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Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

Zur Verfügung gestellt in Kooperation mit / provided in cooperation with:

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Empfohlene Zitierung / Suggested Citation:

Hoffmann, L., Basedau, M., Gobien, S., & Prediger, S. (2020). Universal Love or One True Religion? Experimental Evidence of the Ambivalent Effect of Religious Ideas on Altruism and Discrimination. *American Journal of Political Science*, 64(3), 603-620. <https://doi.org/10.1111/ajps.12479>

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Universal Love or One True Religion? Experimental Evidence of the Ambivalent Effect of Religious Ideas on Altruism and Discrimination

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Abstract: *Contrary to the expectations of secularization theory, religion remains socially important and affects politics in multiple ways—especially regarding conflict between religious communities. Theoretically, religion can increase altruism, but belief in the superiority of one's faith may facilitate intergroup discrimination and related conflict. Previous findings remain inconclusive, however, as specific religious ideas have hardly been tested. In this article, we argue that the content of religious ideas has causal effects on intergroup discrimination. We hence test the impact of two opposing, prominent religious ideas on altruism and discrimination: universal love and the notion of one true religion. Conducting dictator games with Christians and Muslims in Ghana and Tanzania, we find causal effects: Whereas the idea of one true religion increases intergroup discrimination, that of universal love fosters equal treatment. The policy implications hereof are obvious—promoting tolerant religious ideas seems crucial to avoiding conflict.*

Verification Materials: The data and materials required to verify the computational reproducibility of the results, procedures, and analyses in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/VG3UFZ>.

Man is a Religious Animal. He is the only Religious Animal. He is the only animal that has the True Religion—several of them. He is the only animal that loves his neighbor as himself and cuts his throat if his theology isn't straight.

—Mark Twain (“The Lowest Animal” 1896)

Secularization theory has assumed religion to be becoming less and less important, but real-world developments have not conformed to this expectation (Fox 2018; Toft, Philpott, and Shah 2011). As of 2017, more than 80% of the world's population identified with a religious group, and the number of

Muslims and Christians was expected to increase even further in the coming decades (Pew Research Center 2017). Religious ideas and identities affect politics in many ways, including political preferences (McKenzie and Rouse 2013; Spenkuch and Tillmann 2018), foreign policy choices, and democratization processes (Fox 2018; Philpott 2007; Toft, Philpott, and Shah 2011).

Religion seems to have become more contentious recently. Distrust between Christians and Muslims (Obaidi et al. 2018) has increased, as has discrimination against religious minorities (Fox 2016; Grim and Finke 2011). Violence between religious communities and over religious ideas has been on the rise: Basedau, Pfeiffer, and

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Open access funding enabled and organized by Projekt DEAL.

[Correction added on 28 October 2020, after first online publication: Projekt Deal funding statement has been added.]

American Journal of Political Science, Vol. 64, No. 3, July 2020, Pp. 603–620

© 2019 The Authors. *American Journal of Political Science* published by Wiley Periodicals LLC on behalf of Midwest Political Science Association DOI: 10.1111/ajps.12479

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Vüllers (2016) find in more than 45% of all conflicts since 1990 that religion came between warring factions (see also Svensson and Nilsson 2018; Walter 2017). The most recent data from the Uppsala Conflict Data Programme (UCDP) identified 51 armed conflict dyads in 2017 (Pettersson, Höglad, and Öberg 2019), at least 69% of which were related to either religious identity and/or ideas.¹

Theoretically, the effect of faith on social relations is ambivalent (Appleby 2000; Philpott 2007). Religion provides values and norms, and bonds believers—thus fostering cohesion and altruism (Barro and McCleary 2003; Johnson 2015). At the same time, religious ideas and identities can be exclusive, imposing religious norms and discriminating against nonbelievers (Basedau, Pfeiffer, and Vüllers 2016; Obaidi et al. 2018). The key question is this: Does religion foster altruism only toward members of one's own religious group while also promoting discriminatory behavior toward members of other ones?

We argue that the specific content of religious ideas contributes to answering this pertinent question. Focusing on the highly important yet understudied topic of the “ambivalence of the sacred,” we test the impact of two opposing, prominent religious ideas on altruism and intergroup discrimination: *universal love* and the notion of *one true religion*. Both ideas can be considered as core elements of the Abrahamic faiths, especially the world's biggest religions, Christianity and Islam (e.g., Assmann 2009). We chose Ghana and Tanzania as our sites of experimentation, as they demonstrate religious diversity but are not hot spots for religious violence—thus constituting a robust test of our hypotheses.

A large body of research has explored the impact of religion on prosocial behavior. *Prosociality* can be broadly defined as acts that benefit others, such as cooperating and sharing (Batson and Powell 2003). Many of these studies, which rely on self-reported measures, find a positive connection between the two (Batson, Schoenrade, and Ventis 1993; McCullough and Worthington 1999; Power 2017). However, they often lack causal identification and cannot rule out reverse causality, spurious correlation, or omitted variable bias. In addition, religiosity increases socially desirable responses (Gervais and Norenzayan 2012; Sedikides and Gebauer 2010).

Incentivized experimental studies have overcome these shortcomings by paying participants according to their decisions, thus providing them with incentives to

truthfully reveal their core preferences (Smith 1982). This makes experiments less prone to response biases compared to survey measures (Hoffmann 2013). Participants are randomly assigned to different treatments where one specific factor of interest is varied while the others are held constant. In this way, causality can be detected. Studies on religion and prosocial behavior draw primarily on priming techniques.² For religion, priming appears to be a realistic tool—as attending religious services can be considered a prime that activates one or more specific religious ideas.

A recent meta-study—including 93 experimental studies with more than 11,000 participants—reports a small to medium positive effect of religious primes on prosocial behavior (Shariff et al. 2016). Yet most of the studies included did not differentiate between religious content, instead aiming to make general statements about the effects of religion on behavior. A notable exception is the one by Preston and Ritter (2013), who find behavioral differences between priming the concepts of “God” and “religion.” This has important implications: Varying aspects of religion can lead to different forms of behavior, and thus they should be closely investigated.

A related strand of literature focuses on the darker side of religion: the limits to altruism. These studies measure discrimination, specifically by observing whether participants' behavior in experiments varies depending on the religious identity of their interaction partner. This literature again offers inconclusive results, however. A meta-study by Lane (2016) finds that 14.3% of their included studies reveal (mostly small levels of) religious outgroup discrimination (80.9% find none; 4.8% find outgroup favoritism).

We believe that religious content matters, and it helps explain the inconclusive results of the literature. When priming a general concept of religion, it is impossible to control for the specific religious idea that an individual associates with this multidimensional concept. In particular, it is not possible to account for the ambivalence of the sacred—according to which different religious ideas may have opposing effects on behavior. To the best of our knowledge, our study is the first to focus on the impact of ambiguous religious ideas on prosocial behavior. We thus unravel the mechanisms determining how religion influences altruism and intergroup discrimination.

¹Out of the 51, we count only six cases that do not have a religious dimension to them. The remainder have either an ideational dimension ($n = 12$; e.g., Algeria, Mali, Somalia), an identity dimension ($n = 4$; e.g., Azerbaijan, Myanmar), or both ($n = 19$; e.g., Iraq, Syria, Nigeria). In six further cases, the role of religion could not be ascertained.

²We focus on experimental studies using such techniques too. For overviews of the multidimensional effects of religion on socioeconomic development, see Hoffmann (2013) and Basedau, Gobien, and Prediger (2018). Typical primes that activate religious identity and ideas include the sentence-unscrambling tasks used by Shariff and Norenzayan (2007), which contain words such as *God* or *divine*; situating players in a religious surrounding, such as a temple or church (Ahmed and Salas 2013); or letting participants listen to a prayer call (Aveyard 2014).

