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Article

Do Protestant Missionaries Undermine Political Authority? Evidence From Peru

Anselm Rink[1]

Abstract

The relation between religious organizations and political authority is notoriously tense. Max Weber argued that this is because both compete over the same resource: human commitment. This article revisits Weber's hypothesis. Specifically, we explore two psychological mechanisms through which Protestant missionaries affect political authority: obedience and persuadability. Exploiting exogenous variation in missionary activity in Peru, we demonstrate that missionaries make converts more obedient, which we attribute to a theological and a social mechanism. Yet, we also find that missionaries make converts less susceptible to persuasion by political authorities because they shift attention from secular topics to questions of theological importance, and endorse a skeptical stance toward the government. Exploiting variation in treatment intensity, we argue that the degree to which political authority is affected depends on a given mission's theological strictness. We arrive at these findings by combining experimental outcomes and process-tracing evidence using Bayesian integration.

Keywords

religion and politics, quantitative methods, Latin American politics

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Do religious organizations undermine political authority? This question arguably lies at the heart of the study of religion and politics (Habermas, 2006; Stepan, 2000). As early as 413 CE, Augustine of Hippo argued that the city of God stood in irreconcilable conflict with the earthly city. To this day, the relation between religious organizations and political authority continues to be tense (Grzymala-Busse, 2012; March, 2013). According to Max Weber (1978), the reason for this rivalry is simple: both spheres compete over the same resource, human commitment, and share similar means to gain compliance. Just as political authority rests on the use of physical coercion, religious organizations, for Weber, rest on the use of psychic coercion.

Protestants have historically taken a particularly critical stance against political authorities. Indeed, the Protestant reformation was partly a product of widespread skepticism over close church–state relations (Witte, 2002). In today's world, Protestants are known as strong critics of political authority—a phenomenon particularly visible in developing countries (Freston, 2008; Marshall, 2009). At the same time, scholars have highlighted Protestantism's historic role in advancing democracy (Woodberry, 2012). Political stability and accountability, too, tend to cluster in countries with high shares of Protestants (Tusalem, 2009).

Many scholars contend that the mechanisms underlying these associations operate at the structural level. Woodberry (2012), for example, argues that Protestant missionaries foster democracy by establishing a civil society, supplying printing resources, and providing education. Others underscore the role of missionaries in advancing economic growth, which in turn correlates with democratic consolidation (Barro, 1999; Blum & Dudley, 2001). However, religious organizations also exhibit effects at the individual-cognitive level (McClendon & Riedl, 2015; Wulff, 1991). Religious organizations shape how individuals relate to authority figures (Purzycki et al., 2016). They also affect what topics believers take interest in (Tirole & Bénabou, 2006).

This article reconsiders the contested relation between Protestantism and political authority. We depart from more structurally framed accounts by honing in on the psychological mechanisms through which missionaries may impact political authorities at the local level. In particular, we explore to what degree Protestant missionaries affect two essential determinants of effective authority: obedience and persuadability (Weber, 2009). On one hand, Protestant missionaries expose converts to highly charismatic preachers and demand submission to God's commands (Reuschling, 2005), which propels obedience. On the other hand, Protestant missionaries rigorously focus on questions of theological importance and are skeptical of state authorities (Casanova, 2011). This, in turn, reduces political authorities' leeway to persuade converts regarding topics of secular interest. The effect of Protestant missionaries on political authority is thus ambiguous.

We test these hypotheses using evidence from agrarian villages in south-eastern Peru. We document near-exogenous variation in missionary activity that exposed villages to two types of missions, Evangelical and Pentecostal, which we compare with control communities, which were not evangelized and remained nominally Catholic. Using structured surveys and experiments, we confirm that both types of Protestant missionaries increase obedience to authority, while also undermining persuadability with regard to secular topics. We solidify these findings by integrating quantitative with process-tracing evidence.

Importantly, we demonstrate that our findings are driven by theological, not structural, mechanisms: The effects are particularly pronounced among villages exposed to Pentecostal missions. These missions differ from their Evangelical counterparts primarily with regard to the intensity of their religious preaching. Services are excessively long and use manifold charismatic elements like speaking in tongues and divine healings. Preachers demand strict obedience to God, and shift converts' attention toward the afterlife. Our study thus adds to recent advances in the study of Pentecostalism (Grossman, 2015; Sperber, 2014).

