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Religion and Oppression: Cross-National and Experimental Investigations

Ian Hansen1, Valerie Jackson2 and Andrew Ryder3

1Department of Behavioral Sciences, York College, CUNY, Jamaica, NY, Corresponding author

2Department of Clinical Psychology, California School of Professional Psychology, Alliant International University, San Francisco, CA, USA

3Department of Psychology, Concordia University, Montreal, Quebec

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Abstract

Our goal was to investigate the relation between religiosity and oppression. We examined, in two multi-national samples taken from pre-existing data, how national oppression—measured by an index of lack of liberty and by the number of refugees originating from the country—related to religiosity (e.g., belief in God, prayer frequency). We also examined how oppression related to authoritarianism and exclusivity, measures of conservatism often correlated with religiosity. Oppression’s relation to religiosity depended on what was controlled in the analysis: without additional controls, the relation was modestly positive; when controlling for human development and other demographic variables, the relation was modestly negative. In a separate student sample, participants at an ethnoculturally heterogeneous urban college who were primed with questions measuring religiosity were less supportive of oppression and militarization than if unprimed or primed with questions measuring authoritarianism. These findings offer preliminary evidence that religion and oppression are potentially in tension with each other.

**Religion and Oppression: Cross-National and Experimental Investigations**

Oppression—the non-consensual, and often exploitative or violent, domination of one group by another—is common in human societies. Resistance to oppression is likewise common, although less so. Some evidence suggests that the ability and inclination to resist oppression have become more ingrained in human psychology over time (Boehm, 1999, 2012). Nevertheless, nations, countries and cultural groups differ in the inclination and success of their people with regard to protecting or defending themselves against oppressive conditions.

Religiosity and its associated ideological attitudes are among the psychological variables that should be relevant to oppression. Since the Enlightenment if not before, modernity has been characterized by a partial or full detachment from previously-revered religious and cultural traditions. Though secular developments have by no means fully replaced religion, many contemporary societies are now accustomed to questioning and strategically revising their own traditional beliefs and practices.

Insofar as such tradition-threatening questioning and revision may enable—or result from—national resistance to oppression (Voltaire, 1765/2010), the question of the compatibility between a free society and a religious one has long been a matter of intellectual and political interest. Modern individuals decide whether to be religious with more ease than deciding whether to live in an oppressive society. Thus we imagine more people are concerned about whether religion causes oppression (e.g., an “opiate of the people”) rather than whether oppression causes religion (a “sigh of the oppressed,” Marx, 1843/1970). We share the former interest while recognizing that the influence may be bi-directional and that the causal arrow can be more difficult to identify than an overarching relationship. Generally, oppression is a product of people’s decisions, and people’s religiosity potentially affects such decisions. Thus it is not implausible that popular religiosity (or lack thereof) has some overall influence on institutional indicators of oppression vs. liberty.

Theoretical and empirical work by psychologists on the social and moral differences between cultural groups suggests that variation in respect for rights and liberties overlaps with variation in religious and collective concerns. For instance, secular liberal cultures that primarily respect the “ethics of autonomy” (Shweder et al, 1997) or the moral foundations of “care” and “fairness” (Graham, Haidt & Nosek, 2009) tend to be more concerned about rights, liberties and freedom. In contrast, religious, conservative, and collectivist cultures that give equal or greater weight to “ethics of community,” “ethics of divinity” (Shweder et al, 1997) or moral foundations of “ingroup,” “authority,” and “purity” (Graham, Haidt & Nosek, 2009) tend to elevate these other competing values at the potential expense of rights, liberties, and freedom (Haidt, 2012; Haidt & Graham, 2007). The implication is that civil and political liberties tend to be in tension with religiosity, and thus that an oppressive society may be more enabling of religious flourishing, or that cultures where religiosity flourishes enable oppression.

There is no strong competing theoretical narrative in psychology to suggest that protecting individual rights and freedoms would increase the popularity of religious beliefs or that religiosity would attenuate oppression. This may be because of the general paucity of literature on what psychological qualities are related to national oppression and liberty. There are, however, two related literatures that suggest that religion, or at least aspects of it, might be compatible with liberty and an obstacle to oppression. The first is the literature on religion’s complex relationship to prejudice (and rejection thereof) and the second is the literature on religion’s relationship to universalist-benevolent “transcendence” values.

Prejudice relates both conceptually and empirically to oppression, as oppression tends to be a psychological cause, consequence or correlate of prejudice. The extensive literature on religion’s relationship to prejudice does not conclusively indict religion as a reliable cause of prejudice but instead has produced mixed and contingent results. Gordon Allport’s early claim that “[religion] makes and unmakes prejudice” (Allport 1954/1979, p. 444) is probably the most widely accepted summary of this literature. There are good theoretical reasons for this ambivalent view of religion. On the one hand, religious texts tend to contain at least a few passages that exhort followers to respect and fairly treat one another, foreigners, the poor, the oppressed, and sometimes even convicts and enemies. On the other hand, the translation of these humane messages into an equally humane collective ethos often runs up against other psychological and institutional obstacles associated with religion. These obstacles include a number of messages in the same religious texts exhorting more prejudicial and chauvinistic inclinations.

Empirical investigations have validated Allport’s ambivalence by finding that certain forms of religiosity (e.g., “Quest” religiosity) consistently predict lack of prejudice, while others (e.g., “extrinsic” religiosity) more often than not predict harboring prejudice. Still others (e.g., “intrinsic” religiosity) predict prejudice or a lack thereof, depending on the sample, measures, and other contingent factors (for a recent review, see Hall, Matz, & Wood, 2010). Measures of intrinsic religiosity—inwardly held, sincere religious belief—are probably most relevant to the measures of religion used in the present investigation, such as belief in God and prayer frequency. Intrinsic religiosity, moreover, appears to be quite psychologically complex in its relationship to prejudice and other indices of likely support for oppression.

Literature in personality and social psychology that focuses on a contrast of broadly “liberal” views and traits with “conservative” views and traits (e.g., Haidt, 2012; Jost, Glaser, Kruglanski, & Sulloway, 2003) puts intrinsic religiosity on the conservative side of a values dichotomy and rejection of intrinsic religiosity on the liberal side. And measures indicative of conservatism tend to correlate reliably with intolerance and support for oppressive and discriminatory violence (Altemeyer, 1981, 1988, 1996; Cohrs et al, 2007; Doty, Winter, Peterson & Kemmelmeier, 1997; Elms & Milgram, 1966; Hansen & Norenzayan, 2006; McFarland, 2005; Mann, 1973; Swami et al, 2012; Van Hiel & Mervielde, 2005; Whitley, 1999). Moreover, there are good empirical reasons to place intrinsic religiosity with conservatism: measures of the former tend to be positively correlated with some measures of the latter, most notably Right Wing Authoritarianism (RWA). Altemeyer’s (1988) studies have found an average correlation between RWA and intrinsic religiosity (Allport & Ross, 1967) of about *r*= .40. On the basis of this robust relationship, it is plausible to hypothesize that religiosity and liberty are incompatible.

However, intrinsic religiosity’s positive correlation with authoritarianism does not prevent the intrinsically religious from having an independent inclination to reject prejudice. When holding authoritarianism and related variables statistically constant, people who are more intrinsically religious are *less* prejudiced by a variety of measures (Hansen & Norenzayan, 2006; Kirkpatrick, 1993; Laythe, Finkel and Kirkpatrick, 2001; Laythe et al., 2002). Hansen and Ryder (2016) found this pattern—intrinsic religiosity negatively related to prejudice and support for violence when controlling for authoritarianism and other “coalitional rigidity” variables—not only in American Protestant samples, but also in international samples of Catholics, Jews, Buddhists, Hindus, Muslims, and the religiously unaffiliated.

This curious and cross-culturally robust pattern suggests the need for an alternative non-unidimensional model of analysis, what might be called a “value mapping” model. Such a model allows researchers to posit a prosocial/egalitarian vs. selfish/amoral dimension that is orthogonal to the liberal (open to change) vs. conservative (protective of old ways) dimension (see, e.g. Schwartz, 1992, 1994). Empirical evidence suggests that some content-valid measures of religiosity represent the most pro-social and egalitarian correlates of conservatism. For instance, religiosity—measured by scales conceptually overlapping with measures of intrinsic religiosity—has been found to be most strongly positively correlated with the benevolence subscale of the Schwartz Value scale (Saroglou & Muñoz-García, 2008). Benevolence is indicative of the pro-sociality that, on Schwartz’s empirically based map of values, is a neighbor to an egalitarian social justice orientation—labeled by Schwartz as “Universalism.”