We conducted artefactual field experiments³ with nonstudent Christian and Muslim believers in Accra (Ghana) and Dar es Salaam (Tanzania). In contrast to most previous studies, we drew on a large sample size: 1,254 participants. To measure the effect of religious ideas on altruism,⁴ we used a dictator game and tested in a between-subject design a nonreligious control prime versus two treatments: (1) priming the idea that religion means loving all human beings and (2) priming the idea of the existence of only one true religion. To measure discrimination, we adopted a within-subject design: Each participant played the dictator game twice—once with a recipient of the same religion and once with a recipient of a different one. With this design, we can contrast the two religious ideas and explore whether they have an effect on the *general level of altruism* as well as on the intensity and occurrence of *intergroup discrimination*.⁵

We find that neither religious idea has an effect on the general level of altruism displayed. However, religious ideas do matter when it comes to the limits thereof. The idea of universal love leads to more equal treatment of both the religious ingroup and outgroup: It increases the proportion of participants who transfer the same amount of money to both. In contrast, under the one true religion prime, the religious outgroup receives 11.82% lower transfers compared to the ingroup.

The remainder of this article is structured as follows: In the next section, we discuss related research on religion and subsequently develop our hypotheses. The subsequent section outlines the country contexts for Ghana and Tanzania and presents the experimental procedures. We then discuss our results. We close the article by discussing our findings and summarizing their implications.

Related Research, Theorizing, and Hypotheses

Religion and Altruism

Why should religion even be connected to prosocial behavior in the first place? Via the lens of cultural

³We consider our experiment to be an artefactual field experiment in accordance with Harrison and List (2004). Our subjects are nonstudents recruited in natural environments, who are nonetheless aware that they are participating in an experiment.

⁴We measure altruism as one specific form of pro-sociality while ruling out potential reciprocity impulses that can be prevalent in other experiments such as the ultimatum game—in which one player responds conditionally on the behavior of the other.

⁵We take into account that discrimination has two relevant dimensions to it: how many people discriminate and the intensity of discrimination (e.g., see Piazza 2009). Even if only a small number of people discriminate (occurrence), the intensity of discrimination may still be high, or vice versa. This distinction is less relevant for altruism. Here, the overall size of the transfers is crucial.

evolution, Norenzayan et al. (2016) explain the emergence of religions' social functions. Ideas, norms, and practices that increased social cohesion, including the collective practicing of rituals, were historically favored. Aspects of supernaturalism were eventually combined with ones of morality and altruism in such a way that supervision and punishment duties were "outsourced" to supernatural agents (Norenzayan et al. 2016). Punishing or rewarding gods who cared about human morality determined the behavior of humans on Earth, providing the latter with incentives to behave prosocially (Johnson 2015; Norenzayan and Shariff 2008; Preston and Ritter 2013). As such, a connection between religion and altruism is attributed to the belief in supernatural punishment in case of misbehavior (Galen 2012; Purzycki et al. 2016).

On the other hand, religious practices such as confession, divine forgiveness, and absolution may decrease prosocial behavior. Weber referred to the "catholic cycle of sin, repentance, atonement, release, followed by renewed sin" (1904/1905 [2011]). If one's sins are forgiven, the reason to behave prosocially may cease to exist—or one might even have an incentive to sin whenever it suits one's own interests. "Theological determinism" might also work against prosociality: If the world was created by God, He willingly made it that way. Therefore, if humans were not created as prosocial beings, this is also God's will (Vihvelin 2017).

This ambiguity is also reflected in the experimental literature. Several studies show that religious primes increase altruism (Ahmed and Salas 2011; McKay et al. 2011; Shariff and Norenzayan 2007; Xygalatas 2013), cooperation (Ahmed and Salas 2011; Xygalatas 2013), honesty (Mazar and Ariely 2006; Randolph-Seng and Nielsen 2007), or the costly punishment of actions perceived as unfair (McKay et al. 2011). Other studies, however, do not confirm the positive effect of religion on prosocial behavior. Gomes and McCullough (2015) fail to replicate the results of Shariff and Norenzayan (2007)—the pioneering study here—when conducting dictator games with a larger sample of student and nonstudent participants from the United States. Likewise, a recent large-scale replication of the study by Mazar and Ariely (2006) did not observe positive effects of priming religious concepts on honesty (Verschuere et al. 2018). Similarly, Benjamin, Choi, and Fisher (2016) do not observe any effects of religious primes on prosocial behavior in laboratory dictator games, even though they find a positive effect on contributions within public goods games. In their experimental study in Ghana, Parra, Joseph, and Wodon (2016) observe that priming religious concepts even reduces transfers in dictator games.

These inconclusive results might be explained by the fact that most studies using priming techniques activate the concept of religion *per se* (see Table 1 for more information on the primes used by different studies); none used religious primes that are explicitly connected to altruism, however. Often, they did not provide any specific content, but rather primed single words or neutral sentences. Herewith, it is not possible to control which particular religious idea exactly becomes salient to the primed person.

Religious scripts include, for example, rules for different aspects of life—such as prayer requirements or dietary regulations—not directly connected to prosocial behavior. Other ideas, such as the Protestant ethic of self-responsibility for earthly matters, might even decrease altruism. However, numerous other religious ideas, especially in the Abrahamic faiths, emphasize love, morality, and prosociality (e.g., in the Quran, “God loves the doers of the good deeds,” 3:133; in the Bible, “Whoever does not love does not know God, for God is love,” 1 John 4:8). Such ideas of universal love may activate unconditional prosociality toward a third person. We therefore do not expect that activating religion *per se* increases altruism, but rather:

H1: The religious idea of universal love increases unconditional altruism relative to a neutral, nonreligious idea.

Religion and Intergroup Discrimination

A growing body of research looks at the connection between religion and intergroup discrimination. Social identity theory argues that people tend to favor their ingroup when group identity is made salient (Tajfel et al. 1971). The existence of a religious identity may already result in stereotypes and prejudices toward other religious groups, leading to intergroup discrimination (Allport 1954). Furthermore, as mobilization theories argue, leaders can capitalize on social identities such as religious ones to gain political support, which can, under certain circumstances, lead to violent conflict (Basedau, Pfeiffer, and Vüllers 2016; Walter 2017). Here, characteristics such as the size of the religious group, the structure of its hierarchy, and individual religious beliefs play a role (Philpott 2007). Although the social cohesion effect of religion may cause greater ingroup favoritism, on the one hand, it may also facilitate outgroup discrimination, on the other.⁶

⁶While ingroup favoritism does not imply outgroup discrimination *per se*, it can be a driver thereof (Allport 1954; Balliet, Wu, and De Dreu 2014; Brewer 1999).

Empirically, most studies aim at exploring whether intergroup bias between different identity groups—like religious ones—exists. Previous studies on religious intergroup bias have come to inconclusive results (see Table A1 in the supporting information [SI]). In their experiments, Chuah, Fahoum, and Hoffmann (2013) and Chuah et al. (2016) find outgroup biases between Muslims and Hindus in India, China, Malaysia, and the United Kingdom. Similarly, Chakravarty et al. (2016b) provide some evidence of outgroup bias between Hindus and Muslims in cooperation games in India. At the same time, the authors do not find consistent evidence in noncooperative games (Chakravarty et al. 2016a), and Johansson-Stenman, Mahmud, and Martinsson (2009) similarly find no evidence for intergroup bias among religious groups in Bangladesh. Gupta et al. (2018) conclude that it is not religious identity but rather minority versus majority status that is responsible for intergroup bias.

