotheses

Our aim was to understand if political ideologies affect how people perceive and report a crime depending on the religion of the perpetrator. Specifically, we hypothesized that people's political ideology and the offender's group membership jointly affect how much people attribute the crime to religion (as a group-level factor) or mental health (as an individual factor). Furthermore, we proposed that political ideologies would bias people on both the right *and* the left to interpret information in ways consistent with their worldviews. We thus expected relatively conservative people to be more likely to conclude that a crime committed by a Muslim (rather than Christian) perpetrator was primarily due to religion, while concluding that a crime committed by a Christian (rather than Muslim) perpetrator was primarily due to mental health problems. Conversely, we expected relatively liberal people to attribute a crime committed by a Christian (rather than Muslim) perpetrator was primarily due to religion, while attributing a crime committed by a Muslim (rather than Christian) perpetrator was primarily due to mental health problems. We also tested two competing explanations for why political ideology might affect judgments of Muslim versus Christian perpetrators. Specifically, we included measures of attitudes toward Muslims and attitudes toward Islam to test whether the effects are explained by negative attitudes toward a group of people, or negative attitudes toward a religion or ideology.

For both experiments, we report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (Simmons, Nelson, & Simonsohn, 2011). Due to space limitations, reports of some outcome measures can be found in the supplemental file. All data were collected prior to any data analysis.

Experiment 1

Experiment 1 tested our hypotheses in a convenience sample of psychology and journalism undergraduate students, using a between-subject design in which participants were randomly assigned to read about either a Christian or a Muslim perpetrator.

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Method

Participants

We recruited 248 participants from a large university to participate in this study. Of these participants, 48 were recruited specifically from journalism courses and an additional 200 were recruited from the general student population accessible via the psychology subject pool. Participants completed the 30-minute study for course credit or $10. The survey was completed on paper in classrooms or in research labs. All participants were included in the analyses. Participants were mostly female (76.0% female; 23.6% male; 0.4% non-identified), between 17 and 31 years old (M = 20.24, SD = 1.79), and on average significantly left of the political center (1 = *very liberal*, 6 = *very conservative*; M = 2.63, SD = 1.05, $t(240) = 38.77$, $p < .001$)].

Measures

Experimental Manipulation

Participants were randomly assigned to read a crime scenario with either a Muslim (Ahmed Yusuf) or Christian (Matthew Clark) perpetrator, which described a shooting at a university that injured 13 students, and noted the religion, mental health, social life, and childhood of the perpetrator. The scenario was identical across conditions except for the name and religious identifiers of the offender (see Electronic Supplementary Material, ESM 1).

Interpretation of the Crime Scenario

We included a number of measures to understand how participants interpreted the crime they read about.

To compare different attributions for the crime, we measured to what extent participants attributed the crime to the offender's religion, mental health, social life, and difficult childhood on a scale from 1 (= *completely disagree*) to 6 (= *completely agree*). Four items (α = .80) also measured, on a scale from 1 (= *none*) to 6 (= *very much*), to what extent participants believed the offender was feeling guilt, shame, pain, and pleasure (reverse-scored). To measure intentionality, four items (α = .87) measured to what extent participants believed the offender had intended the crime (e.g., "How much do you think the incident was intentional?"), on a scale from 1 (= *none*) to 6 (= *very much*). Finally, five items measured, on a scale from 1 (= *extremely unlikely*) to 6 (= *extremely likely*), how likely participants believed it to be that the offender had been targeting a specific group as a "hate crime" (e.g., "members of a specific race or religion"). However, the items did not form a reliable scale and were not analyzed.

News Production Based on the Crime Report

We next asked participants a series of questions designed to understand how they would communicate the news to others.

After reading the scenario, participants wrote a minimum 200-word news report about the incident, which we analyzed based on the use of keywords related to religion and mental health. We also assessed which of the four details about the perpetrator (i.e., religion, mental health, social life, and childhood) participants would prioritize in their reports using a rank-order measure. On a scale from 1 (= *none*) to 6 (= *a lot*), participants rated four items measuring how much time they would spend investigating the four different aspects of the offender's life mentioned in the report (religion, mental health, social life, and difficult childhood). Lastly, participants were asked to rate, on a scale from 1 (= *extremely unlikely*) to 6 (= *extremely likely*), how likely they would be to use each of seven headlines for the article they had written (e.g., "Student critically injures 3 people during a shooting"; "3 people injured during a possible [Christian/Muslim] terrorist attack").

Process Variables

We measured political ideology as a moderator using four statements (α = .90) measured, on a continuous scale from 1 (= *extremely liberal*) to 6 (= *extremely conservative*), participants' political ideology (e.g., "Regarding economic issues [e.g., taxation, public spending], I am. . ."). We also included potential mediators using statements to measure attitudes toward Muslims, Islam, Arabs, and the portrayal of Muslims in the media (e.g., "Arabs are a threat for America"; "I would like to be friends with Muslims"; "Islam is radical and intolerant"; "People believe that the negative media portrayal of Arabs is justified"). These were all measured on a scale from 1 (= *completely disagree*) to 6 (= *completely agree*).

Results

We analyzed the main and interaction effects of the offender's religious group membership and participants' political ideology on the dependent variables using mixed analysis and moderated regression analysis. Due to space constraints, we only report main effects that reached significance; additionally, we only report the results for a subset of the dependent variables. Details and results from the other variables can be found in the supplemental file.