Universalism values—e.g., “world at peace,” “social justice,” “equality,” “unity with nature”—are often considered core values of the political “left” (Schwartz, 1994). Both benevolence and universalism are toward the “self-transcendence” area of the values map, an area orthogonal to liberalism (what Schwartz calls “Openness to Change”) vs. conservatism (what Schwartz calls “Conservation”). Assuming that benevolence and universalism should be conducive to liberty, the fact that religiosity is positively correlated with benevolence, which in turn is correlated with universalism, can be considered circumstantial evidence supporting the hypothesis that religion might be in tension with oppression.

As already noted, there have been few empirical investigations relevant to the relation of actual oppression to religion—that is, the broad relation of oppression at the level of a society to the religiosity of the individuals in that society. Norris and Ingelhart (2011) found indirect evidence, using the World Values Survey, that well-developed post-industrial societies were sites of religious decline. Moreover, the vast majority of post-industrial societies are relatively democratic and higher than the world average in their protection of civil liberties and political rights. Thus, religious diminishment, post-industrial development, and democratic protection of rights and liberties all tend to co-occur, though the pattern of causal effects between these three variables is unknown.

To gain more confidence about whether religion actually *causes* oppression, oppression causes religion, or some third variable (such as lack of human development) causesboth, it is important to employ both multiple regression and experimental methods. For our cross-cultural multiple regression investigations, we sampled countries that suffer varying degrees of oppression and then examined whether residents of these countries expressed more or less intense religiosity and conservatism, respectively. We also conducted multiple regressions controlling for human development and other demographic variables to identify independent relationships less likely to lead to spurious causal inferences. Our first two studies thus investigated the degree to which religiosity and conservatism varied by country of residence when holding human development and other demographic measures constant.

For our third study we conducted an experiment as a preliminary inquiry into possible causal mechanisms that might underlie any independent relationships identified in the first two studies. This third study investigated, in a separate student sample, how increasing the salience of religiosity (relative to a control condition and an authoritarianism-salience condition) impacted support for institutional oppression and militarization.

**Study 1**

**Method**

**Overview.** In Study 1, we examined pre-existing data from 10 countries spanning four continents/world regions, with approximately 33% of the world population residing in the countries surveyed.

Our analyses addressed the following specific concerns: individual endorsements of religiosity items varying by (a) country-level protection of political rights and civil liberties (Freedom House, 2007); and (b) the number of refugees originating from the country (United Nations Human Development Program [UNHDP], 2007). The potential biases associated with each measure should not cause interpretive errors in the same direction, since Freedom House is independent of the UNHDP.

In addition, we also rank ordered countries by two measures of militarization that could be measured more objectively and that have theoretical implications for oppression: armed forces per capita in each country (UNHDP, 2007); and national spending on the military as a percentage of GDP (UNHDP, 2007). The years associated with data collection by index are as follows: civil and political liberty, 2007; refugees by country of origin, 2006; armed forces, 2007; and military expenditure, 2005. To examine the relationship of religion to liberty, we rank ordered countries by their Freedom House or UNHDP ratings—country, thus ordered, was the predictor—and evaluated individual-level “religiosity” and “conservatism” variables as criterion measures.

For the sake of our investigation, we operationalized “religiosity” as self-reported beliefs and behaviors such as belief in God and regular attendance at religious services. We operationalized “conservatism” as a kind of exclusive traditionalism, as we judged this to be the only measure in the sample that could be considered more related to conservatism than to religiosity, though it could also be considered a variable that captures the overlap of religiosity and conservatism. We acknowledge that many who would consider themselves “conservative” may not recognize themselves in this particular measure. Exclusivist conservatism is rooted more in boundary-setting cultural-ideological assertiveness than in, say, opposition to financial regulation. We included conservatism as a criterion in part to control for it as an extraneous variable, and also to inform interpretation of our religiosity-related findings.

**Sample.** The first author obtained the sample for Study 1 from a survey conducted by the British Broadcasting Corporation (BBC) (*n =* 10,068). The BBC conducted this survey in 2003-2004 as part of a television program called “What the World Thinks of God” (BBC, 2004). Ginges, Hansen and Norenzayan (2009) previously analyzed a subset of these data.

**Participants**. Of 10,068 participants, 49% were male, 51% female; 20% were between the ages of 18 and 24, 23% between 25 and 34, 20% between 35 and 44, 17% between 45 and 54, and 20% over 55. National and religious demographics are summarized in Table 1.

The BBC commissioned a British polling agency, ICM Associates, to partner with professional polling agencies in each country surveyed to conduct either telephone or face-to-face interviews with participants. ICM selected participants in all countries to be representative of either the national population or the population of the country’s most populous major metropolitan areas.

**Materials and measures**. We converted all BBC survey measures of religiosity-related variables into binary form for ease of analysis by binary logistic regression, as the existing measures were not clearly ordinal, and bimodal distributions were common. We also wished to maintain consistency with previous research using this sample (Ginges et al., 2009). Ginges and colleagues had also converted the variables into binary form, and for the same reasons. For instance, in the dataset, the question “Do you believe in God?” had a number of possible responses. We dummy coded “I have always believed in God” or “I believe in God but have not always”—the only definitively affirmative responses—as 1 and all other responses as 0. Additional binary coding details can be found in our descriptions of the relevant variables below.

Four of the key “religious” variables for the analysis were based on Atran and Norenzayan’s (2004) taxonomy of the relatively universal features of religion: counterintuition, commitment, communion and compassion. Their taxonomy identified four broad features that distinguish religious worldviews from nonreligious or less religious ones. “Counterintuition” is belief in supernatural agents that violate intuitive principles of folk epistemology. “Commitment” is a willingness to sacrifice on behalf of one’s worldview and its adherents. “Communion” is regular, often ritualistic, engagement with one’s co-religionists. “Compassion” in Atran and Norenzayan (2004) refers not to concern for relieving the suffering of others, *per se*, but to the passionate supernatural engagement (and usually explanatory resolution) of common human existential concerns like the fate of human consciousness after death. “Compassion” in this sense might be more appropriately labeled “supernatural resolution of existential concerns.”

The variables in the BBC dataset approximating these four features were (1) “theism” (“I have always believed in God” or “I believe in God but have not always” vs. other response), (2) “willing martydrom” (“I would die for my God/beliefs” vs. other response), (3) “religious attendance” (“I regularly attend an organized religious service” vs. other response), and (4) “afterlife belief” (agreement that “death is not the end” vs. other response). As an additional index of religiosity, we measured (5) “prayer frequency” (“regularly” vs. other response), which Ginges et al (2009) previously used as an index of religious belief. Though all five items are dissociable aspects of religiosity, our theoretical interest was in the common element shared by these measures.

The measure of conservatism in this sample was a kind of religious/ideological exclusivity: agreement with “My God (beliefs) is the only true God (beliefs)” vs. other response. There was no other item in the BBC’s brief survey that better approximated conservatism. Although the item as worded invites a religious interpretation, the inclusion of “beliefs” in parentheses provided an option for a secular exclusivist response. In addition, exclusivity is not one of Atran and Norenzayan’s (2004) four hypothesized features of religiosity. It is, however, close to the dogmatism identified as a feature of conservative motivated cognition (Jost et al, 2003).

**Analysis*.*** For multiple regression analyses, country (rank-ordered by a given criterion related to oppression or militarization) was the predictor variable, and religiosity the main criterion or outcome variable. Exclusivity was also a criterion variable in a separate analysis. We examined how each religiosity index differed by country when they were ranked according to: (1) lack of civil liberty and political rights (an oppression measure); (2) number of refugees originating from the country (oppression); (3) armed forces as a percentage of the population (a militarization measure); and (4) military spending as a proportion of GDP (militarization). We used rankings rather than raw scores or standardized scores for each criterion because our analysis used “country” as a predictor in the same way researchers treat sex, race, religious affiliation or political identity as predictors. When using an ordinally ambiguous predictor like country, race, or religion, it is customary to simply rank each level of the predictor by some numerical criterion (e.g. percentage of the population), rather than to assign all people of that race or religion the exact score of that criterion. For the present analysis we ranked “country” in several ways rather than just one, with ranking system relevant to oppression or militarization. For all analyses listed below, the direction of independent relationships remained the same if using standardized scores of country level variables rather than rank scores, though the strength of relationships varied (e.g. from weak when using rank scores to modest when using standard scores).