Previous studies have not looked at the ambivalent content of religious ideas and how they relate to interreligious discrimination. Ideas of universal love may activate prosociality independently of the other person's religious identity and thus prevent discrimination. Yet, monotheistic and missionary religions' claim to universal validity comes at a price (Assmann 2009). These religions usually distinguish between a religious ingroup, which believes in the one true religion, and an outgroup, which follows the false faith. Such missionary monotheism is a central feature of the world's biggest faiths, Christianity and Islam. Examples can be found in the Quran—“O you who believe! Fight those of the unbelievers who are near to you and let them find in you hardness” (9:123)—or in the Bible: “If your very own brother, or your son or daughter, or the wife you love, or your closest friend secretly entices you, saying, ‘Let us go and worship other gods’ [. . .], do not yield to them or listen to them. Show them no pity. Do not spare them or shield them. You must certainly put them to death” (Deuteronomy 13).

Such notions of only one true religion call for negative behavior toward other religious groups—in sharp contrast to the idea of universal love. For this reason, we expect the following:

H2a: The idea of universal love reduces outgroup discrimination relative to a neutral, nonreligious idea.

H2b: The idea of one true religion increases outgroup discrimination relative to a neutral, nonreligious idea.

TABLE 1 Overview of Experimental Priming Studies on Prosociality

Author(s)/Year/ Sample Size	Treatments and Religious Prime	Prime Description	Operationalization of Prosociality	Results
Preston and Ritter (2013)—Study 3, n = 85	T1: religion (prime: religion), T2: God (prime: God), T3: control (prime: broccoli/hat)	Participants see the religious/control primes on a computer screen	Prisoner's Dilemma (cooperation)	No general effect on cooperation; God prime increases cooperation with outgroup; religion prime increases cooperation with ingroup
Parra, Joseph, and Wodon (2016), n = 393	T1: religion (photos of Christian/ Muslim believers praying and other religious symbols), T2: control (photos of fruits)	Participants see photos of the religious/control primes.	Dictator Game (altruism)	Religious primes reduce general prosociality. Priming for religion reduces the allocations to participants with a different religious affiliation
Shariff and Norenzayan (2007), n = 50 in Study 1, n = 78 in Study 2	S1–T1: religion (prime: spirit, divine, God, sacred, prophet), T2: no prime S2–T1: religion (same prime as S1), T2: secular (prime: civic, jury, court, police, contract), T3: control (the words are not reported)	Participants unscramble sentences involving religious words	Dictator Game (altruism)	Religious primes increase general prosociality
Ahmed and Salas (2011), n = 224	T1: religion (prime: spiritual, divine, benediction, holy, Jerusalem, God, Jesus, prophet), T2: control (prime e.g.: palm, sofa, beach, food)	Participants unscramble sentences involving religious words	Dictator Game + Prisoner's Dilemma (altruism and cooperation)	Religious primes increase general prosociality
Gomes and McCullough (2015), n = 650	T1: standard religious prime (same prime as S&N in a sentence with religious context), T2: enhanced religious prime (same prime as S&N in a sentence with nonreligious context), T3: control (prime e.g.: fall, shoes, train, sky)	Participants unscramble sentences involving religious words	Dictator Game (altruism)	No effect on general prosociality
Benjamin et al. (2016), n = 817	T1: religion (same prime as S&N), T2: control (same prime as S&N)	Participants unscramble sentences involving religious words	Public Goods Game + Dictator Game (cooperation and altruism)	No effect on general prosociality in DG, more contributions in PGG (not for Catholics)
Xygalatas (2013), n = 62	T1: experimental venue is a temple, T2: experimental venue is a restaurant	Participants either play in a temple or in a restaurant	Bargaining game	Participants in the temple are more prosocial
McKay et al. (2011), n = 304	T1: religion (primes: divine, holy, pious, religious), T2: punishment (primes: revenge, punish, penalty, retribution), T3: religion–punishment (primes: divine, revenge, pious, punish), T4: control (primes: northeast, acoustic, tractor, carton)	Words appear on a computer screen	Punishment of unfair behavior	Religious primes increase the costly punishment of unfair behaviors for those participants who had previously donated to a religious organization

(Continued)

TABLE 1 Continued

Author(s)/Year/ Sample Size	Treatments and Religious Prime	Prime Description	Operationalization of Prosociality	Results
Randolph-Seng and Nielsen (2007), n = 52 in Study 1, n = 54 in Study 2	S1–T1: religion (primes: heaven, bless, gospel, cross, faith, prayer, salvation, saved, holy, worship), T2: sports (primes not reported), T3: neutral (primes not reported) S2–T1: religion (primes: same as in S1 and, additionally, baptism, amen, church, resurrection, commandments, communion, saint, prophet, sabbath, preacher), T2: neutral (primes not reported)	Participants unscramble sentences involving religious words	Cheating	Religiously primed group cheats less
Mazar and Ariely (2006), n = 229 in Study 1	T1: moral reminder (writing down 10 Commandments) T2: no moral reminder (writing down names of books)	Participants write down what they remember of the 10 Commandments	Cheating	Religious prime significantly reduces cheating
Verschuere et al. (2018), n = 5,786	Same as Mazar and Ariely (2006), S1	Participants write down what they remember of the 10 Commandments	Cheating	No effect of religious prime

Research Context and Experimental Design

Context

We conducted artefactual field experiments in two countries in sub-Saharan Africa: Ghana and Tanzania. In Ghana, the majority of the population is either Christian (75%) or Muslim (16%; Afrobarometer 2018a). The majority of Muslims live in the northern part of the country, whereas Christianity has spread through the coastal areas in the center and south. Tanzania does not collect census data on the distribution of faiths. However, sources suggest that approximately 61% of believers are Christian and approximately 30% are Muslim (Afrobarometer 2018b). Although Islam originally spread from the coastal regions to the inland of the country, a clear geographical separation of predominantly Muslim or Christian regions is not possible; Julius Nyerere's "Ujamaa" policy of relocating citizens led to a blending of religions and ethnicities there in the 1980s.

We conducted experiments in the largest city of each country: Accra in Ghana and Dar es Salaam in Tanzania. In Accra, Christians form the majority, and Muslims constitute the largest minority. In Dar es Salaam, division is more equal. Both cities can be considered melting pots

for the whole country, attracting migrants from rural areas. Whereas Dar es Salaam's religious composition has traditionally been characterized by a comparably large number of Muslim believers, in Accra, Muslim believers have mostly migrated to the city. In both countries, religious practice plays a major role in the lives of most: According to surveys, 77% of the population in Ghana and Tanzania attend a religious service at least once a week (Afrobarometer 2014).

Both country cases represent examples of the rather peaceful coexistence of a Christian majority and a Muslim minority. In Ghana, there have been no major conflicts between Christian and Muslim believers, and interactions between the two religious groups work well (Religious Freedom Report 2016). Tanzania's everyday interactions between the religions also seem generally peaceful and tolerant, though there have been recurring conflicts (Heilman and Kaiser 2002). Recently, attacks on churches and mosques have been slightly on the increase (Religious Freedom Report 2016). Findings on religious discrimination in these relatively peaceful nations can be interpreted as a robust test of our hypotheses compared to more troublesome countries, where discrimination is more likely to occur. In addition, relying on data from two separate countries increases the generalizability of our results.

Experimental Design

We used a dictator game to measure altruism. This is a two-player game in which one person is assigned the role of the “dictator” and endowed with a monetary budget. The dictator decides how to allocate the budget between himself or herself and the second player (“receiver”). Under the standard assumption of the “homo oeconomicus model,” rationally acting dictators would keep the entire amount for themselves, as this maximizes their personal gain. Positive transfers to the receiver are commonly interpreted as demonstrative of altruistic behavior.