This article makes three contributions to the literature on religion and politics. First, we bring new theoretical considerations and empirical evidence to the century-old question whether religious organizations undermine political authority. Our hypotheses predict that Protestant missionaries affect authority via two psychological mechanisms—obedience and persuadability—which counteract each other. Second, we present new measurement strategies for these concepts that have heretofore been confined to laboratory settings. Third, we offer the first quasi-experimental analysis of missionaries, which integrates qualitative and quantitative evidence in a Bayesian framework.

Protestant Missionaries and Political Authority

"Authority," writes Sebastian De Grazia (1959), "is a subject indispensable to politics" (p. 321). Effective governance—be it in a democratic or authoritarian system—rests on assertive and persuasive authority figures. Political authority, however, is inherently unstable. At the individual level, citizens may fail to comply with political authority. At the societal level, rival political groups challenge those in power. A particularly stark symbol of a government's difficulty in securing its authority is religious organizations. Indeed, Max Weber, in his book *Economy and Society*, argues that religious and political organizations are bound to be in conflict as both organizations predominantly focus on securing human commitment.

The tense relation between religious groups and state authorities is evidenced in countless historic accounts (e.g., Philpott, 2007, p. 506). Political theorists, too, have explored the tense relationship between religion and state authorities.[1] The rivalry has also generated a significant body of empirical work. Several scholars have assessed how religious organizations may weaken state authorities. Berman (2000), for instance, argues that religious organizations operate as clubs that provide insurance to members. Such clubs render state provision of resources less pivotal (see also Huber & Stanig, 2011). Yet, the interaction between religious and political organizations has also been described in more positive terms (Nasr, 2001; Woodberry, 2012).

However, current research on the relation between religious groups and political authorities suffers from two shortcomings. First, empirical studies have been largely observational and have thus failed to establish causal relationships between religious organizations and political authority. Second, theoretical accounts have not sufficiently explored the psychological mechanisms through which religious groups affect political authority. In fact, the psychological repercussions of religious organizations have received almost no attention in political science (McClendon & Riedl, 2015). This is despite a generally burgeoning interest in political authority.[2]

Defining Political Authority

Before exploring the psychological mechanisms through which Protestant missionaries may affect the relationship between individuals and state authority, we first outline this article's definition of political authority. In the interest of simplicity, we adopt Max Weber's seminal definition. Weber distinguishes between authority/power (*Macht*) and leadership (*Herrschaft*). In this article, we focus entirely on the former concept.

In its most minimal form, Weber (2009) defines authority as "the chance of obtaining the obedience of others to a particular command" (p. 54). However, Weber explicitly states that authority relies on more than obedience. It depends on "a certain minimum of voluntary submission" (Weber, 2009, p. 324). Thus, as Blau (1963) notes, Weber distinguishes between two basic determinants of authority: domination that rests on the *power to command*, and domination that rests on the *ability to influence*. In addition to obedience, effective state authority must therefore count on the persuadability of its citizens.

How do religious groups, and Protestant missionaries in particular, affect the two individual-level psychological determinants of effective authority? In the following, we present theoretical considerations of how missionaries may affect obedience and persuadability. We condense our theoretical considerations using
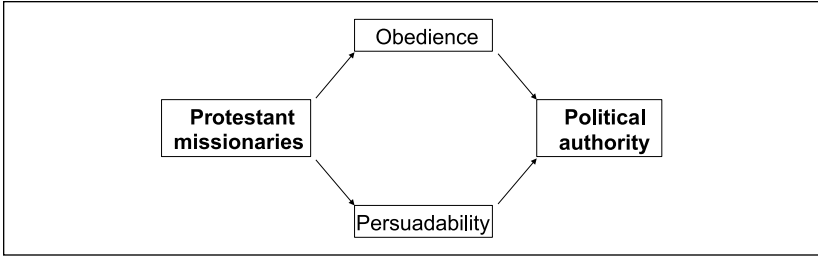
Figure 1. Directed acyclic graph of theoretical model.

a directed acyclic graph (DAG) displayed in Figure 1, which depicts our arguments without making assumptions about the distribution of variables, the functional form, or the magnitude of effects (Elwert, 2013; Pearl, 1995).