We also ranked countries by human development, as measured by the Human Development Index (HDI) for the year 2005, obtained from the UNHDP (2007). Given the findings of Norris and Ingelhart (2011) that human development is confounded both with lack of religiosity and with democracy (implying lack of oppression), we intended this ranking to serve as a variable to be controlled, rather than a main predictor of interest.

To measure national differences in liberty, we ranked countries so that rank scores of higher number indicated greater oppression or militarization—greater lackof civil liberties and political rights, more refugees originating from the country, greater percentage of the population in the military, and a greater percentage of the GDP devoted to military spending. Our analyses ranked the HDI so that higher scores indicated more human development. Due to the number of countries analyzed, we also standardized rank scores so that missing data and ties in rank on some indices would not lead us to compare measures with different metrics. We used regression rather than ANOVA or ANCOVA to assess the relationship of the predictor variable to the criterion (and these analyses are fundamentally interchangeable for hypothesis-testing purposes), but our analysis otherwise followed the same principle as most subject variable-based analyses. We have also listed the countries in order of their rankings on analytically relevant variables in the Supporting Online Material (Section1, SOM Table 1).

We performed separate binary logistic regressions to assess the degree of religiosity in countries with less civil and political liberty, more refugees originating from the country, more military spending as a proportion of GDP and more military personnel as a percentage of the country’s population. The first regression of each analysis thus had country (ranked by some index) as the predictor, the standardized index of religiosity as the criterion, and the HDI rank (also standardized) as an extraneous variable to be controlled. In the second regression, we controlled for other demographic factors also (age, sex, socioeconomic status—measured by type of work), as well as exclusivity. Moreover, to assess the relation of the predictor variables to exclusivity, we then performed another set of regressions in which the criterion measure was exclusivity rather than religiosity, and religiosity was controlled along with demographics.

To scaffold interpretation of the regression analyses, we first performed zero-order correlation analyses among our variables of interest. We examined the zero-order relation between rank in human development and rank in oppression- and militarization-related variables not only in the 10 countries examined in Study 1 but in all countries for which we had these data (expressing *n*s at the country level, 148 < *n*s < 176).

**Results and Discussion**

**Internal reliability of religiosity items.** The five religion items in Study 1 formed a reliable “religiosity” index when standardized (α = .72). We used the mean of these five items as a single measure of religiosity.

**Zero-order relations among religiosity, conservatism, human development, and oppression.** Table 2 shows the pattern of relations among religiosity, conservatism, oppression, militarization and human development. As expected, we found that our two indices of oppression were moderately to strongly correlated, that our two indices of militarization were strongly correlated, and that all four indices were intercorrelated, Cronbach’s α = .66. We also found, as expected, that religiosity and conservatism (exclusivity) were notably more predominant in lower HDI countries, and that lower HDI countries suffered notably more oppression.

Despite the general empirical overlap between militarization and oppression, and the negative relation between HDI and oppression indices, HDI was positively related or unrelated to our measures of militarization. This means that HDI was confounded with oppression indices in one direction (negatively), and confounded with militarization indices in a different direction (positively) or unconfounded. Table 2 in aggregate yields the following complex and partially counterintuitive result: that countries enduring more militarization were modestly *less* likely to be religious and slightly *less* likely to be conservative, yet countries enduring more oppression were modestly *more* likely to be religious and moderately *more* likely to be conservative.

Table 2 also suggests that exclusivity and the standardized five-item religiosity index were moderately to strongly correlated. A supplementary analysis found that the relationship between religiosity and exclusivity varied in magnitude (modest to strong) but not in direction by religious subsample (Unaffiliated, Buddhist, Hindu, Jewish, Muslim, Protestant, Catholic, Christian Orthodox, Other Christian, and Other). All subsample correlations were positive for religiosity and exclusivity, .16 < *r* < .56, all *p*s < .001.

**Scatterplots of zero-order relationships between oppression and religiosity.** Figures 1 and 2 show scatterplots of the zero-order relationship between lack of civil liberties and political rights and the number of refugees originating from the country (our main oppression predictors of interest) to average religiosity in the country. The X-axis of both figures consists of points along each oppression-measuring scale. For civil and political liberty (Figure 1), this was simply the number used by Freedom House, with higher numbers indicating more oppression. For the measure of refugees originating from the country, which ranged from 0 to 159,000, we log-transformed the data before graphing it (Figure 2). Because UNHDP presented refugee data in units of 1000, we log-transformed the data as if 1000 were 1, 159,000 were 159, etc. A number of countries in the UNHDP dataset were classed as having zero (thousands of) refugees originating from the country. Since log base 10 of zero is undefined, we present these countries as -1 on the X-axis of the figure. The Y-axis displays each country’s name and religiosity score (standardized) at the corresponding point on the X-axis.

In Figures 1 and 2, the regression lines slope upward, consistent with the modest positive correlations of Table 2 for the variables portrayed. However, in both figures, the relationship appears to be more curvilinear, like a downward-facing parabola. The figures also confirm that our sample had some national representation in all four possible quadrants combining religion and liberty: religious and free; not religious and free; religious and not free; not religious and not free. We note that the single country in the not-religious not-free category in both figures is Russia, in which the ruling state ideology mandated atheism for part of its history. Russia’s history is different from many plurality or majority non-religious nations. Specific political legacies of historical communism and the political-economic aftermath of its downfall, rather than popular lack of religiosity, may best explain oppression in Russia. Addressing the specific details of each country’s history is beyond the scope of our research, however.

**The independent relationship of predictors to conservatism.** As Table 3 shows, exclusivity’s relation to all four indices was consistently positive when controlling simultaneously for human development, demographics and religiosity. In other words, the more oppressed or militarized a country appeared to be, the more conservative (exclusivist) its inhabitants were, all else being equal. We consider this an unsurprising result given the known relation of conservatism both to prejudice and to pro-military attitudes. It is, moreover, an unsurprising result that emerges clearly only in a multiple regression analysis controlling for potentially confounding variables, not the zero-order analysis of Table 2. This suggests that a multiple regression analysis yields both more consistent and more intuitive results than a simple zero-order analysis. As noted in Table 3, the statistical significance of the negative relationship between exclusivity (without additional controls) and refugees is eliminated when using a Bonferroni correction for multiple tests.

**The independent relationship of predictors to religiosity.** As Table 3 shows, each index of national rank in oppression (lack of civil and political liberty, refugees originating from the country) and of militarization (military as a percentage of the population, military spending as a proportion of GDP) was a *negative*independent predictor of religiosity. This was the case whether controlling only for HDI or controlling for other demographics as well. That is, the more oppressed or militarized a country was, the less religious its inhabitants were, all else being equal. Parallel to the findings with exclusivity, the inconsistent zero-order relationships between religiosity and oppression and militarization (Table 2) contrasted with the more consistent independent relations in multiple regression (Table 3).

**Eliminating multicollinearity as an alternative explanation.** In any multiple regression analysis, it is possible that a statistical artifact known as multicollinearity might be distorting the strength and direction of regression coefficients. We did not find any evidence of strong multicollinearity in any of the regressions we performed in the main analyses of Study 1. Multicollinearity is usually only considered a problem when Variance Inflation Factors (VIFs) exceed 10—and even VIFs exceeding 10 do not necessarily call for removing variables from a regression analysis (O’Brian, 2007). The VIFs in the analyses listed in Table 3 were all less than 4 (maximum VIF = 3.24).

 The results of Study 1 offer differing levels of support to two hypotheses. At the very least they support a “weak religion-liberty (r-l) hypothesis” that there is no reliable *positive* relationship between religiosity and national oppression when other potentially confounding third variables are controlled. They may also support a strong r-l hypothesis: that there is a reliable *negative* relationship between religiosity and oppression when employing such controls. This weak vs. strong hypothesis framework follows the framework used in Hansen & Ryder (2016). Given the non-directional nature of multiple regression analyses, however, it is unclear whether the independent relation found suggests that religiosity may attenuate oppression and militarization or that oppression and militarization may attenuate religiosity.

Whether our Study 1 results most plausibly support only the weak hypothesis or the strong one as well, it is possible that our findings are specific to the 10 countries studied. Including more countries could provide a greater degree of confidence in any results obtained. Replication with more countries and individuals would also allow for a greater degree of power to minimize the possibility of Type II error when evaluating our weak hypothesis. Thus, for Study 2, we examined a larger dataset.