Each participant was randomly assigned to one of three treatments: (1) universal love, (2) one true religion, or (3) the control group. Participants played the role of dictator twice. For one of the two decisions, the receiver⁷ belonged to the same religious group as the dictator; for the other, he or she belonged to a different one. The order used for revealing the religious identity of the receiver was randomized, meaning that some participants first decided how much to give to a receiver from the same religious group, whereas others first decided how much to give to one from a different group (see Figure 1). This randomization allowed us to separate out order effects (i.e., participants’ giving more in the first round than in the second) from differences in giving behavior depending on the receiver’s religious group. Decisions were taken directly after the priming, and in private.

Two features of our experimental design were crucial: (1) how we primed the religious ideas and (2) how we revealed the religious identity of the interaction partner. As noted earlier, we used priming techniques to make the two specific religious ideas salient. The scrambled-sentence task proposed by Shariff and Norenzayan (2007; cf. Ahmed and Salas 2011; Benjamin, Choi, and Fisher 2016; Laurin, Kay, and Fitzsimons 2012) proved in our context not to be feasible, as many of our participants were illiterate. Instead, we chose to prime religious ideas by verbally reading out three statements with which the participants could agree, disagree, or neither agree nor disagree before making decisions. Verbally reading out statements has the additional advantage that it is similar to attending religious services, where content is also verbally transmitted. The two initial statements conveyed content neutral to religion and prosocial behavior,⁸ and they were the same across the control and treatment groups. In

contrast, the third statement transmitted the religious idea and varied between treatments:

Universal love treatment: Some people claim that true religion means loving all human beings. Do you agree, disagree, or are you undecided?

One true religion treatment: Some people claim that their religion is the only true religion and wish that everyone followed their faith. Do you agree, disagree, or are you undecided?

Control treatment: Some people claim that everyone is talented at something, but no one is talented at everything. Do you agree, disagree, or are you undecided?

Our priming approach was relatively subtle. We consider this a strength. If we observed results when using this subtle prime, they would potentially be stronger when priming in a more explicit way and when priming in more conflict-prone contexts.

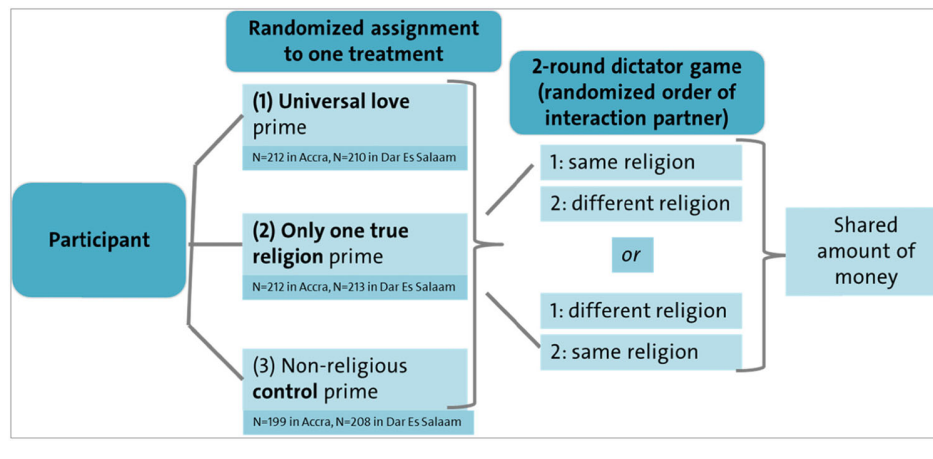
The second relevant design feature was the question of how to reveal the religious identity of the interaction partner. Most studies reveal the interaction partner’s (ethnic or religious) identity by using typical names (Chakravarty et al. 2016b; Chuah, Fahoum, and Hoffmann 2013; Fershtman and Gneezy 2001) or by using names and visual cues simultaneously (Chakravarty et al. 2016b). Prior to our research study, we therefore conducted a survey at a large market in Accra to find out more about the connection between religion and names. This survey revealed that Muslim first names are often associated with that religious group, but that Christian ones are often unrelated to their own respective group. Instead, they are associated with categories such as “Western name” or “friend.” Therefore, we refrained from using this approach.

Other studies explicitly inform participants about the interaction partner’s religion by directly stating his or her religious affiliation (Chuah et al. 2014; Gupta et al. 2018). Providing such information about the recipient’s religious group in a within-subject design has the disadvantage that participants can infer what the study is about and may therefore seek to demonstrate socially desirable behavior. We ultimately chose to give visual information about the interaction partner’s religious group by showing photos of large crowds of people praying in a church or in a mosque directly before the participants were to make the first decision, and then again directly before they made the second. The photos were taken from behind, in such a way that faces could not be recognized (see examples in SI Appendix C). While viewing the photos, participants

⁷Transfers were randomly assigned to participants of a survey conducted in both countries independently of the experiments.

⁸Independently of their treatment, all participants were asked: “Some people claim that the world is better off because of new technologies. Do you agree, disagree, or are you undecided?” and “Some people claim that it is important to think up new ideas and

be creative, to do things one’s own way. Do you agree, disagree, or are you undecided?”

FIGURE 1 Structure of the Dictator Game

were told: “The person you can give money to lives in [Accra/Dar es Salaam] and is someone like those in this picture.” This was meant to imply the religious affiliation of the interaction player (“someone like” was meant to refer to the religious group) without explicitly stating that the photo was connected to religion in any way. Possible associations other than the religious affiliation of the interaction player are “religious person” and “religious service.” This prime is, again, relatively subtle, and we would expect to find stronger effects with more explicit identity revelations.

Experimental Procedures

We ran artefactual field experiments with a total of 623 participants in Accra and 631 in Dar es Salaam, respectively. Fieldwork in Accra was conducted between March 5 and March 20, 2017, whereas that in Dar es Salaam was conducted between July 2 and July 22, 2017. During this period, there was no readily identifiable conflict between Christian and Muslim believers in either of the countries.

The session sizes varied from 20 to 49 participants. In both cities, Muslim participants were recruited in front of mosques after Friday prayer, whereas Christian participants were recruited outside of churches on Sundays after service. They were then invited to the experimental sessions for a different calendar day, to avoid possible contagion effects due to their previous attendance at the religious service. Sessions took 2.5 hours,⁹ on average,

and were held in neutral (nonreligious) schools, community centers, or open spaces. Sessions consisted usually of either Christian *or* Muslim participants. However, participants were informed that their interaction player was not present in the respective session. Note that participants could be affected by the presence of fellow ingroup members in such a way that they show more favoritism thereto. However, as our participants were randomly assigned to the various treatments, this effect would be constant and not affect the respective treatments differently.

In Accra, participants (in the role of dictator) could decide to give up to GHS 10 (USD 7.19 in purchasing power parity [PPP], terms). In Dar es Salaam, participants could decide to give up to TZS 5,000 to the recipient (USD 7.24 in PPP terms). Thus, participants in both countries had 11 choices (in Accra, in increments of GHS 1; in Dar es Salaam, of TZS 500). Figure 1 shows the number of participants per treatment.

We implemented the whole experiment as a pen-and-paper one in the local language (Akan/Ga/Hausa in Ghana; Swahili in Tanzania) in order to account for the aforementioned illiteracy issue. All participants afterward filled in a postexperimental questionnaire. Those participants who could not read and write received help from our local teams. At the end of the experimental procedures, one of the two decisions made in the dictator game was randomly drawn, and participants received their respective experimental earnings, plus a showing-up fee.¹⁰ On average, participants in Ghana earned GHS 38 (USD 27.33 in PPP terms) while in Tanzania the average earnings were TZS 14,000 (USD 20.28 in PPP terms).