Interpretations of the Crime Scenarios

Attributions for the Crime

To examine whether participants attributed the crime to the offender's religion or mental health, we conducted a mixed analysis with offender religion and political ideology as between-subject factors, and participants' attributions of

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the crime to either religion or mental health as a two-level within-subject factor.[1] The three-way interaction was significant, $F(1, 236) = 15.14$, $p < .001$, $\eta_p^2 = .060$, indicating that the interaction between perpetrator religion and political ideology differed for attributions of the crime to mental health versus religion. Therefore, we tested the two-way interaction effects of perpetrator religion and political ideology on attributions of the crime to religion and on attributions of the crime to mental health separately. The interaction between perpetrator religion and political ideology was significant for attributions to religion, $F(1, 236) = 12.00$, $p < .001$, $\eta_p^2 = .048$, such that increasing liberal (and decreasing conservative) attitudes predicted marginally greater attributions of the crime to religion when the offender had been described as Christian, $B = -.14$, $SE = .08$, $t(236) = -1.90$, $p = .058$, but significantly weaker attributions of the crime to religion when the offender had been described as Muslim, $B = .23$, $SE = .08$, $t(236) = 2.98$, $p = .003$. As Figure 1 shows, this resulted in liberals (−1 *SD* on political ideology) attributing the crime to a lesser extent to religion when the offender had been described as Muslim ($M = 2.01$) rather than Christian ($M = 2.83$), $t(236) = 5.31$, $p < .001$, $d = 0.69$. In contrast, there was no significant difference among conservatives (+1 *SD*; $M_{Muslim} = 2.48$, $M_{Christian} = 2.54$), $t(236) = .410$, $p = .684$, $d = 0.05$. A significant main effect of perpetrator religion also showed that participants overall attributed the crime more to religion when the perpetrator was Christian ($M = 2.68$, $SD = 0.80$) rather than Muslim ($M = 2.24$, $SD = 0.90$), $F(1, 236) = 16.39$, $p < .001$, $\eta_p^2 = .065$.

The interaction between perpetrator religion and political ideology was also significant for attributions to mental health, $F(1, 236) = 4.13$, $p = .043$, $\eta_p^2 = .017$. Here, however, increasing conservative attitudes predicted marginally greater attributions of the crime to mental health when the offender had been described as Christian, $B = .17$, $SE = .09$, $t(236) = 1.91$, $p = .056$, but did not significantly predict attributions of the crime to mental health when the offender had been described as Muslim, $B = -.09$, $SE = .09$, $t(236) = -.980$, $p = .329$. Thus, liberals attributed the crime significantly more to mental health when the offender had been described as Muslim ($M = 3.95$) rather than Christian ($M = 3.59$), $t(236) = -2.07$, $p = .039$, $d = -0.27$, whereas conservatives' attributions again did not significantly depend on offender religion ($M_{Muslim} = 3.78$, $M_{Christian} = 3.92$), $t(236) = .810$, $p = .417$, $d = 0.11$. The results for offender remorse and offender intentionality were similar and significant and can be found in ESM 1.

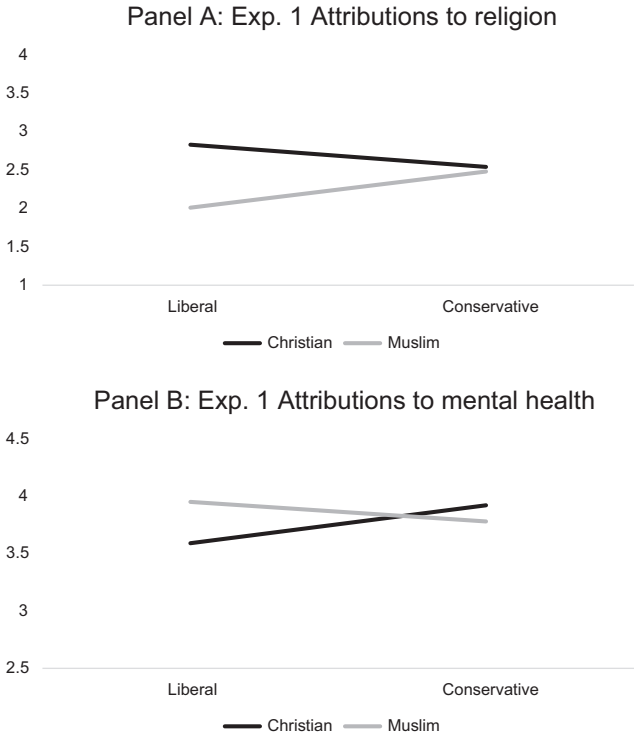

Figure 1. The effects of political orientation when the offender was either Christian (black lines) or Muslim (gray lines) on attributions for the crime to religion (Panel A) and mental health (Panel B) in Experiment 1.

Measures of News Production

Written Reports

We also evaluated participants' written reports about the crime scenario to test how the offender's religious group membership and participants' political ideology would affect how people communicate crimes to others. Specifically, based on the content of the scenario participants had been given, we created a list of words related to religion and the situational context participants were describing (e.g., Islam-, Christ-, Quran, Bible, mosque, church) and mental health (e.g., mental, health, depress, psych) and calculated the overall total of words in each category that were used, while controlling for the overall number of words used (see ESM 1 for the complete list of words). For this analysis, the data of three participants were excluded because they wrote about unrelated events.

For words associated with religion, we found a marginally significant interaction, $F(1, 226) = 3.74$, $p = .054$, $\eta_p^2 = .016$, indicating that the effect of political ideology on the use of words associated with religion (i.e., the slopes

[1] For ease of presentation, when reporting analyses of mixed models where the four aspects of the offender's life are included (i.e., religion, mental health, social life, and childhood), we report the results for the two most theoretically interesting aspects, namely religion and mental health.

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of political ideology) differed when the perpetrator was Christian compared to Muslim, $B = .55$, $SE = .28$, $t(226) = 1.93$, $p = .054$. Simple sloped analysis revealed that while neither simple slope differed significantly from zero, the two slopes significantly differed from each other. When the perpetrator was Christian, increasing conservatism decreased the use of religious words, $B = -.22$, $SE = .20$, $t(226) = -1.14$, $p = .254$, whereas when the perpetrator was Muslim, increasing conservatism increased the use of words associated with religion, $B = .32$, $SE = .20$, $t(226) = 1.57$, $p = .117$; see Figure 2. The simple effects at high (+1 *SD*) and low (−1 *SD*) were not significant, ts < 1.49, $p > .138$. For words associated with mental health, the two-way interaction was not significant, $F(1, 226) = 1.52$, $p = .219$, $\eta_p^2 = .007$.