**Study 2**

**Method**

**Overview.** In Study 2, we examined pre-existing data from 52 countries spanning six continents/world regions, with approximately 74% of the world population residing in the countries surveyed. Our procedure was much the same as it was in Study 1, though we employed slightly different measures of religiosity and measured conservatism not by exclusivity, but by a set of measures most closely approximating the concept of authoritarianism. In this case, our measure of conservatism was more clearly secular, with no references to anything that could be considered religious. No equivalent to exclusivity as a more “religious” form of conservatism was available in this sample.

**Sample**. We obtained the sample for Study 2 from the 2005 wave of the World Values Survey (WVS) (*n* = 76,303) (World Values Survey, 2005). The WVS is an online resource for researchers interested in the variation of human values (see, e.g., Norris & Ingelhart, 2011).

**Participants.** Of 73,303 participants, 48% were male and 52% were female, with a mean age of 41. National and religious data are summarized in Table 1.

**Materials and measures**. The Supporting Online Material (Section 1, SOM Table 2) includes a list of the countries in order of their rankings on all analytically relevant variables. Like the BBC survey in Study 1, the WVS also measured prayer and religious attendance, and included the following items specifically: “Moments of prayer and meditation” (yes or no) and “How often do you attend religious services?” (7-point continuous variable from “more than once a week” to “never”). Though the WVS did not explicitly measure beliefs about the afterlife, willing martyrdom and belief in God, the survey measured other related indices of devotion to religious belief: “How important is God in your life?” (10 point continuous variable from “not at all” to “very”), “religion important” (4 point continuous variable from “very important” to “not at all important”), and “religious person” (given a binary coding of “a religious person” vs. all other responses).

The WVS did not include any item that referred directly to exclusivity, although it did measure variables suggestive of deference to authority and preference for authoritarian arrangements: “Having the army rule,” “Having a strong leader,” and “Having experts make decisions” (all were continuous measures with four response options, ranging from “very good” to “very bad”); and “Democracy: The army takes over when government is incompetent” (from 1 “not an essential characteristic of a democracy” to 10 “an essential characteristic of a democracy”). For demographic measures, we again measured sex, age and socioeconomic status (this time in terms of educational level attained).

**Results and Discussion**

**Internal reliability of religiosity and conservatism items.**  When standardized, the five religiosity items formed a reliable “religiosity” index (α = .84). The four standardized authoritarianism items formed an “authoritarianism” index with less adequate reliability (α = .57); deleting any of the four items from the scale, however, reduced reliability further. We combined the five religiosity items into a single measure of religiosity and the four authoritarianism items into a single measure of authoritarianism by taking the items’ mean after standardizing them.

**Zero-order relations among religiosity, conservatism, human development, and oppression.** Table 4 shows the pattern of zero-order relations among religiosity, conservatism, oppression, militarization and human development. The correlations among country-level measures (oppression, militarization and human development) are shown for countries included in Study 2 only. We again found, as expected, that militarization and oppression measures were intercorrelated, that HDI was confounded with both lack of oppression and lack of religiosity and conservatism, and that countries suffering more oppression were modestly more likely to be religious and moderately more likely to be conservative.

We found somewhat different results in this sample regarding militarization: religiosity had a modest positive zero-order correlation with military spending as a proportion of GDP, and a weak negative zero-order correlation with armed forces per capita. Conservatism was weakly or modestly positively related to both variables.

The relation between religiosity and conservatism (authoritarianism in this case) was again positive, though modest rather than strong (in Study 1, religiosity and exclusivity were strongly related). Again, the relationship between religiosity and conservatism varied in magnitude but not in direction by religious subsample (Unaffiliated, Buddhist, Hindu, Jewish, Muslim, Protestant, Catholic, Christian Orthodox, Other Christian, and Other). All subsample correlations of religiosity and authoritarianism were positive, .06 < *r*< .25, all but one correlation statistically significant at *p*< .001.

**Scatterplots of zero-order relationships between oppression and religiosity.** Similar to Figures 1 and 2 for Study 1, Figures 3 and 4 show scatterplots of the zero-order relation between lack of civil liberties and political rights and the number of refugees originating from the country (our main predictors of interest) with religiosity (our main criterion of interest). For the measure of refugees who had fled the country, which ranged from 0 to 1,451,000, we log-transformed the data before graphing it, as in Study 1. We used the same method of log-transforming the data used in Study 1.

In Figures 3 and 4, as in Figures 1 and 2 from Study 1, the regression lines slope upward, consistent with the modest positive correlations of Table 4 for the variables portrayed. As in Study 1, the relationship appears to be more curvilinear than linear (inverse parabolas), and again there is some representation of all four quadrants of religion-liberty possibilities: religious and free; not religious and free; religious and not free; not religious and not free. Again, current or former communist countries that once mandated atheism as a state ideology predominated among the not-religious-not-free countries. Figures 3 and 4 again confirm what Figures 1 and 2 showed: that there exist countries in all four quadrants. In our Supporting Online Materials (Section 2) we note how quadratic and cubic equations account for the curvilinear zero-order relational pattern found in both studies.

**The independent relationship of predictors to conservatism.** As Table 5 shows, national rank in indices of oppression and militarization were weakly positive independent predictors of authoritarianism in the WVS sample when controlling for HDI. There was little change in any of these findings when controlling also for demographics and religiosity variables. These findings are broadly consistent with the findings of Study 1, at least when controlling for demographic variables.

**The independent relationship of predictors to religiosity.** As Table 5 shows, the two indices of national rank in oppression (lack of liberty and refugees) were modestly negative independent predictors of religiosity when controlling for HDI. The two indices of militarization (military spending and armed forces) were weakly positive predictors of religiosity. There was little change in these findings when controlling also for demographics and conservatism variables. These findings on the negative relationship between oppression and religiosity are consistent with those of Study 1, though in Study 2 militarization appears, if anything, to be weakly positively related to religiosity rather than negatively related, a result inconsistent with Study 1. As was the case in Study 1, we found no evidence of multicollinearity in our analyses (all VIFs < 4).

The large sample of countries in Study 2 allowed us to assess the possible moderating role of human development and religious groupings of countries with regard to the relation between religiosity and oppression. In separate analyses available in the Supporting Online Materials (Section 3) we found that three different levels of human development were associated with different zero-order relations between religion and oppression: weakly positive, moderately negative and weakly negative. These findings are not inconsistent with the modestly negative HDI-controlled relation between oppression and religiosity in the full sample.

In the Supporting Online Material (Section 4) we discuss a more substantial moderating role for different religious groupings of countries (see SOM Table 3). Religiosity was negatively related to oppression in countries with pluralities of Orthodox Christians, Muslims, practitioners of South Asian or East Asian religions, and those unaffiliated with a religion. These nations composed the majority of the sample, and together represented a majority of the world’s people. Religiosity was positively related or inconsistently related to oppression in plurality Protestant/mixed Christian and Roman Catholic countries. Paradoxically, Protestant/mixed Christian and Roman Catholic countries also had the lowest mean levels of oppression in the sample, even when controlling for human development (see SOM Table 4).

**Causal ambiguities**. As noted earlier, our analyses in Studies 1 and 2 did not involve manipulated independent variables, only attempts to control for potential third variables confounded with our variables of interest. Controlling for human development and other demographic variables helped reduce the likelihood of drawing spurious causal conclusions from zero-order relationships, but fully addressing the question of causality on a cross-national scale would be much more difficult. It might require, for instance, randomly assigning countries to undergo religious revivals (vs. conservative revivals vs. no revival) and comparing changes in Freedom House and UNHDP refugee figures over some period of years.

In Study 3, we took a less direct but also less ethically fraught and costly approach to addressing causality, working with an ethnically and religiously diverse student sample, one in which religious beliefs were relatively normative. We employed country ranking along indices of oppression and militarization as a predictor in Studies 1 and 2 to maximize power in these essentially correlational analyses. However, we are, as noted in the introduction, more interested in the causal impact of religiosity on oppression. Our analyses in Study 3 thus make support for oppression the dependent variable. Our Supporting Online Material (Section 5) has more detailed discussion of our rationale for examining oppression as a predictor variable in Studies 1 and 2 and as a dependent variable in Study 3.