⁹The whole experimental session consisted of a dictator game, a prisoner’s dilemma, and a postexperimental questionnaire in that order. As the prisoner’s dilemma was played as a one-shot game without varying interaction partners, we did not measure discrimination in this particular experiment.

¹⁰Participants were informed that one of their decisions from the dictator game would be randomly drawn beforehand (see instructions in SI Appendix B).

Results

Sample Description

Our sample consists of 1,254 experimental participants.¹¹ On average, 50% were female; the mean age was 38.5 years. Of the experimental participants, 49% were Muslims. Religion plays a major role in the life of the people involved; approximately 38% of the participants identified primarily with their religious group.¹² The vast majority of participants across all treatments believe in God/Allah, go to church/the mosque once a week, and pray regularly. They believe in heaven, and that God/Allah punishes bad deeds and rewards good ones. Standard variations for these variables were low, with at least 80% of participants choosing the highest category and nearly all observations being in the two highest categories.

An exception was service attendance, where the two highest categories combined make up only approximately 70% of the observations. Although it is common in both countries to have friends from other religions, participants are unsure about whether interreligious marriage is acceptable. Overall, our randomization into different treatments worked well (see Table 2). However, participants in the one true religion treatment considered interreligious marriage as less acceptable, and those in the universal love treatment thought religious laws were more important than state ones, as compared to the other treatments. We control for all sociodemographic factors in the regression analysis.

SI Figure A2 provides a closer look at the three primes we used in the dictator game. Approximately 90% of the participants agreed with the religious idea of universal love, and with the control prime. The one true religion prime reveals a more diverse picture: Some 57% of participants agreed that there is only one true religion, whereas 42% disagreed.¹³

Religion and Altruism

Across all treatments, participants transferred, on average, 23% of their endowment in Accra and 24% in Dar

es Salaam. We did not observe significant differences in the general level of altruism between the treatments (see Figure 2) or between the two locations.¹⁴ Thus, being reminded of the ideas of universal love or one true religion does not promote more altruism as compared to exposure to a neutral, nonreligious prime (Hypothesis 1).

We use random effects generalized least squares (GLS) regressions for the proportion of endowment sent, as presented in Table 3. Regression 1 shows a basic form thereof for the treatments, including two dummy variables indicating the one true religion and the universal love primes. Regression 2 adds sociodemographic and religious control variables, as well as interviewer dummies. We control for order effects in all regressions. Treatment dummies turned out to be insignificant in both regressions. The small coefficients show that there is virtually no difference across treatment conditions. Adding sociodemographic and interviewer controls (Regression 2) does not change the results.

SI Table A6 presents different model specifications that test for interaction effects regarding Ghana and Tanzania (Regression 1), Muslims and Christians (Regression 2), religion as identity group (Regression 3), and service attendance (Regression 4). All interaction terms turn out to be insignificant. Affirmation of the religious idea conveyed in the treatments also has no significant effect in either (Regression 5). We therefore do not find that religious primes increase the general level of altruism. As such, we cannot support Hypothesis 1, which expects a positive effect of the universal love prime on altruism compared to the control prime.

Result 1: Contrary to Hypothesis 1, we do not find evidence that the idea of universal love increases unconditional altruism relative to a neutral, non-religious idea.

Intensity of Intergroup Discrimination

Next, we look at intergroup discrimination. Considering all treatments and participants, the mean proportion of endowments sent to recipients with the same religion is significantly higher than that transferred to recipients

¹¹Complete sociodemographic information was available for 1,098 participants (the differences across countries can be found in SI Table A2). For robustness, we control for sociodemographic variables in our regression analysis. The complete postexperimental questionnaire can be found in SI Appendix D.

¹²Other identity groups included in this question are per nationality, ethnicity, occupation, and gender.

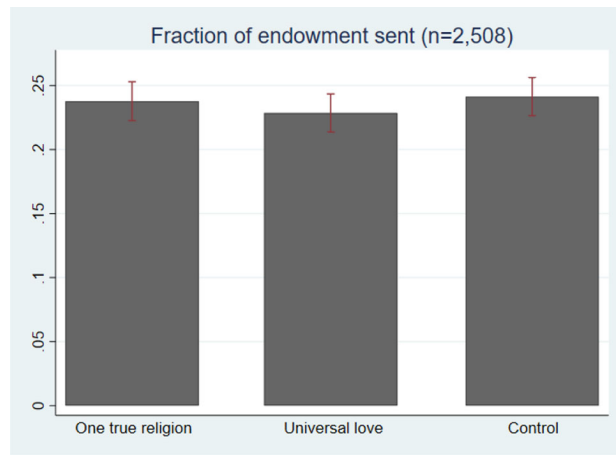
¹³There are no major differences between Accra and Dar es Salaam. Histograms for the two statements prior to the primes and across locations are displayed in SI Figure A3.

¹⁴The results hold true if we restrict the sample to the first round. We do not find any difference between Christians and Muslims. We observe a small effect vis-à-vis round in both locations, with participants sending a higher proportion of their endowment in the first than in the second (Wilcoxon matched-pairs, signed-ranks tests to compare within-participant differences: Accra: 0.24 vs. 0.22, $p = .00$; Dar es Salaam: 0.24 vs. 0.23, $p = .02$). An overview of the distribution of transfers and the average proportion of endowments sent per treatment, round, and location is presented in SI Figure A1 and SI Table A3.

TABLE 2 Sample Description and Randomization

Variables	(1) One True Religion Mean/SE	(2) Universal Love Mean/SE	(3) Control Mean/SE	Test for Differences ^x (1)–(2)	Test for Differences ^x (1)–(3)	Test for Differences ^x (2)–(3)
Female ^a	0.48 (0.03)	0.52 (0.03)	0.49 (0.03)	–0.04	–0.01	0.03
Age	39.20 (0.87)	38.88 (0.83)	37.50 (0.84)	0.32	1.69	1.38
Education ^b	3.00 (0.10)	2.83 (0.10)	2.95 (0.10)	0.17	0.06	–0.12
Perceived living condition ^c	2.89 (0.06)	2.80 (0.06)	2.90 (0.06)	0.09	–0.01	–0.10
Muslim faith ^a	0.47 (0.03)	0.51 (0.03)	0.49 (0.03)	–0.03	–0.02	0.02
Belonging first and foremost to religious group ^a	0.38 (0.03)	0.40 (0.03)	0.37 (0.03)	–0.01	0.02	0.03
Belief in God/Allah ^d	4.91 (0.02)	4.92 (0.02)	4.94 (0.02)	–0.00	–0.03	–0.02
Attendance of religious service ^e	3.83 (0.06)	3.90 (0.06)	3.87 (0.06)	–0.07	–0.04	0.03
Practice of prayer ^f	4.90 (0.02)	4.89 (0.03)	4.89 (0.02)	0.01	0.01	0.00 ⁺
Belief in heaven ^a	0.95 (0.01)	0.97 (0.01)	0.96 (0.01)	–0.02	–0.01	0.01
Belief in hell ^a	0.92 (0.01)	0.91 (0.01)	0.90 (0.02)	0.00	0.01	0.01
God/Allah punishes bad deeds ^g	4.80 (0.03)	4.80 (0.03)	4.77 (0.03)	–0.01	0.02	0.03
God/Allah rewards good deeds ^g	4.82 (0.03)	4.81 (0.03)	4.75 (0.03)	0.01	0.07	0.06
Religious laws more important than state laws ^g	3.84 (0.07)	4.05 (0.06)	3.86 (0.07)	–0.21*	–0.02	0.19*
No marriage between different religions ^g	2.93 (0.08)	3.15 (0.08)	3.08 (0.08)	–0.21*	–0.14	0.07
Friends from other religions ^a	0.90 (0.02)	0.88 (0.02)	0.89 (0.02)	0.02	0.02	–0.01
N	369	366	357			