Sample essays (see ESM 1) illustrate how participants distinguished between perpetrators based on their religion. For example, a liberal participant in the Christian condition reported at the beginning of their essay that "[Matthew Clark] had been frequenting online Christian chat rooms and looking for local religious groups", whereas a liberal participant in the Muslim condition did not mention religion at all, but rather reported that "[Ahmed] Yusuf did not have any friends on campus… he was a victim of cyber bullying in high school, and that he became even more reserved after the incident, refusing to leave his room for weeks at a time." Meanwhile, a conservative participant in the Christian condition avoided religion and rather stated that "Clark's behavior prior to the attack indicates that he was a troubled young man. His therapist, whom he had been seeing for a month prior to the shooting indicated Clark showed symptoms of being clinically depressed." A similarly conservative participant in the Muslim condition mentioned religion first, stating that "Officers found religious verses and symbols on his side of the room… and found that Yusuf had recently spent a lot of time looking for Muslim chat groups online and local religious groups." Of course, the samples cannot be taken as full representations of how liberals and conservatives reported on these crimes, though they do illustrate the decisions participants made in communicating these crimes to others.

Ranked Prioritization of Offender's Experiences

We also examined which aspect of the offender's life participants prioritized reporting about, using a mixed analysis with offender religion and political ideology as between-subject factors, and the aspect of the offender's life as a two-level within-subject factor (religion vs. mental health). The three-way interaction, $F(1, 234) = 8.35$, $p = .004$, $\eta_p^2 = .034$, indicated that the interaction between offender religion and political ideology differed depending on which aspect participants were ranking. To disentangle this three-way interaction, we tested for the two-way interaction between political ideology and offender religion on each aspect separately. Importantly, note that as this is a ranking variable (1–4), lower numbers indicate a higher ranking and thus greater importance for a given concept.

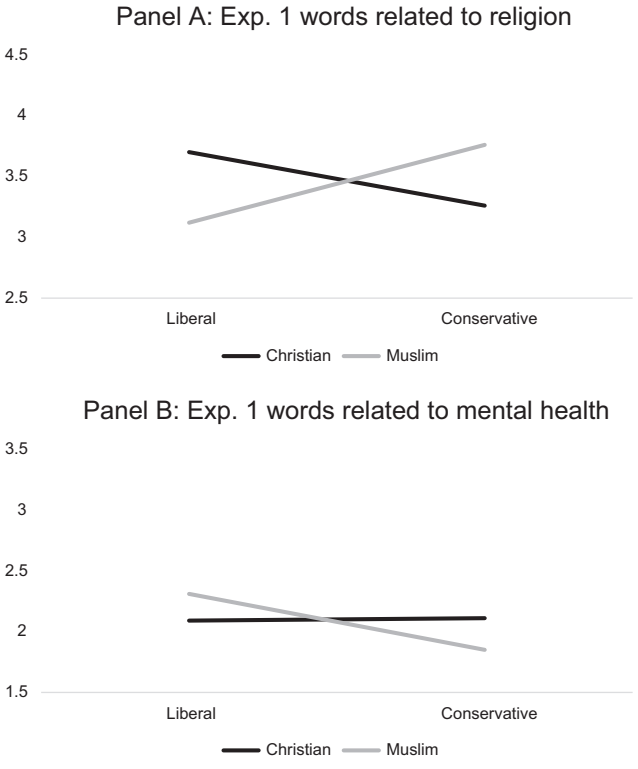

Figure 2. The effect of political orientation when the offender was either Christian (black lines) or Muslim (gray lines) on use of words associated with religion (Panel A) and mental health (Panel B) in Experiment 1.

For religion, the main effect of perpetrator religion was significant, $F(1, 234) = 17.13$, $p < .001$, $\eta_p^2 = .068$, such that participants prioritized religion less when the perpetrator was Muslim ($M = 2.94$, $SD = 1.25$) rather than Christian ($M = 2.27$, $SD = 1.30$). This effect was moderated by a significant interaction, $F(1, 234) = 10.92$, $p < .001$, $\eta_p^2 = .045$, such that increasing liberal (and decreasing conservative) attitudes predicted increased prioritization of religion when the offender had been described as Christian, $B = .31$, $SE = .11$, $t(234) = 2.73$, $p = .007$, but marginally decreasing prioritization of religion when the offender had been described as Muslim, $B = -.23$, $SE = .12$, $t(234) = -1.96$, $p = .051$. From another perspective, liberals prioritized reporting on religion when the offender had been described as Christian ($M = 1.95$) rather than Muslim ($M = 3.16$), $t(234) = -5.27$, $p < .001$, $d = -0.69$, while conservatives did not differ significantly ($M_{\text{Muslim}} = 2.70$, $M_{\text{Christian}} = 2.57$), $t(234) = -.580$, $p = .562$, $d = -0.08$.

For mental health, the main effect of perpetrator religion was again significant, $F(1, 234) = 8.83$, $p = .003$, $\eta_p^2 = .036$,

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such that participants prioritized mental health more when the perpetrator was Muslim (M = 2.68, SD = 1.11) rather than Christian (M = 3.08, SD = 1.01). This effect was moderated by a marginal interaction, $F(1, 234) = 3.79$, $p = .053$, $\eta_p^2 = .016$, with increasing liberal (and decreasing conservative) attitudes predicting lower prioritization of mental health when the offender had been described as Christian, $B = -.20$, $SE = .10$, $t(234) = -2.07$, $p = .039$, but having no effect when the offender had been described as Muslim, $B = .07$, $SE = .10$, $t(234) = .710$, $p = .478$. A simple effects perspective revealed that liberals prioritized reports about the offenders' mental health more when the offender was Muslim (M = 2.61) rather than Christian (M = 3.27), $t(234) = 3.48$, $p < .001$, $d = 0.45$, while conservatives did not differ (M_{Muslim} = 2.75, $M_{Christian}$ = 2.89), $t(234) = .720$, $p = .473$, $d = 0.09$.

Examining the Underlying Process

To better understand why political ideology had this effect on processing and disseminating news about offenders who differ on their religion, we investigated two competing arguments. First, that political ideology comprises negative intergroup attitudes (Jost et al., 2003) and therefore those who like and dislike the target group adapt their responses to better fit their worldviews. Second, that people from different political ideologies may differ in their moral values (Graham, Haidt, & Nosek, 2009) and therefore have principled reasons to reject a different religion or to find it more likely to be threatening (Sniderman et al., 1989). Therefore, we conducted mediation analyses where we pitted measures of these two competing perspectives against each other: attitudes toward Muslims and attitudes about Islam.