**Study 3**

**Method**

 **Participants.** Participants were 157 students at an urban public college in New York City who completed the study as part of the local human participants pool. The study received ethics approval from the college’s Institutional Review Board. Participants were 53% female, 38% male, remainder not reporting; mean age 24; by ethnicity 31% African descent, 21% Latina/o, 21% Asian descent, 6% European descent, 11% other, remainder not reporting; by religion, 20% Catholic, 20% other Christian, 13% Muslim, 10% not religious, 6% Hindu, 13% other, remainder not reporting; 61% US citizens/residents, 24% non-US, remainder not reporting; by political orientation 33.1% liberal, 20.4% moderate, 14.7% conservative, the remainder expressing uncertainty or not reporting. The sample was relatively religious, with only 6% describing themselves as atheistic, 28% neutral, 49% religious, and the remainder not reporting.

 **Materials and procedures**. We randomly assigned participants to one of three conditions. All conditions consisted of three pages of items with which participants could agree or disagree, or rate as good or not good. These pages were presented in different orders in each condition. Each page had a validated scale (e.g., Intrinsic Religious Motivation or Right Wing Authoritarianism) as well as items adapted from the measures of Studies 1 and 2. We expected the adapted items of each page to be positively correlated with the validated scale on each page.

On one of the three pages, there were religiosity-related items: Hoge’s (1972) Intrinsic Religious Motivation scale as well as items adapted from the Study 1 and Study 2 religiosity items. On another page, there were authoritarianism-related items: a short version of the RWA scale (Manganelli Rattazzi, Bobbio & Canova, 2007) as well as items adapted from Study 1’s exclusivity item and Study 2’s authoritarianism items. On another page, there were intolerance, oppression and militarization-related items: an adaptation of a political intolerance scale (based on Sullivan, Pierson, & Marcus, 1982) and items designed to capture support for the institutional oppression and militarization measured in Studies 1 and 2.

In the “religiosity first” condition, participants first completed the religiosity page, then the page with dependent variables, and then the authoritarianism page. In the “authoritarianism first” condition, participants first completed the authoritarianism page, then the page with dependent variables, then the religiosity page. In the control condition, participants first completed the page with dependent variables, then the religiosity page, then the authoritarianism page. Dependent variables thus either immediately followed primes (religiosity items or authoritarian items) or were presented without primes preceding them.

Table 6 shows sample items composing the scales used in Study 3, either as primes or as dependent variables. Table 6 notes the category of each scale, as well as the function it serves in our analysis. We intended religiosity and conservatism scales to be primes of the concepts associated with the items in these scales—possibly as elicitors or reminders of pre-existing religious or conservative inclinations among a critical mass of individuals in the sample. Ginges and colleagues (2009) used a comparable religion reminder manipulation in their third study. We intended oppression and militarization scales to be dependent variables potentially affected by the primes. Scales listed as “correlates” are previously validated scales whose empirical association to adapted items from Studies 1 and 2 could be measured in a way that was not possible in those studies. The full set of measures is available in the Supporting Online Material (Section 6, SOM Table 5).

**Results and Discussion**

**Preliminary analyses**. The items measuring religiosity cohered relatively well. Both Hoge’s (1972) Intrinsic Religious Motivation (IRM) scale and the nine items adapted from questions used in the BBC and WVS surveys showed good internal reliability, α = .88 and α = .92 respectively. These two summary measures were also correlated, *r*(153) = .76, *p* < .001. In the overall sample, both religiosity scores were above the scale midpoint of 5 (on a 9 point scale): *M* = 5.47, *SD* = 1.74 for IRM and *M* = 6.40, *SD* = 1.93 for the index of survey-adapted religious items. These sample means suggest that participants were relatively religious and so were likely inclined to increased salience of pre-existing religious beliefs when completing the religious items.

The items measuring conservatism/authoritarianism/exclusivity also cohered, though not as well. Both Manganelli et al’s (2007) 14-item RWA scale and the four authoritarianism items adapted from Study 2’s WVS survey showed adequate internal reliability, α = .69 and α = .71 respectively. These summary scores were moderately correlated, *r*(152) =.27, *p* = .001. Both scales were also correlated with the single exclusivity item adapted from Study 1’s BBC scale, *r*(152) = .31 and *r*(151) = .23 respectively, both *p*s < .01. A reliability analysis on all 19 items suggested adequate internal reliability, α = .74, greater than the reliabilities of either authoritarian subscale on its own. For RWA the sample mean was just below the midpoint, *M* = 4.74, *SD* = .95, as was the case for the four WVS-scale adapted authoritarianism items, *M* = 4.94, *SD* = 1.55, but the sample mean was more clearly below the midpoint for the exclusivity item, *M* = 3.63, *SD* = 2.79. The sample could, in general, be considered moderate, leaning liberal, with regard to matters of authoritarianism and exclusivity. The nature of the sample itself thus provides some evidence that “religious liberal” is not a contradiction in terms, and particularly that being both religious and low in exclusivity is possible. What is possible and what is likely nevertheless differ. Even within this sample, the religious measures were positively correlated with authoritarianism measures, .16 < *r*s < .41, ps < .05, and with exclusivity, .28 < *r*s < .31, *p*s < .001.

For the dependent measures, the six support-for-oppression items and the four support-for-militarization items both showed adequate or near-adequate internal reliability, α = .79 and α = .62. The 10 items together formed a more reliable index, α = .82. Exploratory factor analyses using principle axis factoring and direct oblimin rotation suggested that support for oppression and support for militarization did not form distinguishable factors. The most coherent presentation of the items was a principle components analysis extracting one component. All 10 items (after reversing con-trait items) loaded > .42 on that component, which explained 39.4% of the variance. We thus averaged all items to form an index of support for oppression-militarization.

The oppression-militarization index was positively correlated with items adapted from a validated scale of political intolerance (Sullivan, Pierson & Marcus, 1982), regardless of whether the intolerance items were framed as intolerance for people with values similar to participants, *r*(153)= .52, *p* < .001, or intolerance for people classed as “enemies” of participants’ values, *r*(153) = .49, *p* < .001. Overall sample averages for intolerance of similar others and intolerance of enemies were both well below the scale midpoint of 5, *M* = 2.83, *SD* = 1.42 and *M* = 3.66, *SD* = 1.51, respectively.

**Experimental analysis**. We performed a preliminary one-way ANOVA to identify whether there was an overall difference between the religiosity first, control and authoritarianism first conditions with regard to the dependent variable: support for oppression and militarization. This ANOVA was significant, *F*(2,150) = 3.50, *p* = .03. Figure 7 shows the means of oppression-militarization by condition. There was the least support for oppression-militarization in the religiosity first condition, and the most in the authoritarianism-first condition. The authoritarianism first and control conditions did not differ, |*t|* < 1. There was significantly less support for oppression-militarization in the religiosity-first condition compared to these two combined conditions, *t*(150) = -2.48 *p* = .01 (*p < α* even if using a Bonferroni-adjusted *α* of .0167), Cohen’s *d* = 0.40. This experimental result is broadly consistent with the findings of Studies 1 and 2. As noted earlier, our main interest is religion’s impact on oppression, so we were particularly concerned with whether the religion first condition differed from the other conditions, and if so in what direction. The reliability of other contrasts can be inferred from the confidence intervals noted in Figure 5, and further discussion of the contrasts is available in our Supporting Online Material (Section 7).

**Controlling for demographics**. To control for the demographic characteristics of participants, we performed a five-way ANOVA with condition (religiosity first vs. control vs. authoritarianism first), race (African descent and Hispanic vs. other), sex (male vs. female), citizenship (US vs. other), and religion (Catholic and other Christian vs. other) as independent variables, and with age as a covariate. Experimental condition remained a significant predictor of oppression-militarization in this analysis, *F*(2,78) = 4.59, *p* = .01. Estimated marginal means continued to indicate that the religiosity-first condition had the lowest oppression-militarization score (*EMM* = 2.69, *SE* = .25) and the authoritarianism-first condition the highest (*EMM* = 3.58, *SE* = .27). Our Supporting Online Material (Section 8) includes discussion of demographic effects and interactions with experimental condition.

In other subsequent analyses, condition had no effect on religious variables or authoritarianism variables, all *F*s < 1, suggesting that while activating the salience of pre-existing religiosity may attenuate support for oppression-militarization, it does not necessarily increase professed religious belief itself or have an effect one way or the other on authoritarian sentiment. Also, while we found evidence that exposure to religious questions decreased support for militarization-oppression, we did not find that exposure to questions on militarization-oppression (support for which was very low in the overall sample) affected religious belief. Thus, although we necessarily measured oppression as predictor and religiosity/conservatism as criterion in the first two studies (as oppression could only be measured at the country level and religiosity was measured at the individual level), Study 3 suggests that the causal arrow may run more readily from religiosity to lack of militarization/oppression, rather than from lack of militarization/oppression to religiosity.