Notes: (a) Dummy variable: 1 = yes, 0 = no; (b) 7 = postgraduate, 6 = completed postsecondary, 5 = some postsecondary, 4 = secondary completed, 3 = some secondary, 2 = primary completed, 1 = some primary, 0 = no schooling; (c) In general, how do you rate your living conditions compared to those of other people in your country? 1 = much worse, 2 = worse, 3 = same, 4 = better, 5 = much better; (d) 5 = I am sure that God/Allah really exists, 4 = Although I sometimes question his existence, I do believe in God/Allah, 3 = I don't know if there is a personal God, but I do believe in a higher power of some kind, 2 = I don't know if there is a personal God or a higher power of some kind, 1 = I don't believe in a personal God or in a higher power; (e) 5 = a few times a week, 4 = once a week, 3 = once a month, 2 = few times a year, 1 = never; (f) 5 = prayer is a regular part of my daily life, 4 = I usually pray in times of stress or need but rarely at any other time, 3 = I pray only during formal ceremonies, 2 = prayer has little importance in my life, 1 = I never pray; (g) 5 = strongly agree, 4 = agree, 3 = uncertain/neither agree nor disagree, 2 = disagree, 1 = strongly disagree. (x) We use a test of proportions for dummy variables and a Mann-Whitney test for all other variables. The value displayed for the tests are the differences in the means across the groups. ***, **, *, and + indicate significance at the 0.1, 1, 5, and 10% level, respectively.

FIGURE 2 Average Fraction of Endowment Sent over Both Rounds

with a different faith (0.24 vs. 0.23, $p = .02$).¹⁵ The lower transfers to recipients with a different religion are driven by the one true religion treatment. Here, the difference increases in magnitude and becomes statistically significant at the 0.1% level (0.22 vs. 0.25, $p = .002$). We find the same tendency when we divide our sample according to the two locations.¹⁶ In the universal love and the control treatments, the difference between transfers to recipients with the same religion as opposed to a different one is insignificant and close to 0.¹⁷

The regressions in Table 3 include a dummy variable that becomes 1 if the sender and the recipient in the dictator game have the same religion, and 0 otherwise. We use this dummy to test whether participants discriminate with respect to the recipient's religion. The dummy is positive and significant in Regressions 1 and 2. A coefficient of 0.01 means that the proportion of the endowment sent to a recipient with the same religion increases by 4.3% on average compared to the average proportion of 0.23 sent to one with a different religion. Table 3 also presents heterogeneous treatment effects depending on whether the interaction player comes from the same or a different religion (Regressions 3 and 4). Regression 3 shows pure

treatment effects, whereas Regression 4 adds additional control variables.

The interaction term between the one true religion dummy and the same religion dummy turns out to be positive and significant in Regressions 3 and 4.

Therefore, the difference in the proportions sent—that is, the intensity of discrimination—increases under the one true religion treatment, thereby supporting Hypothesis 2b. The coefficient of 0.03 in Regression 3 means that the difference in the proportion of the endowment sent increases on average by 12.5% compared to the average proportion of 0.24 sent in the control treatment. The dummy for the universal love prime is insignificant. Regressions 5 and 6 represent Tobit models with censoring at 0 and 1 (as participants cannot give less than 0 or more than 1 in Round 2). The results remain robust, and the interaction term between the one true religion dummy and the same religion dummy increases in size.

In SI Table A8, we explore the impacts of agreeing with the primes. The interaction term between the one true religion treatment (i.e., having an interaction player of the same religion) and agreement with the one true religion prime becomes (marginally) significant. Thus, the one true religion prime especially affected the behavior of participants who agreed with its content.

Result 3: Supporting Hypothesis 2a, the one true religion prime increases the intensity of intergroup discrimination as compared to a neutral, nonreligious idea.

Result 4: Opposing Hypothesis 2b, the universal love prime does not significantly affect the intensity of intergroup discrimination as compared to a neutral, nonreligious idea.

Occurrence of Intergroup Discrimination

We next turn to the occurrence of discrimination. Table 4 shows the percentage of participants who sent a lower transfer (negative discrimination) or a higher transfer (positive discrimination) to recipients with a different religion compared to those from the same religion, as well as participants who did not discriminate with regard to the recipient's religion (no discrimination). First, we see that the majority of participants across all three treatments did not discriminate on the basis of the religious identity of the receiver. Second, and somewhat surprisingly, we find a sizable share of individuals who positively discriminated—that is, they sent more to a person of a different faith than to a recipient of the same one.

¹⁵See SI Table A4. We do not observe a significant difference in discrimination between Muslims and Christians.

¹⁶Accra: same religion = 0.26, other religion = 0.23, $p = .001$; Dar es Salaam: same religion = 0.24, other religion = 0.22, $p = .24$.

¹⁷Our results are unlikely to be determined by ethnicity. Participants in the control group who did not receive a religious prime did not discriminate (see SI Table A4 and SI Table A5). If ethnicity was the driver of differences in giving behavior, we would also find such differences in the control group. For an extensive discussion about ethnicity and religion as drivers of conflict, see McCauley (2017).

TABLE 3 Proportion of Endowment Sent (1)–(4) Random Effects GLS Regressions, (5)–(6) Tobit Regressions with Censoring at Zero and One

Variables	(1)	(2)	(3)	(4)	(5)	(6)
One true religion	–0.01 (0.01)	0.00 (0.01)	–0.02 (0.02)	–0.01 (0.02)	–0.03 (0.02)	–0.01 (0.02)
Universal love	–0.01 (0.01)	0.00 (0.01)	–0.01 (0.02)	0.01 (0.02)	–0.03 (0.02)	–0.00 (0.02)
Same religion	0.01*** (0.00)	0.01* (0.00)	0.00 (0.01)	0.01 (0.01)	0.01 (0.01)	0.01 (0.01)
One true religion * Same religion			0.03* (0.01)	0.02+ (0.01)	0.03* (0.01)	0.03+ (0.01)
Universal love * Same religion			–0.00 (0.01)	–0.01 (0.01)	–0.00 (0.01)	–0.01 (0.01)
Dummy round 2	–0.02*** (0.00)	–0.02*** (0.00)	–0.02*** (0.00)	–0.02*** (0.00)	–0.02*** (0.01)	–0.02*** (0.01)
Constant	0.29*** (0.04)	0.24+ (0.14)	0.30*** (0.04)	0.24+ (0.14)	0.26*** (0.04)	0.18 (0.17)
Session controls	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer controls	No	Yes	No	Yes	No	Yes
Sociodemographic controls	No	Yes	No	Yes	No	Yes
Observations	2,508	2,196	2,508	2,196	2,508	2,196
N	1,254	1,098	1,254	1,098	1,254	1,098

Notes: We control for order effects and include a dummy for Dar es Salaam in all regressions. We also control for session effects in all regressions as multi-lelling models showed between-session variation. We cluster standard errors at the individual level. Regressions (2), (4) and (6) further include interviewer, sociodemographic and religious controls. The results are presented in Table A7 in the appendix. Regressions (5) and (6) are Tobit models with double censoring at zero and one. Standard errors in parentheses; ***, **, *, and + indicate significance at the 0.1, 1, 5, and 10% level, respectively. Table A9 and Table A10 show regressions for round one and round two separately.