Process analysis (Hayes, 2013) revealed an indirect effect of political ideology on attitudes toward Islam, $B = -.47$, $SE = .05$, $t(234) = -8.64$, $p < .001$, 95% CI [−.575, −.362], which in turn, moderated by religion of the perpetrator, predicted attribution of the crime to religion, $B = -.37$, $SE = .14$, $t(234) = -2.67$, $p = .008$, 95% CI [−.643, −.096]. This mediation path was significant when the perpetrator was Muslim, $B = .19$, Boot $SE = .06$, 95% CI [.083, .305], rather than Christian, $B = .01$, Boot $SE = .05$, 95% CI [−.074, .113], and the direct effect of political ideology was no longer significant, indicating that participant attitudes toward Islam partially explained why political ideology interacted with perpetrator religion to determine attributions to religion. Neither of the other two mediation models was significant for either attitudes toward Muslims or attitudes toward Islam.

Thus, a mediation analysis suggested a role for attitudes toward Islam but not attitudes toward Muslims, which might suggest a principled rejection of the religion which is based in an understanding of Islam rather than a prejudice toward Muslims driving this effect. To test this explanation of the source of attitudes toward Islam, we predicted attitudes toward Islam through knowledge of Islam and political ideology and found no significant effect, $F(1, 234) = .12$, $p = .733$, suggesting that a basic knowledge of Islam did not seem to play any role in people's attitudes toward Islam, both among conservatives and liberals. The mediation analyses above included knowledge of Islam as a covariate, with no effect on the overall mediation.

Discussion

Experiment 1 found consistent evidence for motivated reasoning on the basis of political ideology. Supporting the hypothesis that motivated reasoning bias can also be driven by liberalism (rather than exclusively by conservatism), Experiment 1 found that liberals consistently emphasized the role of religion in crimes committed by a Christian (rather than Muslim) while simultaneously emphasizing the role of mental health in crimes committed by a Muslim (rather than Christian) in interpreting and disseminating a news report. Conservatives, on the other hand, did not differ based on the offender's religious group membership. Importantly, these results emerged not only in how participants processed information, but also in how they communicated it to others. These results partially support the hypothesis that people interpret as well as communicate information in ways that are consistent with politically preferred conclusions about the specific incidents in question. Yet, this motivated reasoning and communication bias was only found for liberals, but not conservatives. It was not clear, however, whether the null findings among conservatives indicate that conservatives do not have such bias, or whether, due to the sampling methodology of Experiment 1, the left-leaning sample may not have included participants who were sufficiently conservative to detect such bias. Experiment 1 also found preliminary evidence that attitudes toward Islam, and not attitudes toward Muslims, may explain some of the effect of political ideology specifically on attribution of a crime to religion.

Experiment 2

Experiment 2 sought to replicate Experiment 1 in a large sample of American adults recruited online through Amazon Mechanical Turk. As Experiment 1 drew from a relatively young and liberal set of participants (college students in the Northeastern United States), we aimed to test whether similar evidence of motivated reasoning based on political ideology would emerge for a larger sample that was more representative in both age and political ideology.

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Method

Participants

We recruited 1,115 participants online through Amazon Mechanical Turk. Participants were mostly female (female: 58.2%, male: 40.9%, non-identified: 0.6%, other: 0.4%), between 18 and 79 years old (M = 36.89, SD = 12.69), and their political ideology was close to the scale midpoint (α = .94; M = 3.27, SD = 1.40, 1 = *very liberal*, 6 = *very conservative*). The study took about 30 min to complete, and participants were compensated with 50 cents for their participation.

Measures

The measures and procedure were identical to those in Experiment 1. However, we added questions about the 2012 and 2016 presidential elections.

Results

Interpretations of the Crime Scenarios

Attributions for the Crime

Consistent with Experiment 1, a mixed analysis with offender religion and political ideology as between-subject factors and participants' attributions of the crime to either religion or mental health as a two-level within-subject factor revealed a significant three-way interaction, F(1, 1,066) = 39.15, p < .001, η_p^2 = .035, indicating that the interaction between offender religion and participant political ideology differed based on the type of attribution. When attributions were made based on religion, a significant main effect of perpetrator religion revealed that people attributed the crime more to religion when it was committed by a Muslim (M = 2.98, SD = 1.22) rather than a Christian (M = 2.69, SD = 1.13), F(1, 1,066) = 19.64, p < .001, η_p^2 = .018. A main effect of political ideology also revealed that the more conservative participants were, the more they attributed the crime to religion, B = .25, SE = .03, t(1,066) = 7.11, p < .001. These effects were moderated by a significant two-way interaction, F(1, 1,066) = 46.44, p < .001, η_p^2 = .040, where increasing liberal (and decreasing conservative) attitudes predicted significantly weaker attributions of the crime to religion when the offender had been described as Muslim, B = .48, SE = .05, t(1,066) = 9.65, p < .001, but political ideology did not have a significant effect on attributions of the crime to religion when the offender had been described as Christian, B = .01, SE = .05, t(1,066) = .220, p = .829. A simple effects analysis revealed that liberals were marginally more likely to attribute the crime to religion when the offender was Christian (M = 2.68) rather than Muslim (M = 2.52), t(1,066) = 1.69, p = .091, d = 0.10, while conservatives were significantly more likely to attribute the crime to religion when the offender was Muslim (M = 3.48) rather than Christian (M = 2.70), t(1,066) = −7.95, p < .001, d = −0.49. The two-way interaction on attribution to mental health was not significant, F(1, 1,066) = .85, p = .356, η_p^2 = .001. See Figure 3.

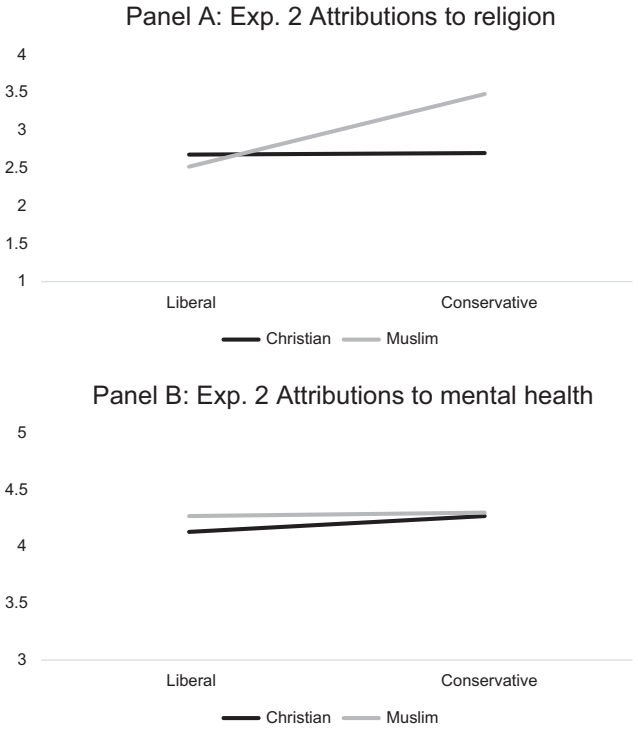

Figure 3. The effects of political orientation when the offender was either Christian (black lines) or Muslim (gray lines) on attributions for the crime to religion (Panel A) and mental health (Panel B) in Experiment 2.