**Multiple regression analyses**. We also entered an aggregated measure of religiosity (combining all 10 items of IRM with the nine other religiosity items) and an aggregated measure of conservatism (combining all 14 items of RWA with the four other authoritarianism items and the one exclusivity item) into a linear multiple regression predicting oppression-militarization in each of the three conditions. Conservatism was not a significant predictor of the dependent variable in the religiosity first condition, β = .27, *t*(50) = 1.53, *p* = .13, ns, though it was a significant positive predictor in the control and authoritarianism first conditions, β = .46, *t*(44) = 3.14, *p* = .003 and β = .43, *t*(48) = 3.18, *p* = .003, respectively. Religiosity was not a significant independent predictor of the DV in any of the three conditions, though it approached marginal significance as a negative predictor in the control condition, β = -.24, *t*(44) = -1.68, *p* = .10, ns. In the control condition, the positive independent relationship found between conservatism and support for oppression combined with the (nominally) negative independent relationship found between basic religiosity and such support replicates the findings of Hansen and Norenzayan (2006), Hansen and Ryder (2016), Laythe, Finkel and Kirpatrick (2001), Laythe et al. (2002), and Kirkpatrick (1993).

Our findings—that those primed with religiosity were less supportive of oppression-militarization than those in alternative conditions—are broadly consistent with some studies finding that experimental manipulations of religiosity can attenuate prejudice or support for violence (e.g. Clingingsmith, Khwaja, and Kremer, 2008; Ginges et al, 2009, study 3; Jonas & Fischer, 2006; Rothschild, Abdollahi, Pyszczynski, 2009). They are inconsistent, however, with those finding that reminders of religion can, under some circumstances, increase intolerance and support for violence (e.g. Bushman et al, 2007; Johnson, Rowatt & LaBouff, 2010; Johnson et al, 2011). Insofar as religiosity straddles both pro-prejudice and anti-prejudice inclinations, as Allport (1954/1979) noted, the effects on support for oppression and violence of experimental reminders of religiosity are likely to vary by other contingencies inherent to the sample or the specific nature of primes and measures. It remains theoretically possible that samples more representative of the American undergraduate population (e.g., disproportionately white and middle and upper class) might obtain results contrasting with those presented here.

**Validation of measures**. The results of Study 3 also contribute to validating the religiosity and authoritarianism variables used in Studies 1 and 2. Adaptations of the religiosity items from Studies 1 and 2 were positively correlated with a previously validated scale of religiosity, and adaptations of the exclusivity and authoritarianism items from Studies 1 and 2 positively correlated with a previously validated authoritarianism scale. Also, adding the Study 1 exclusivity item and the four Study 2 authoritarian items to Manganelli and colleagues’ (2007) RWA scale increased rather than decreased internal reliability. Though our measures of “conservatism” across the three studies inevitably differed from each other—see our Supporting Online Material (Section 9) for further discussion—they nevertheless appeared to be both thematically and empirically coherent, and potentially contrasting with religiosity. In the control condition especially, religiosity and conservatism contrasted in the direction of their independent relationship with oppression-militarization.

This pattern of findings in Study 3 is at least consistent with our weak r-l hypothesis: that religiosity has no reliable positive relationship with oppression when controlling for other potentially confounding variables, in this case by using random assignment to condition in an experiment. The findings also lean somewhat toward corroborating the strong r-l hypothesis as well.

**General Discussion**

Our analyses in Studies 1 and 2 suggest that when holding human development and other potentially confounding demographic variables constant, a country’s oppression is modestly negatively related to the religiosity of that country’s people. The two indices that showed consistent results across samples—assessment of civil and political liberty by Freedom House and UNHDP’s measure of refugees originating from the country—were also the two indices most plausibly related to the concept of national liberty vs. oppression. If focusing on these indices, at least the weak and to some extent the strong religion-liberty hypotheses are supported: when demographics are statistically controlled, religiosity is not reliably positively, and in fact appears reliably negatively, related to national oppression. If we regard measures of militarization as equally relevant criteria of oppression, then the balance tips more toward the weak hypothesis. Study 3 also offers additional evidence for at least the weak r-l hypothesis, and to some extent the strong one as well. What Studies 1 through 3 do *not* support, however, is the religion-oppression (r-o) hypothesis.

“Zero-order” methods of prediction (methods that involve correlating two variables without controlling for any other potentially confounding variables) are desirable for certain purposes. Zero-order methods can help with decisions about which countries to invest or live in, for instance. With analytic goals like these in mind, the data do support a non-causal “simple prediction” r-o hypothesis. Religion and oppression can be expected to have a modest positive zero-order relationship—due, we believe, to the particular way both variables are affected by global differences in human development. These differences in human development, moreover, plausibly precede the establishment of global differences in both national oppression and religiosity (Diamond, 1999). We consider it most plausible to consider human development (or lack thereof) a third variable that is antecedent to both oppression and religiosity, in much the same way that summer heat is antecedent to both ice cream sales and outbreaks of violence in crime-prone neighborhoods. Ice cream sales and violence may be positively correlated only as a coincidence of their relation to the antecedent summer heat; even if positively correlated, they may have no relation or even a negative independent relation when daily average temperature is controlled. Likewise, we have found that religiosity and oppression have a negative independent relation when the antecedent third variable of human development is statistically controlled.

Overall, the negative independent relation between religiosity and oppression does not appear to be particularly strong, and it is by no means immune to regional, cultural and economic variation. It appears neither necessary nor sufficient to liberty for a country to have a highly religious population at any particular range of human development. The findings of Study 3 offer some preliminary evidence for the possibility of religiosity salience causally reducing support for oppression and militarization. This causal effect might explain the negative independent relationship found between religiosity and indices of national oppression in Studies 1 and 2, but without a more ambitious international experimental study we must remain skeptical about this inference. A single study like Study 3 cannot eliminate alternative causal explanations; for example, that among countries of roughly equal development, experiences of oppression and militarization diminish the popular appetite for religiosity. A single study also cannot eliminate the possibility that some as-yet-unexamined “fourth variable” both increases religiosity and decreases oppression within narrow development ranges.

Even with all caveats considered, the three studies corroborate each other in suggesting some tension between religiosity and oppression, both sociologically (Studies 1 and 2) and psychologically (Study 3). The tension found in our data is not the result of multicollinearity affecting the interpretability of coefficients in the multiple regression analyses. Also, given the large sample sizes used, at least in the first two studies, we believe we have sufficient power to avoid Type II error with regard to our evidence of the lack of a positive independent relationship between religiosity and oppression.

**Explaining religion’s negative relationship to oppression**. The findings of Study 3 that reminders of religiosity dampen support for oppression do not rule out alternative causal explanations for the independent negative relationship between religiosity and national oppression. However, they add more plausibility to the hypothesis that religiosity can contribute relatively directly to reducing oppression. Explanations for such an effect, should subsequent research continue to corroborate it, are inevitably speculative. The following tentative explanations should be considered hypotheses for later testing.

First, what Atran and Norenzayan (2004) call the “counterintuitive” idea of a supernatural transpersonal being or beings has the potential to be an equality-promoting idea insofar as any omnipotent God who is creator and ruler of the universe is likely to see all human beings—whatever their social status in each others’ eyes—as relatively equal in stature. Personal stories of life-transforming “religious experience” tend to be consistent with this more inclusive, egalitarian understanding of God (James, 1902/1982). This egalitarian potential in the God idea may grow stronger as God becomes understood more as a universal conjoiner of beings and the threads of existence—a “big God” (Norenzayan, 2013) rather than as some other bigger-than-normal being or set of beings roaming the earth and interfering occasionally in human affairs. Researchers might test this by cross-culturally sampling religions with more abstractly conceived or concretely personalized deities and comparing the potential of religiosity to predict opposition to oppression/militarization (when controlling for conservatism variables like exclusivity and authoritarianism).

Also, as prayer and meditation are a part of most religions, the calming effect of silence, ritualized repetition, or pouring out one’s private thoughts before an imagined deity may contribute to less violent reactivity and thus to less potential for oppression in society. Hansen and Ryder (2016) found that prayer frequency negatively predicted religious scapegoating and, when controlling for authoritarianism and related conservative variables, also negatively predicted intergroup hostility toward people of other religious beliefs. Religiosity correlates positively with other psychological inclinations—like forgiveness and gratitude (Davis, Worthington Jr, Hook, & Hill, 2013; McCullough, Emmons, & Tsang, 2002) —and prayer may be an active ingredient for stimulating these inclinations.