TABLE 4 Occurrence of Discrimination: Descriptives

Variable	(1) One True Religion Mean/SE	(2) Universal Love Mean/SE	(3) Control Mean/SE	Test for Differences (1)–(3)	Test for Differences (2)–(3)
Positive discrimination	0.15 (0.02)	0.15 (0.02)	0.19 (0.02)	–0.05+ (0.03)	–0.04 (0.03)
No discrimination	0.62 (0.02)	0.68 (0.02)	0.59 (0.02)	0.03 (0.03)	0.09* (0.03)
Negative discrimination	0.23 (0.02)	0.17 (0.02)	0.21 (0.02)	0.02 (0.03)	–0.05+ (0.03)
N	425	422	407		

Note: We use a test of proportions for dummy variables. The value displayed for the tests are the differences in the means across the groups. ***, **, *, and + indicate significance at the 0.1, 1, 5, and 10% level, respectively.

Across all treatments, 16% of the participants engaged in such positive discrimination.¹⁸

¹⁸Positive discrimination occurred partly due to a round-specific effect, driven mainly by participants who did not care about the religious identity of their interaction partner but who were paired with someone of a different religion in Round 1 (the round in which they gave higher proportions). If we exclude these participants from

Third, we observe differences in the occurrence of discrimination across treatments. We observe a marginally lower occurrence of negative discrimination and a significantly higher occurrence of no discrimination in the universal love treatment, as compared to the

the analysis, we observe 10.7% positive discrimination, 65% no discrimination, and 24% negative discrimination.

TABLE 5 Occurrence of Discrimination (Multinomial Logistic Regression for Positive, No, or Negative Discrimination, Marginal Effects)

Variables	(1) Positive	(2) No	(3) Negative	(4) Positive	(5) No	(6) Negative	(7) Positive	(8) No	(9) Negative
One true religion	-0.04 ⁺ (0.02)	0.03 (0.03)	0.01 (0.03)	-0.04 (0.03)	0.02 (0.04)	0.01 (0.03)	-0.03 (0.03)	0.03 (0.04)	-0.01 (0.04)
Universal love	-0.04 (0.02)	0.09*** (0.03)	-0.05 ⁺ (0.03)	-0.03 (0.02)	0.10*** (0.03)	-0.07*** (0.03)	-0.02 (0.03)	0.10* (0.04)	-0.08* (0.03)
Tanzania	0.08 (0.08)	-0.11 (0.10)	0.03 (0.08)	0.11 (0.11)	-0.26* (0.13)	0.16 (0.10)	0.17 (0.13)	-0.26 ⁺ (0.15)	0.09 (0.13)
Same religion in round 1	-0.11*** (0.02)	0.05 (0.03)	0.07*** (0.02)	-0.13*** (0.02)	0.07* (0.03)	0.06*** (0.02)	-0.21*** (0.03)	0.04 (0.04)	0.17*** (0.03)
Session controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Interviewer controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Sociodemographic controls	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
N	1,254	1,254	1,254	1,108	1,108	1,108	832	832	832

Notes: We include a dummy for Dar es Salaam and a dummy that becomes one if the recipient in the first round belongs to the religious in-group in all regressions. Regressions (4)–(9) include sociodemographic and religious control variables, as well as interviewer and session dummies. The results are presented in Table A11 in the appendix. All regressions use standard errors clustered at the individual level. Standard errors in parentheses; ***, **, *, and + indicate significance at the 0.1, 1, 5, and 10% level, respectively.

control treatment. As we cannot control for order effects when only considering descriptives (i.e., being matched with an interaction player from a different religious group in Round 1, and at the same time giving more in Round 1 than in Round 2), we most likely overestimate positive discrimination in Table 4. We control for these order effects in the regression analysis.

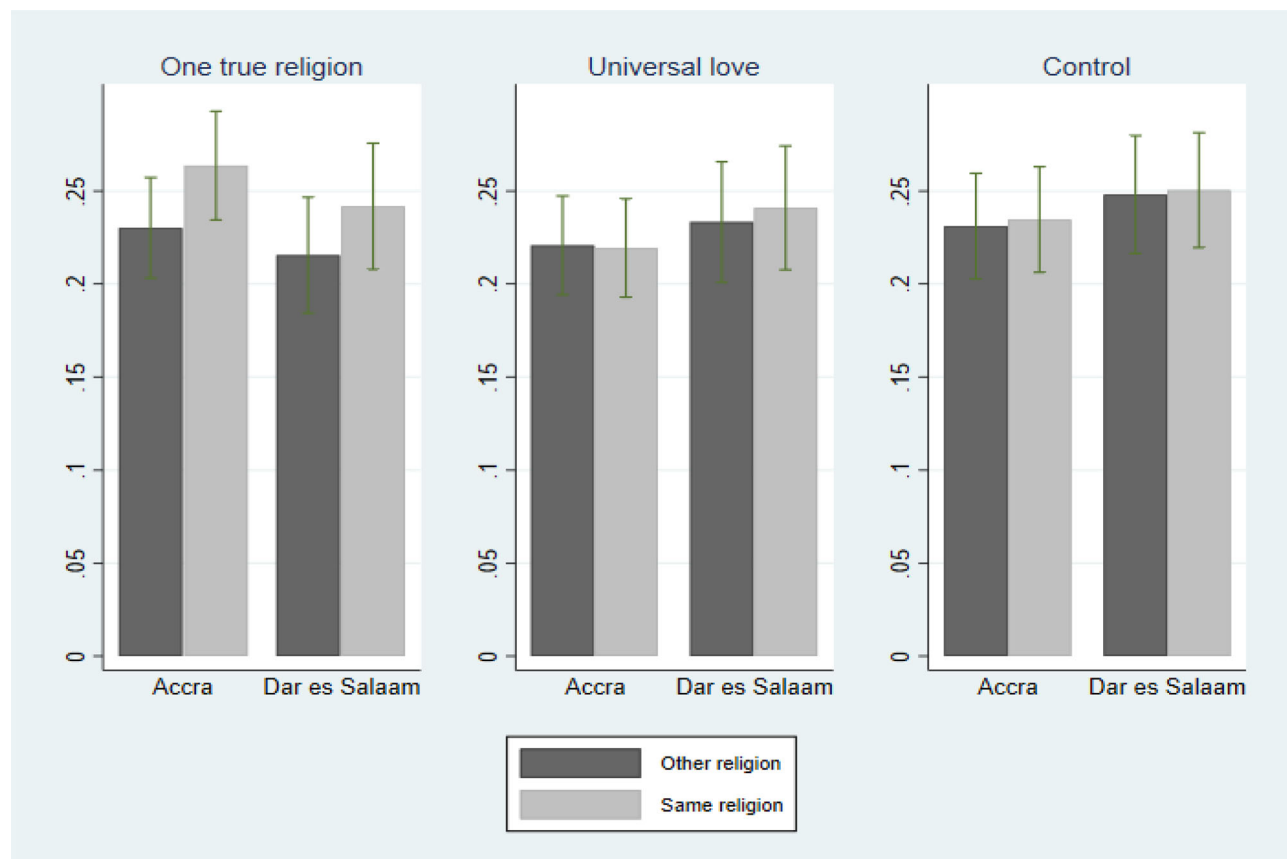
Using a multinomial logit regression, we take a closer look at the question of whether the occurrence of discrimination is influenced by the respective religious treatments (see Table 5). Our dependent variable includes positive, no, or negative discrimination. Regressions 1–3 in Table 5 show pure treatment effects, whereas Regressions 4–9 add additional control variables.

While we find only marginally significant effects of the one true religion prime on positive discrimination, the universal love prime significantly increases equal treatment and tends to counteract both forms of discrimination. This effect is particularly strong for the occurrence of negative discrimination (Regressions 3 and 6), which significantly declines relative to the control group. Regressions 7, 8, and 9 exclude all participants who gave 0 or 100% of their endowments in Round 1 and therefore cannot discriminate by giving less than 0% or more than 100% in Round 2. These specifications considerably restrict our sample. Our results are robust: For the universal love treatment, the coefficients confirm the positive significant effect on no discrimination and the significant negative effect on negative discrimination.