Measures of News Production

Written Reports

As in Experiment 1, we evaluated participants' written reports about the crime scenario to see how the offender's religion and participants' political ideology would affect how much participants focused on religion and mental health when communicating the crime to others. Using the same set of words as in Experiment 1 to indicate focus on religion and mental health (see ESM 1), we analyzed participants' written reports. One hundred and four participants were excluded from this analysis for irrelevant writings.

When looking at the use of words associated with religion, we found a main effect of offender religion, F(1, 971) = 6.27, p = .013, η_p^2 = .006, such that people used more words associated with religion when the offender was portrayed as Muslim (M = 2.85, SD = 2.59) rather than Christian (M = 2.67, SD = 2.66). This effect was moderated by a significant two-way interaction between perpetrator

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religion and participant political ideology, $F(1, 971) = 6.25$, $p = .013$, $\eta_p^2 = .006$. Specifically, the effect of political ideology on the use of words associated with religion (i.e., the slopes of political ideology) differed when the perpetrator was Christian compared to Muslim, $B = .33$, $SE = .13$, $t(971) = 2.50$, $p = .012$. When the perpetrator was Christian, increasing conservatism and decreasing liberalism were associated with a nonsignificant tendency to use *less* religious words, $B = -.13$, $SE = .09$, $t(971) = -1.47$, $p = .142$, whereas when the perpetrator was Muslim, increasing conservatism and decreasing liberalism led to *more* use of words associated with religion, $B = .19$, $SE = .09$, $t(971) = 2.05$, $p = .040$. A simple slopes analysis revealed that, in contrast to Experiment 1, conservatives used more words related to religion with a Muslim ($M = 3.12$) compared to a Christian offender ($M = 2.47$), $t(971) = -3.43$, $p < .001$, $d = -0.22$, with no difference emerging among liberals ($M_{Muslim} = 2.73$; $M_{Christian} = 2.73$), $t(971) = .01$, $p = .994$, $d = 0.00$ (see Figure 4, Panel A).

Similarly, with respect to mental health, the two-way interaction was significant, $F(1, 971) = 4.83$, $p = .028$, $\eta_p^2 = .005$. Once again, the effect of political ideology on mental health words differed based on perpetrator religion, $B = -.20$, $SE = .09$, $t(971) = -2.20$, $p = .028$. While neither simple slopes significantly differed from zero, the interaction was driven by how they differed from each other. With a Christian perpetrator, increasing conservatism and decreasing liberalism were associated with a tendency to use more mental health words, $B = .08$, $SE = .06$, $t(971) = 1.30$, $p = .195$, whereas with a Muslim perpetrator, increasing conservatism and decreasing liberalism led to somewhat less use of mental health words, $B = -.12$, $SE = .06$, $t(971) = -1.80$, $p = .072$ (see Figure 4, Panel B). The simple effects were nonsignificant.

Once again, reviews of sample essays (see ESM 1) illustrate how participants used or emphasized religious or mental health themes in their reports. A liberal participant wrote about a Christian perpetrator saying early on in the essay that "many religious items were found and... he had been spending a lot of time in Christian chat groups recently." A liberal participant writing about a Muslim, however, reported first that "witnesses report Yusuf had recently been seeing a counselor at the university who believes Yusuf was suffering from depression... At the time of the shooting, Yusuf was not taking any medication to combat his depression." Sample essays by conservatives illustrate a reverse approach. When writing about a Christian, a conservative participant reported that "Clark had engaged in many social activities but had recently withdrawn from almost all interaction. Speaking to the chaplain on campus, found that Clark had become more involved in religious activities. Clark had also recently sought counseling for depression, but had not been formally diagnosed. An interview with a family member revealed a strained family relationship circling around abuse and suicide." While religion is included in this essay, it is not emphasized or presented as a leading fact. A conservative in the Muslim condition, however, wrote that "detectives found Islam and Muslim searches as well as diagrams in the shooter's apartment... the shooter had recently taken up an increased interest in Muslim and Islam. The matter is still being investigated and it is still yet to be determined if the shooting is linked to terrorism and radical Islam." Again, these samples are not fully representative, but they illustrate how people from different sides of the political spectrum can spread different narratives based on identical information.

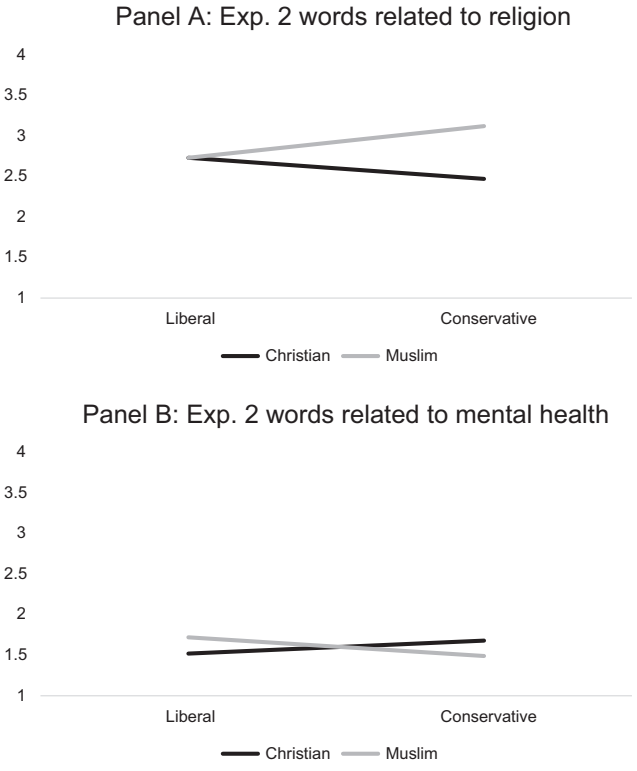

Figure 4. The effect of political orientation when the offender was either Christian (black lines) or Muslim (gray lines) on use of words associated with religion (Panel A) and mental health (Panel B) in Experiment 2.