In addition, religion’s cultivation of what Atran and Norenzayan (2004) call “commitment”—including willingness to sacrifice—should make coercing a religious group into enduring oppression or colluding with the oppression of others potentially difficult. This may be particularly relevant when activating the “communion” aspect of religion. It should be more difficult to violate the rights of people organized into independent communities of mutual support than it is to violate the rights of atomistic asocial uncooperative individuals. Milgram’s (1974/2009) “Two Peers Rebel” variation on his classic obedience experiment offers a good empirical illustration of the value of solidarity to resisting oppression—if the two confederates out of three “teachers” in the study created a group norm of disobedience, the last teacher (the participant) was much more likely to conform to this rebellious group norm than to obey the torture-commanding authority.

**Conclusion.** The analyses of this article support the view that when the potentially confounding third variables of human development and other demographics were controlled, religiosity was compatible with various indices suggestive of national liberty even as the conservatism correlated with it was more compatible with oppression and militarization. When not controlling for other variables, however, religiosity was modestly negatively correlated with liberty, and in one study (Study 1) conservatism was modestly negatively related to militarization. We consider our multiple regression findings more empirically and theoretically coherent—and consistent with our experimental findings—than our zero-order correlational findings. Our multiple regression findings confirm that it is possible—even somewhat likely within narrow ranges of development (that is, when controlling for development)—to have widespread and intense religiosity associated with less, rather than more, institutional oppression. And our experimental findings confirm that it is possible for reminders of religiosity to decrease support for militarization and oppression.

Even to the extent religiosity is a liberty-compatible worldview, the cross-cultural relation between religiosity and conservatism suggests that religiosity is not well-guarded against psychological inclinations associated with oppression. This “double-edged” feature is unlikely to be unique to religiosity, however. Egalitarianism and liberal individualism are two more secular orientations that are similarly double-edged. Egalitarianism is associated both with liberty-respecting welfare states (e.g., Scandinavian countries in Europe and the state of Kerala in India) and with the totalitarian excesses of historically practiced communism. Liberal individualism is associated both with the ideological exaltation of human and civil rights and with colonialism, slavery, and the violent extractive histories of industrialization.

Religiosity, egalitarianism, and liberal individualism also tend to relate inconsistently to each other, even though all may be plausible sometime contributors to institutional resistance to oppression. Adherents of these distinct value orientations can sometimes even treat each other like enemies, potentially undermining their own contributions to liberty in the process. Prejudice and selective oppression are generally not conducive to meaningful liberty.

Though the evidence of the present research is insufficient to bolster any particular worldview, we suspect that ideological pluralism at the global level—encouraging the peaceful coexistence of theoretically clashing ideologies—is likely more liberating than some cultural equivalent of the monolithic Tower of Babel. In a Tower of Babel-ruled global society, all other possible ideological goods would be made subordinate to, say, religious charisma, liberal individualism, or egalitarianism. Thus some psychological division between liberty-compatible orientations may be salutary to global liberty. It would likely be better for all, however, if the division were a peaceful and amicable one, with regular and friendly interaction across the divides to reinforce what is best on each side.

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Table 1. List of participating nations and religions, by sample

|  |  |  |  |
| --- | --- | --- | --- |
| Sample  | Nation (N), by continent | Sample | Religion, by category (N) |
|  BBC |  | BBC |  |
|   | Africa: |  | Buddhist (254) |
|  | Nigeria (1000) |  | Christian Catholic (1644) |
|  | Asia: |  | Christian Orthodox (832) |
|  | India (1006) |  | Roman and Greek Orthodox |
|  | Indonesia (1038) |  | Russian Orthodox |
|  | South Korea (1000) |  | Christian Protestant (1668) |
|  | North America: |  | Christian Protestant  |
|  | USA (1002) |  | Evangelist |
|  | Mexico (1021) |  | Presbyterian |
|  | Europe:  |  | Christian unclassified (566): |
|  | UK (1001) |  | Jehovah's Witness |
|  | Russia (1000) |  | Mormon |
|  | Middle East: |  | Quaker |
|  | Israel (1000) |  | Other Christian |
|  | Lebanon (1000) |  | Hindu (1003) |
|  |  |  | Jewish (866) |
|   |  |  | Muslim (1962) |
|  |  |  | Other (356) |
|  |  |  | Chinese traditional |
|  |  |  | Confucian |
|  |  |  | Jain |
|  |  |  | Sikh |
|  |  |  | Other |
|  |  |  | Refused to answer |
|  |  |  | Unaffiliated (917) |
|  |  |  | Agnostic |
|  |  |  | Atheist |
|  |  |  |  |

|  |  |  |  |
| --- | --- | --- | --- |
| WVS |  | WVS |  |
|  | Africa (sub-Saharan): |  | Buddhist (3298) |
|  | Burkina Faso (1534) |  | Buddhist |
|  | Ethiopia (1500) |  | Hoa Hao |
|  | Ghana (1534) |  | Christian Orthodox (7777) |
|  | Mali (1534) |  | Armenian Apostolic Church |
|  | Rwanda (1507) |  | Orthodox |
|  | South Africa (2988) |  | Christian Protestant (9756) |
|  | Zambia (1500) |  | Anglican |
|  | Americas: |  | Baptist |
|  | Argentina (1002) |  | Evangelical |
|  | Brazil (1500) |  | Lutheran |
|  | Chile (1000) |  | Methodist |
|  | Colombia (3025) |  | Presbyterian |
|  | Mexico (1560) |  | Protestant |
|  | Peru (1500) |  | Christian Roman Catholic (17289) |
|  | Trinidad and Tobago (1002) |  | Christian unclassified (1157) |
|  | USA (1249) |  | Assembly of God |
|  | Asia: |  | Christian |
|  | Japan (1096) |  | Christian Reform |
|  | South Korea (1200) |  | Church of Christ |
|  | India (2001) |  | Independent African Church |
|  | China (2015) |  | Jehovah’s Witness |
|  | Taiwan (1227) |  | New Apostolic Church |
|  | Thailand (1534) |  | Nondenominational |
|  | Indonesia (2015) |  | Other Christian  |
|  | Vietnam (1495) |  | Pentecostal |
|  | Hong Kong (1252) |  | Hindu (1961) |
|  | Malaysia (1201) |  | Jewish (160) |
|  | Europe:  |  | Muslim (18506) |
|  | Andorra (1003) |  | Muslim |
|  | Britain (1041) |  | Shia |
|  | Bulgaria (1001) |  | Sunni |
|  | Cyprus (1050) |  | Other (2962) |
|  | Finland (1014) |  | Ancestor worshipping |
|  | France (1001) |  | Bahai |
|  | Germany (2064) |  | Cao Dai |
|  | Italy (1012) |  | Israelita |
|  | Moldova (1046) |  | Jain |
|  | Netherlands (1050) |  | Native |
|  | Poland (1000) |  | Nuevo Pacto Universal |
|  | Romania (1776) |  | Other |
|  | Russia (2033) |  | Ratana |
|  | Serbia (1220) |  | Sikh |
|  | Slovenia (1037) |  | Spiritista |
|  | Spain (1200) |  | Spiritualist |
|  | Sweden (1003) |  | Taoist |
|  | Switzerland (1241) |  | Yiguan Dao |
|  | Turkey (1346) |  | Zoroastrian |
|  | Ukraine (1000) |  | Unaffiliated (13707) |
|  | Middle East and North Africa: |  | Don't know |
|  | Egypt (3051) |  | No answer |
|  | Morocco (1200) |  | Not applicable |
|  | Iran (2667) |  |  |
|  | Jordan (1200) |  |  |
|  | Iraq (2701) |  |  |
|  | Oceania: |  |  |
|  | Australia (1421) |  |  |
|  | New Zealand (954) |  |  |

**Table 2**

*Correlations among religiosity, conservatism, oppression, militarization, and HDI, Study 1*

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
| 1. Human Development Index (HDI) | (177) | -.54\*\*\* (176) | -.35\*\*\* (170) | -.08 (150) | .33\*\*\* (160) | -.34\*\*\* (10068) | -.32\*\*\* (10068) |
| 2. Lack of liberty (oppression) |  | (194) | .47\*\*\* (176) | .37\*\*\* (153) | .24\*\* (166) | .17\*\*\* (10068) | .29\*\*\* (10068) |
| 3. Refugees from country (oppression) |  |  | (177) | .34\*\*\* (148) | .13 (160) | .17\*\*\* (10068) | .23\*\*\* (10068) |
| 4. Mil. spending/GDP (militarization) |  |  |  | (153) | .58\*\*\* (151) | -.23\*\*\* (10068)  | -.10\*\*\* (10068) |
| 5. Armed forces/capita (militarization) |  |  |  |  | (166) | -.35\*\*\* (10068) | -.13\*\*\* (10068) |
| 6. Religiosity index (religiosity) |  |  |  |  |  | (10068) | .54\*\*\* (10068) |
| 7. Exclusivity item (conservatism) |  |  |  |  |  |  | (10068) |

*Note*. *n* for each analysis is in parentheses. \* *p* < .05 \*\* *p* < .01 \*\*\* *p* < .001.