Result 5: Somewhat supporting Hypothesis 2a, the one true religion prime marginally reduces the occurrence of positive discrimination as compared to a neutral, nonreligious idea but does not affect negative or no discrimination.

Result 6: Supporting Hypothesis 2b, the universal love prime increases the occurrence of no discrimination and decreases negative discrimination as compared to a neutral, nonreligious idea. It does not affect positive discrimination.

In terms of intergroup discrimination, the *occurrence* (see Table 5) and the *intensity* (see Table 3) of discrimination are both important. The occurrence of discrimination refers to the general attitudes of a given society in establishing differences between religious groups. Ideas such as universal love can reduce the number of people who partake in discrimination. Here, the idea of one true religion does not significantly affect intergroup discrimination—at least not in peaceful countries like Ghana and Tanzania. However, among the (smaller) number of discriminators (i.e., people who possibly are already prejudiced against other groups) emphasizing the idea of one true religion leads to more intense discrimination. Thus, the idea of universal love can reduce the number of discriminators, whereas that of the one true religion may lead to more intense intergroup discrimination among those already prone to discriminating.

FIGURE 3 Proportion of Endowment Sent in Accra and Dar es Salaam

Differences between Ghana and Tanzania

We also explore the differences between Ghana and Tanzania, as country context may matter. Figure 3 shows a bar graph for the mean proportion of the endowments sent under all treatments. In particular, the differences between the one true religion treatment and the control treatment are interesting here.

In Accra, recipients from the other religion received, on average, the same amount as the control group did (control = 0.23, true religion = 0.23, $p = .99$). Recipients from the same religion received a higher proportion of the endowment as compared to the control treatment (not statistically significant: control = 0.23, true religion = 0.26, $p = .19$). In Dar es Salaam, recipients from the other religion received a significantly smaller proportion of the endowment as compared to the control group (control = 0.25, true religion = 0.22, $p = 0.07$), whereas one from the same religion received similar proportions (control = 0.25, true religion = 0.24, $p = .25$). This hints at a somewhat different discrimination structure in the two locations. Participants receiving the one true religion prime in Accra appear to have given higher proportions

to ingroup members, whereas those in Dar es Salaam appear to have transferred lower proportions to members of other religious groups as compared to members of the control group.

We suggest the following explanation for this: In Tanzania, tensions between Muslims and Christians have been increasing in recent decades (e.g., Rukyaa 2010). Based on reporting by the U.S. State Department, there have been a number of violent incidents there since 1990; in Ghana, meanwhile, there have been almost none. This tension at the macro level may be transferred into negative discriminatory behavior at the micro level in Tanzania when religious ideas are activated. The participants' answers in the postexperimental questionnaire somewhat support this claim (see SI Table A2). Participants in Dar es Salaam considered marriage across religions less acceptable than those in Accra did. These tentative explanations should be tested in further studies, for example, in more conflict-prone countries.

The differing views in both countries on intermarriage underscore the political relevance of our findings. We find that people who agree with the idea of one true religion are not only more discriminating (see SI Table

A8) but also hold other hostile and fundamentalist views such as the superiority of religious over state laws.¹⁹ Such perspectives are considered part of an extremist mind-set that also fosters violence (e.g., Koopmans 2015; Obaidi et al. 2018).

Discussion and Conclusion

In this article, we have argued that the specific content of religious ideas affects altruism and intergroup discrimination—a pertinent topic given the continued political relevance and increasingly contentious nature of religion today. To date, the literature on the social effects of religion has delivered only inconclusive results. One reason for this ambiguity is that previous work has primed a general concept of religion, rendering it impossible to control for the individual interpretation of religion activated by that prime. Different religious ideas might, however, have varying effects on altruism and discrimination. To help address this crucial research gap, we conducted dictator games with Christians and Muslims in Ghana and Tanzania to test the effect of two contradictory and prominent religious ideas: namely, universal love and the notion of there being only one true religion. To the best of our knowledge, our study is the first to account for the “ambivalence of the sacred” and to begin to unravel the mechanisms behind how religious ideas influence altruism and interreligious relations.

Our results partly confirm the respective hypotheses that we put forward: We find that neither religious idea affects the level of unconditional altruism, but that both show distinct effects on interreligious discrimination. We also reveal that the idea of one true religion leads to more intense outgroup discrimination. At the same time, we do not find any effect of the idea of universal love on the intensity of discrimination. However, this idea does lead to a higher share of participants opting for equal treatment of the religious in- and outgroups.

Even though our effect sizes are not high, they are nevertheless important. First, we would expect the primes’ effects to be larger when conveyed by real-world religious or political leaders. Second, even though giving 2% of the endowment can be considered small, it is almost 11% of the average endowment shared—and thus actually quite large when put into perspective. The average effect size for religious discrimination in other studies is usually rather small; in his meta-study, Lane

(2016) identifies an average effect of 0.034. Third, Ghana and Tanzania are examples of the relatively peaceful coexistence between followers of different religions. It is likely that our primes would lead to stronger results in more conflict-prone contexts.

In our setting, we cannot rule out social desirability bias: the tendency of certain people to act in a way that portrays them in a favorable light before themselves or others. Although we sought to minimize such biases by creating anonymous decision-making situations and not revealing that we were studying interreligious discrimination, we are nevertheless aware of this issue: Participants who may have inferred what the study was about may have also downplayed their wish to discriminate between different religious groups. If this was the case, we would expect more discrimination—meaning stronger effects—without such biases to manifest in all of our experimental treatments.

There are several opportunities for crucial further research here. Our sample includes only Christians and Muslims in Christian-majority/Muslim-minority countries. Christianity and Islam are both missionary monotheistic faiths that are particularly prone to the belief in one true religion—and the resulting effects on discrimination and related conflict (e.g., Assmann 2009). Whether these results hold true for other faiths such as Buddhism and Hinduism or for traditional beliefs in Africa and elsewhere, as well as in more conflict-prone contexts, should be tested. In our study, the receiver’s religious affiliation was always known. Future studies could include a condition with a receiver’s faith being unknown, to test the difference between in- and outgroup discrimination in a more precise manner. Other prominent religious ideas might also influence altruistic, discriminatory, or other forms of socially relevant behavior, such as trust and cooperation. Research is needed to disentangle the effects of religion and ethnicity more carefully, and to address the impact of social desirability bias more thoroughly.

Although challenges and opportunities for future research abound, our results are politically relevant and have concrete implications in the present. We have found convincing evidence that claims vis-à-vis one true religion increase intergroup discrimination. Such a belief is connected to other fundamentalist attitudes and, indirectly, the readiness to use violence. Accordingly, discrimination may have unpleasant effects well beyond the artificial setting of an experiment, and it holds the potential to fuel tensions between religious communities worldwide. Our findings are connected to the debate on an built-in tendency toward intolerance in missionary monotheistic faiths (e.g., Assmann 2009). Yet, we have good news

¹⁹Participants who agree with the one true religion prime are more likely to esteem religious laws over state laws (two-sided t-test = 0.01) and seem to be more conservative regarding interreligious marriage too (two-sided t-test = 0.15).

too. To promote peaceful coexistence between religious groups, it seems promising to incentivize religious teachings that are particularly tolerant and that deemphasize the superiority of one's own religion, stressing instead tolerance toward other faiths and "universal love."

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Appendix A: Supplementary analyses

Appendix B: Experimental instructions

Appendix C: Pictures to inform about religious identity

Appendix D: Postexperimental questionnaire