Ranked Prioritization of Offender's Experiences

A mixed analysis testing whether the interaction between offender religion and political ideology depended on offender's religion versus mental health was significant, $F(1, 1{,}066) = 10.64$, $p = .001$, $\eta_p^2 = .010$, indicating that the effects of offender religion and political ideology of the participant differed when ranking the importance of religion versus mental health. Again lower numbers indicate a higher ranking and thus greater importance for a given concept.

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Looking first at the importance of religion, main effects of offender religion, $F(1, 1{,}066) = 5.42$, $p = .020$, $\eta_p^2 = .005$, and participant political ideology, $F(1, 1{,}066) = 5.33$, $p = .021$, $\eta_p^2 = .005$, were both significant, indicating that participants ranked religion as more important when the offender was Christian ($M = 2.84$, $SD = 1.19$) rather than Muslim ($M = 3.02$, $SD = 1.18$), and that the more conservative and less liberal participants were, the more important they ranked religion to be, $B = -.08$, $SE = .04$, $t(1{,}066) = -2.31$, $p = .021$. These effects were moderated by an interaction between political ideology and offender religion, $F(1, 1{,}066) = 12.62$, $p < .001$, $\eta_p^2 = .012$, with simple slopes analysis revealing that increasing liberal (and decreasing conservative) attitudes predicted significantly lower prioritization of *religion* when the offender had been described as Muslim, $B = -.21$, $SE = .05$, $t(1{,}066) = -4.06$, $p < .001$, but did not significantly predict prioritization of religion when the offender had been described as Christian, $B = .04$, $SE = .05$, $t(1{,}066) = .900$, $p = .369$. Simple effect showed liberals prioritizing religion more for Christian ($M = 2.80$) rather than Muslim ($M = 3.22$) offenders, $t(1{,}066) = -4.16$, $p < .001$, $d = -0.25$, whereas conservatives' prioritization of religion did not depend on offender religion ($M_{\text{Muslim}} = 2.80$, $M_{\text{Christian}} = 2.88$), $t(1{,}066) = .870$, $p = .386$, $d = 0.05$.

There was also a main effect of offender religion on prioritization of mental health, $F(1, 1{,}066) = 5.42$, $p = .020$, $\eta_p^2 = .005$, indicating that participants ranked mental health as more important when the offender was Muslim ($M = 2.31$, $SD = 0.99$) rather than Christian ($M = 2.46$, $SD = 1.07$). The interaction on mental health was marginally significant, $F(1, 1{,}066) = 3.66$, $p = .056$, $\eta_p^2 = .003$, however the simple slopes were nonsignificant. Simple effects suggested that liberals prioritized mental health more for Muslim ($M = 2.26$) rather than Christian ($M = 2.54$) offenders, $t(1{,}066) = 3.05$, $p = .002$, $d = 0.19$, whereas conservatives' prioritization of mental health did not depend on offender religion ($M_{\text{Muslim}} = 2.35$, $M_{\text{Christian}} = 2.38$), $t(1{,}066) = .350$, $p = .730$, $d = 0.05$.

Examining the Underlying Process

We found that political ideology predicted attitudes toward Islam, $B = -.42$, $SE = .02$, $t(1{,}070) = -17.77$, $p < .001$, 95% CI [−.462, −.370], which in turn, moderated by religion of the perpetrator, predicted attribution of the crime to religion, $B = -.44$, $SE = .08$, $t(1{,}070) = -5.64$, $p < .001$, 95% CI [−.599, −.290], and the prioritization of reporting about religion, $B = .25$, $SE = .08$, $t(1{,}070) = 2.95$, $p = .003$, 95% CI [.082, .411]. The indirect path was significant when the perpetrator was Muslim in both outcomes, and when the perpetrator was Christian for the prioritization outcome. There was no mediation for use of religious words.

Once again, we investigated knowledge of Islam and found that knowledge had no significant effect on attitudes even when moderated by political ideology, $F(1, 1{,}066) = 2.51$, $p = .113$. Once again, the mediation analyses included knowledge of Islam as a covariate.

Discussion

Experiment 2 replicated and extended the effects of Experiment 1. Once again, we found consistent evidence for motivated reasoning on the basis of political ideology. In contrast to Experiment 1, and as predicted, when we extended the research to a more politically representative sample we found that motivated reasoning biases affect people on both sides of the political aisle. Liberals prioritized religion more when communicating a crime by a Christian rather than Muslim, while also prioritizing mental health more when communicating a crime by a Muslim rather than a Christian. Similarly, conservatives attributed the crime more to religion when it was committed by a Muslim rather than a Christian. Furthermore, analyses of open-ended writings showed that people's political ideology shifted the likelihood of using religion or mental health-related words when communicating the crime to others. Additionally, Experiment 2 further investigated the role of attitudes toward Islam and Muslims as underlying belief systems within political ideology and found support for the results of Experiment 1 such that attitudes toward Islam as a religion but not Muslims as a group explain attributional judgments and determinations of what elements of a story to prioritize in a report. However, and again replicating Experiment 1, attitudes toward Islam were unrelated to a measure of knowledge of Islam, suggesting that negative (or positive) attitudes may come from some source other than a fair familiarity with the religion. Thus, Experiment 2 supports and extends the findings of Experiment 1 to suggest that political ideologies create patterns of motivated reasoning that bias perception and communication of news media.

General Discussion

How is news processing, creation, and maintenance affected by religious group membership of offenders? Evidence across two large-scale experiments suggests that it critically depends on people's worldview. Specifically, we found that liberals and conservatives both process and communicate crimes in ways that are consistent with their worldviews. Experiment 1 found that in a sample of mostly left-leaning younger adults, liberals prefer using religious attributions for Christians rather than Muslims, while relative conservatives (i.e., in this sample: political centrists) do not show that bias. However, in a more representative sample, we see that both liberals and conservatives interpret

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and communicate news in ways that are consistent with their political worldviews. Furthermore, across both studies we find evidence that attitudes toward Islam, but not toward Muslims, mediate the role of political ideology, which we unpack below.