Obedience

Obedience describes the behavior of yielding to commands from authority. It is, as Stanley Milgram (1974) writes, "the psychological mechanism that links individual action to political purpose" (p. 1). As such, obedience is a key element of effective governance. Dalton (2008) notes, "even democratic governments emphasize the role of the loyal law-abiding individual as a prime criterion of citizenship" (p. 79). Without obedience, states cannot claim a monopoly of power (Weber, 2009).

Importantly, obedience is also a concept at the core of Christianity. The New Testament includes numerous passages that demand obedience toward God and worldly leaders. The apostle Paul, for instance, states, "everyone must submit himself to the governing authorities, for there is no authority except that which God has established" (Rom. 13:1-2). Similarly, in Peter 2:13-14, it is written "[s]ubmit yourselves for the Lords sake to every human authority: whether to the emperor, as the supreme authority, or to governors, who are sent by him to punish those who do wrong and to commend those who do right." The importance of obedience was clear to the missionaries of Latin America, too. In 1553, Ignatius of Loyola told the Jesuit missionaries: "I desire that you, in more than any other virtue, excel especially in obedience."

Protestant missionaries arguably have a particularly close relation to obedience. In a speech given at the World Mission Conference in 1964, William Crane (1965) reminded his fellow practitioners that "obedience requires meaningful Christian presence in the world where our Lord sends us" (p. 331). Reuschling (2005) argues that this focus on obedience is due to

missionaries' focus on the supernatural, and their reliance on charismatic leaders. Both phenomena give rise to two submechanisms: a *theological* and *social* mechanism, which we discuss in turn.

First, Protestant missionaries *theologically* instruct converts that obedience to God is an essential virtue for Christians. Gill (2004), for example, observes, "Protestantism (and in particular Pentecostalism) favours direct connections with God" (p. 3). As a consequence, notes Reuschling (2005), there is a strong "experiential component in how Evangelicalism is defined with regard to one's personal acceptance of . . . Jesus Christ" (p. 61). Max Weber (1978), too, highlights that "Protestantism (teaches) the principle of loyal fulfillment of obligations" (p. 544). Martin Luther—the seminal figure of the Protestant reformation—too, placed great emphasis on obedience. In his early work *Von Weltlicher Obrigkeit, Wie Weit Man Ihr Gehorsam Schuldig Sei*, Luther (1967), citing Peter (2:13-14), demands that "At first, we must establish the primacy of secular law and the sword" (p. 7).[3] Taken together, there are thus strong theological reasons for Protestant missionaries to instruct converts to obey the supernatural (Gill, 1994). Our own qualitative interviews, discussed below, buttress this argument.

Second, Protestant missionaries *socially* accustom converts to authority figures. Protestant missionaries have a particular tendency to rely on charismatic local leaders, often labeled "religious entrepreneurs." Branded, among other things, as "benign and principled crusaders" (Lankina & Getachew, 2013, p. 103), they rely on strong personalities to spread the gospel. These leaders, as Reuschling (2005) notes, "have positional power as a result of their status, both assigned and ascribed, in evangelical organizations and churches" (p. 65). Protestant leaders construct power by being supreme interpreters of the Bible. This setup, thus, fosters a habit among converts to obey authority figures. Max Weber, too, noted this phenomenon early on. He writes, "preaching achieves the greatest significance in Protestantism, in which the concept of the priest has been supplanted altogether by that of the preacher" (Weber, 1978, p. 464). Again, Pentecostals have a particular tendency to rely on charismatic leaders given their theological strictness and high levels of charismaticism (Anderson, 2013; Meyer, 2004).

Both arguments give rise to our main obedience hypothesis (H1), namely, that Protestant missionaries increase obedience to authority figures among converts. But, to what extent are these mechanisms exclusively a result of Protestant proselytism? Both the theological and social mechanism could arguably be activated by alternative missionaries or other organized groups. Regarding the theological mechanism, several scholars have noted that Islamic proselytism (*Dawah*), too, places a strong theological emphasis on obedience to God. For example, Poston (1992) writes, "[t]he starting point

and core of da'wah consists of . . . obedience to the Messages of God" (pp. 84-85). Regarding the social mechanism, psychologists have long argued that submission to authority can be activated by manifold social groups, notably, when political leaders are perceived as legitimate (Cialdini & Goldstein, 2004; Tyler, 1997, 2006). For instance, Baldassarri and Grossman (2011) find that Ugandan farmers are more likely to cooperate when exposed to leaders with centralized sanctioning powers (see also De Kwaadsteniet & Van Dijk, 2010). Relatedly, one may argue that the Catholic Church, given its hierarchical structuring, also fosters obedience to authority.