**Table 3**

*Standardized regression coefficients (β) of the relationship of nation (ranked by predictors) to religiosity and exclusivity indices, with errors for β in parentheses, Study 1*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Criterion | Regression | Predictor |  |  |  |
|  |  | Lack of liberty |  Refugees | Military spending |  Armed forces |
| Religiosity | Controlling for Human Development Index (HDI) |  -.33 (.016) |  -.20 (014) |  -.13 (.010) |  -.24 (.011) |
|  | Controlling for HDI, demographics, exclusivity |  -.37 (.014) |  -.18 (.013) |  -.13 (.009) |  -.28 (.010) |
| Exclusivity | Controlling for Human Development Index (HDI) |  .09 (.016) |  -.03 (.014)a |  .01 (.010)b |  .09 (.012) |
|  | Controlling for HDI, demographics, religiosity |  .26 (.015) |  .06 (.013) |  .06 (.009) |  .20 (.010) |

*Note*. All coefficients were significant at *p* < .001, unless otherwise indicated. a *p* < .05 b *p* > .05, ns. The statistical significance of the relationship between exclusivity and refugees (a) is eliminated once making a Bonferroni correction for multiple comparisons. Unstandardized coefficients (*b*) and 95% confidence intervals for *b* are available from the authors on request. Minimum residual degrees of freedom in all analyses = 9631.

**Table 4**

*Correlations among religiosity, conservatism, oppression, militarization, and HDI, Study 2*

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | 1 | 2 | 3 | 4 | 5 | 6 | 7 |
| 1. Human Development Index (HDI) | (48) | -.67\*\*\*(48) | -.51\*\*\*(48) | -.25(46) | .17(47) | -.45\*\*\*(70121) | -.31\*\*\*(66102) |
| 2. Lack of liberty (oppression) |  | (52) | .67\*\*\*(50) | .44\*\*(46) | .25(49) | .17\*\*\*(76272) | .26\*\*\*(70821) |
| 3. Refugees from country (oppression) |  |  | (50) | .41\*\*(46) | .26(48) | .09\*\*\*(73825) | .17\*\*\*(69680) |
| 4. Mil. spending/GDP (militarization) |  |  |  | (46) | .59\*\*\*(46) | .15\*\*\*(67592) | .14\*\*\*(63647) |
| 5. Armed forces/capita (militarization) |  |  |  |  | (49) | -.03\*\*\*(72790) | .05\*\*\*(68629) |
| 6. Religiosity index (religiosity) |  |  |  |  |  | (76272) | .20\*\*\*(70805) |
| 7. Authoritarianism index (conservatism) |  |  |  |  |  |  | (70821) |

*Note*. *n* for each analysis is in parentheses. \* *p* < .05 \*\* *p* < .01 \*\*\* *p* < .001.

**Table 5** *Standardized regression coefficients (β) of the relationship of nation (ranked by predictors) to religiosity and authoritarianism indices, with standard errors for β in parentheses, Study 2*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Criterion | Regression | Predictor |  |  |  |
|  |  | Lack of liberty |  Refugees | Military spending |  Armed forces |
| Religiosity(Study 2) | Controlling for HDI | -.22 (.004) | -.19 (.004) | .04 (.003) | .04 (.004) |
|  | Controlling for HDI, demographics, authoritarianism  | -.21 (.004) | -.18 (.004) | .04 (.004) | .02 (.004) |
| Authoritarianism(Study 2) | Controlling for HDI | .12 (.005) | .04 (.004) | .08 (.004) | .11 (.004) |
|  | Controlling for HDI, demographics, religiosity  | .14 (.005) | .07 (.004) | .08 (.004) | .11 (.004) |

*Note*. All coefficients were significant at *p* < .001. Unstandardized coefficients (*b*) and 95% confidence intervals for *b* are available from the authors on request. Certain countries were excluded in some regressions because they did not measure a variable included in those regressions. Minimum residual degrees of freedom in all analyses = 61,818.

**Table 6**

*Sample items in Study 3 scales*

|  |  |  |  |
| --- | --- | --- | --- |
| Scale | Category (# of items) | Function | Sample items |
| Intrinsic Religious Motivation (Hoge, 1972) | Religiosity(10) | prime, correlate | --My religious beliefs are what really lie behind my whole approach to life.-- I feel there are many more important things in life than religion (reversed). |
| Religiosity adapted from BBC and WVS items | Religiosity(9) | prime | --I believe in God--I would die for my beliefs--I have moments of prayer and meditation |
| Right Wing Authoritarianism (Manganelli et al, 2007) | Conservatism/ Authoritar-ianism/ Exclusivity(14) | prime, correlate | --What our country needs most is disciplined citizens, following national leaders in unity.--It is good that nowadays young people have greater freedom to make their own rules and to protest against things they don’t like (reversed). |
| Authoritarianism adapted from WVS items | Conservatism/ Authoritar-ianism/ Exclusivity(4) | prime | --Having the army rule our country\*--Having a strong leader rule our country\*--Having experts make decisions in our country\* |
| Exclusivity adapted from BBC item | Conservatism/ Authoritar-ianism/ Exclusivity(1) | prime | -- My beliefs are the only true beliefs |
| Political intolerance of similar others, adapted from Sullivan et al (1982) | Intolerance/ Oppression/ Militarization(6) | correlate | --Groups formed by people who share my beliefs and values should be outlawed.--People who share my beliefs and values should be allowed to hold rallies in our community (reversed). |
| Political intolerance of “enemies,” adapted from Sullivan et al (1982) | Intolerance/ Oppression/ Militarization(6) | correlate | --Enemies of my beliefs and values should have their phone conversations secretly listened to by our government.-- Enemies of my beliefs and values should be allowed to teach in public schools (reversed). |
| Support for oppression of enemies\*\* | Intolerance/ Oppression/ Militarization(6) | DV | --I and other good people who follow the path of truth should have our political rights protected, but our enemies do not deserve to have these rights protected.--It would be terrible if my country persecuted any people—even my enemies—so terribly that they had to flee to another country as refugees (reversed). |
| Support for militarization\*\*\* | Intolerance/ Oppression/ Militarization(4) | DV | --It would be good if the jobs available to our country's people were mostly military jobs so we could battle our enemies more effectively.--It would be best for everyone if our nation's military spending could be reduced so our economic production could be directed to other concerns (reversed). |

\*Participants were asked to rate these items on a 9 point scale from “totally bad” to “totally good.”

\*\*Statements were composed to reflect opposition to arrangements of civil and political liberty (measured as sociological-level variables by Freedom House in Studies 1 and 2) or support for policies that would lead to increased refugee exodus (measured as a sociological-level variable by UNHDP in Studies 1 and 2).

\*\*\*Statements were composed to reflect support for increasing the proportion of jobs that are military jobs and the proportion of the economy directed to military endeavors (measured as a sociological-level variable by UNHDP in Studies 1 and 2).

Figure Caption

*Figure 1.* Relationship of civil liberty and political rights to religiosity, all countries of Study 1.

*Figure 2*. Relationship of refugees originating from a country to religiosity, all countries of Study 1.

*Figure 3.* Relationship of civil liberty and political rights to religiosity, all countries of Study 2.

*Figure 4*. Relationship of refugees originating from a country to religiosity, all countries of Study 2.

*Figure 5*. Effect of experimental condition on support for oppression-militarization (Study 3).



 





Note. 95% confidence intervals for the means are as follows: religion first, 95% CI = 2.17 – 2.90; control, 95% CI = 2.60 – 3.37; authoritarianism first, 95% CI = 2.85 – 3.59.