Motivated Social Cognition and Political Ideology

Our results support past research indicating the role of motivated social cognition in interpretations of events (Kunda & Sinclair, 1999). When people have ideological or other aims in their preferred interpretation of the world, these motivations are reflected in how people process information. Importantly, our research extends previous research by showing how easily these motivations can also infect the spread of information at a time when information is being spread faster than ever before. In an extension on recent work (Noor et al., 2019), we also show the bias and discrimination that underlie the motivated cognition of politically relevant events for partisan individuals. Both liberal and conservative participants displayed biased attributions based on the religion of the offenders. In line with arguments that conservatives are motivated to find evidence that Muslims and Islam pose a threat (perhaps due to prejudices or xenophobia associated with conservatism, or participants' perceptions of the need to fight a culture of political correctness; Jost et al., 2003; Lalonde, Doan, & Patterson, 2000), conservative participants found the religion of the offender to be a more compelling reason for why the crime occurred when the crime was committed by a Muslim rather than Christian. In contrast, in line with arguments that liberals are motivated against finding evidence that Muslims or Islam pose a threat (perhaps due to their valuing of diversity and an accompanying motivation not to see groups they consider positively diverse as posing a threat, or in order to fight perceived biases that they believe other people hold; Carney et al., 2008), liberal participants found the religion of the offender to be a more compelling reason for why the crime occurred when the crime was committed by a Christian rather than Muslim. This finding in particular expands on research showing that cognitive biases are not solely the provenance of the political right (e.g., Ditto et al., 2017; Kahan et al., 2011), supporting a more nuanced and balanced view of biases related to political worldviews.

Prejudice and Principle

Our analyses of underlying components that are suggested to explain the relationship between political ideology and intergroup judgment found that it was judgments about Islam as a religion rather than Muslims as a group that explained these effects. This would appear to provide some evidence to support the principled conservative perspective (e.g., Sniderman et al., 1989) which argues that many conservative positions reflect principles unfamiliar to many liberals rather than intergroup bias over the argument that conservatism serves as an ideology of intergroup bias (e.g., Jost et al., 2003). However, it is also important to note that in our analyses, knowledge of Islam played no role in determining attitudes toward Islam, which suggests that the attitudes are derived from somewhere other than a fair familiarity with central features of the religion and call into question the principles that might underlie reactions by conservatives that differentiate between Christian and Muslim perpetrators. Additionally, the attitudes toward Islam measure was comparative between religions and differs from a secular critique measure (Imhoff & Recker, 2012) that may be better suited to access secular attitudes toward Islam.

Muslims and the Media

Our findings also highlight the important role that political ideology can play in how media reports are created, presented, and then transmitted through organic social networks. Past research suggests that media reports can often display biases against Muslims and other non-Caucasian groups (Scharrer & Ramasubramanian, 2015), specifically when explaining the cause of a crime (Chen et al., 2015). Furthermore, these biases negatively affect the audience's perceptions of Muslims and other minority social groups (Chen et al., 2015). Thus, it is important to better understand where these biases come from and how they manifest. Our research points to the pervasive quality of politically motivated biases, which can affect information processing and transmission and feed into narratives that support the worldviews of both news reporters and news audiences.

Importantly, this research went beyond measuring attitudes and behavioral intentions to also behaviorally evaluating how people report and communicate politically charged news events. We found that motivated social cognition biases can easily spread beyond people's internalization of news and media reports to the way the news events are then communicated to others. The resulting, already biased communications of such news events can then lead to biased understanding of the news events among the recipients of the communication, even if those do not engage in biased processing themselves, ultimately creating so-called "echo chambers". Finally, this research also highlights that attitudes toward Muslims in particular have increasingly become a distinguishing feature between competing political worldviews and may therefore explain the vitriol in perceived fairness in reporting on issues related to Muslims that is used by political and media commentators as well as media consumers on both sides of the political aisle.

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Limitations and Future Directions

Despite the consistent results, there were some limitations that should be addressed by future research. First, participants' responses and written reports may have been affected by what audience they thought may be reading their reports. If so, rather than reflecting internalized biases, the biased communication of news events to others could reflect the communicator's concerns that a nonbiased communication could lead to dangerous tendencies among the recipients of the communication. For instance, some liberals might communicate crimes committed by Muslims in a biased way not because they have a biased understanding of the crime, but rather as a strategy aiming to not increase any prejudice among the audience against members of vulnerable groups. Further research may seek to manipulate the imagined audience to determine the extent to which the effects reflect purely internal processes rather than responses or impression management to a perceived audience. Secondly, while we did measure attitudes toward Muslims and Islam, we did not measure attitudes toward Christians and Christianity. Analyzing attitudes toward Christianity may have given us more insight into the motivation behind liberals and conservatives or reduced any bias that may have occurred from asking solely about attitudes toward Muslims. Thus, future research may look more into attitudes toward Christianity and Islam, as well as fundamentalist beliefs that may differentiate between different ways of perceiving the religions. Future research might also include measures of social desirability as well to investigate other potential explanations for why political liberals and conservatives might differ in their attributions of crimes to religion or mental health as a function of the religion of the perpetrator. In addition, because political scales vary across countries, it may also be important to collect similar data in other countries to examine the generalizability of these results. Lastly, measuring political orientation after the experimental manipulation, instead of before, may have biased the results. Thus, future research should improve on the method used here to better capture how people's ideological preferences affect their reporting of news events.

Conclusion

Although there are many factors that play a role in the different media portrayals of the Muslim offenders in the San Bernardino shooting and the Caucasian offender in the Las Vegas shooting, the two studies reported here suggest that such differences may arise from how reporters and lay people process, generate, and spread information. Importantly, political ideology can bias processing and generation of media reports for people all along the political spectrum. Increasing awareness of how political ideology can affect how people see and communicate about the world may help them better understand and control the sources and consequences of their biases.

Electronic Supplementary Material

The electronic supplementary material is available with the online version of the article at https://doi.org/10.1027/1864-9335/a000385

ESM 1. Text (.pdf)

Manipulation scenarios, Word library for written report analyses, correlation tables of experimental moderators, and full example written reports and additional analyses for Experiments 1 and 2.

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History
Received September 23, 2018
Revision received March 3, 2019
Accepted March 6, 2019
Published online July 1, 2019

Authorship
Samia Habib, Levi Adelman, and Bernhard Leidner developed, conducted, and analyzed the studies, and wrote the manuscript together. Shaheen Pasha and Razvan Sibii helped develop Study 1 and assisted with data collection in Study 1.

ORCID
Samia Habib
https://orcid.org/0000-0003-2021-1801

Bernhard Leidner
Department of Psychological and Brain Sciences
Tobin Hall, Room 639
135 Hicks Way
Amherst, MA 01003
USA
bleidner@psych.umass.edu

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Erratum

Correction to Habib, Adelman, Leidner, Pasha, and Sibii (2019)

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The article entitled "Perpetrator religion and perceiver's political ideology affect processing and communication of media reports of violence" by S. Habib, L. Adelman, B. Leidner, S. Pasha, & R. Sibii (*Social Psychology,* https://doi.org/10.1027/1864-9335/a000385) contained an error on page 1.

The correct affiliations are as follows:

Razvan Sibii
Journalism Department, University of Massachusetts, Amherst, MA, USA

Samia Habib
Department of Epidemiology, Boston University School of Public Health, MA, USA

We regret any inconvenience or confusion this error may have caused.

Reference

Habib, S., Adelman, L., Leidner, B., Pasha, S., & Sibii, R. (2019). Perpetrator religion and perceiver's political ideology affect processing and communication of media reports of violence. *Social Psychology*. https://doi.org/10.1027/1864-9335/a000385

History
Published online October 7, 2019

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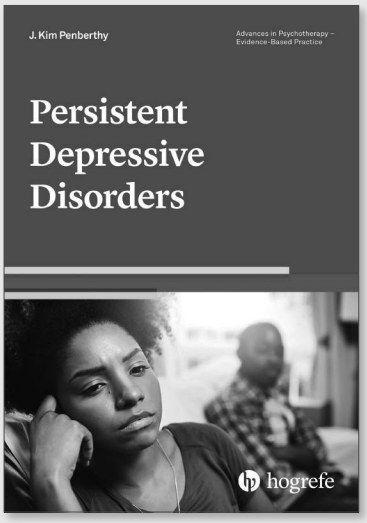

J. Kim Penberthy

Persistent Depressive Disorders

(Series: Advances in Psychotherapy – Evidence-Based Practice – Volume 43)
2019, vi + 106 pp.
US $29.80 / € 24.95
ISBN 978-0-88937-505-5
Also available as eBook

This compact guide is packed with the latest knowledge on the assessment and treatment of persistent depressive disorders (PDDs) – the new DSM-5 diagnosis that amalgamates the categories dysthymic disorder (DD), chronic major depression (MDD), and DD with major depressive episode (MDE).

Written by a leading expert, the book guides us through the complexities of assessing PDDs and the models for understanding how these difficult to identify and potentially life-threatening disorders develop and are maintained over long periods. It then outlines those therapies that have the strongest evidence base. The author goes on to explore in detail the cognitive behavioral analysis system of psychotherapy (CBASP), a treatment specifically developed for PDDs. This compelling integrated approach incorporates components of learning, developmental, interpersonal, and cognitive theory with aspects of interpersonal mindfulness. We are led expertly through the therapeutic process using clinical vignettes and practical tips, with particular attention paid to identifying the assessment and therapy methods most valuable in CBASP. Printable tools in the appendices can be used in daily practice.

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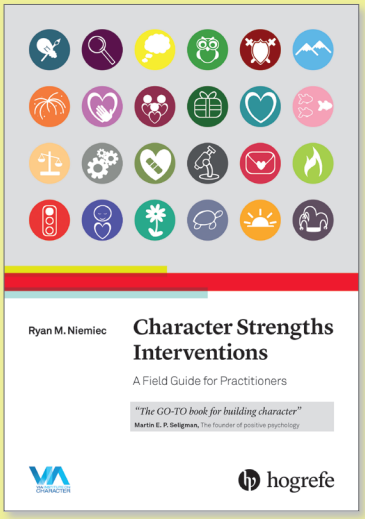

Ryan M. Niemiec

Character Strengths Interventions

A Field Guide for Practitioners

2018, xx + 300 pp.
US $59.00 / € 46.95
ISBN 978-0-88937-492-8
Also available as eBook

This book is the epitome of positive psychology: it takes the "backbone" of positive psychology – character strengths – and builds a substantive bridge between the science and practice. Working with clients' (and our own) character strengths boosts well-being, fosters resilience, improves relationships, and creates strong, supportive cultures in our practices, classrooms, and organizations. This unique guide brings together the vast experience of the author with the science and the practice of positive psychology in such a way that both new and experienced practitioners will benefit. New practitioners will learn about the core concepts of character and signature strengths and how to fine-tune their approach and troubleshoot. Experienced practitioners will deepen their knowledge about advanced topics such as strengths overuse and collisions, hot button issues, morality, and integrating strengths with savoring, flow, and mindfulness. Hands-on practitioner tips throughout the book provide valuable hints on how to take a truly strengths-based approach.

The 24 summary sheets spotlighting each of the universal character strengths are an indispensable resource for client sessions, succinctly summarizing the core features of and research on each strength. 70 evidence-based step-by-step activity handouts can be given to clients to help them develop character strengths awareness and use, increase resilience, set and meet goals, develop positive relationships, and find meaning and engagement in their daily lives.

No matter what kind of practitioner you are, this one-of-a-kind field guide is a goldmine in science-based applications. You'll be able to immediately bring the science of well-being into action!

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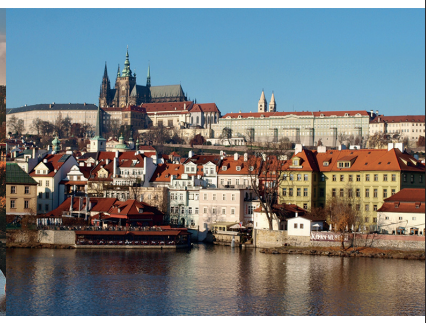

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