These considerations underline that our arguments are not exclusive characteristics of Protestant missionaries. Other groups may also increase obedience. Notwithstanding, our research design, which compares Protestant missionary groups of differing theological strictness, allows us to assess whether the theological mechanisms exerts a causal role. Specifically, we should expect to see greater effect sizes among Pentecostal missionaries if the theological mechanism is to carry any weight, which forms the basis for our second hypothesis (H2). As such, our arguments highlight that the content of religion, too, affects obedience, not merely its social structure. Finally, we should note that the above arguments are predominantly with regard to religious authorities (the supernatural or preachers). One should therefore expect possible increases in obedience to be predominantly with respect to religious authority figures. Still, given that obedience is a general personal trait and given that this article explores the relation between missionaries and political authority, we also hypothesize that the effect spills over to political authorities, which gives rise to our third hypothesis (H3).

> **Hypothesis 1 (H1):** Protestant missionaries increase individuals' obedience to authority in villages where they evangelize as compared with villages where they do not evangelize.
> **Hypothesis 2 (H2):** Protestant missionaries increase individuals' obedience to religious authorities more so than individuals' obedience to political authorities.
> **Hypothesis 3 (H3):** Pentecostal missionaries increase individuals' obedience to authority more so than mainline Evangelical missionaries.

Persuasion

Persuasion is a process aimed at changing a person's attitude, behavior, or belief. As such, persuasion, write Cobb and Kuklinski (1997), is the "the crux of politics" (p. 88). Countless theoretical studies attest to the fact that persuasion is a core tool for political authorities (Garsten, 2009; Sniderman, Brody, & Tetlock,

1993; Zaller, 1992). Given that citizens cannot know everything, yet have to vote authorities into office, "knowledge of the summum bonum . . . must be wedded to political persuasion" (Mayo, 1962, p. 561). Indeed, Weber (2009) argues that political authority is toothless without persuasion.

Importantly, the manner in which political authorities can persuade citizens may be compromised by religious groups. Religious groups not only use persuasion themselves,[4] but they also provide conceptions of the common good, thereby affecting which topics are discussed in the political arena (Audi, 2000). If a given religion takes little interest in secular issues, political authority may find its persuasive efforts on secular grounds futile. Thus, notes Habermas (2006), political authority can only hope that "religious citizens . . . develop an epistemic stance toward the priority that secular reasons enjoy in the political arena" (p. 14).

Protestant missionaries, however, likely diminish the hopes of secular authorities to persuade religious converts. Two factors are responsible for this: missionaries' skepticism toward secular authorities, and their focus on theological concepts (*sola fide*). Both phenomena give rise to two submechanisms by which Protestant missionaries reduce persuadability: *cognitive dissonance* and *intellectual substitution*. They are discussed in turn.

First, Protestant missionaries reduce converts' persuadability by creating *cognitive dissonance*, which they induce by propagating other-worldly beliefs that may be at odds with statements of political authorities and by endorsing a generally skeptical attitude toward political authorities (Casanova, 2011). Indeed, Ireland (1993) notes that the "prevailing stereotype of Pentecostal crentes is that they are apolitical conservatives who leave the injustices of the world to the Lord's care" (p. 4). There is little doubt, as Gill (2001) argues, that "Latin American Protestantism has not shown the political activism of other fundamentalisms" (p. 129). As such, Protestant missionaries may create cognitive dissonance by pitting converts against secular authorities. Examples abound from several anthropological accounts. In one study about a different set of communities in the Peruvian Andes, the author notes that Protestant missionaries believe that "events in society are to be judged and interpreted through the Bible" not through political leaders (Olson, 2006, p. 890). In our own qualitative interviews (discussed below), local experts stated that the relation between village presidents and Protestant converts was strenuous, often shaped by conflicting viewpoints about the common good. When asked to describe the relationship on a scale from 1 (*very bad*) to 10 (*very good*), experts responded with a low average of 3.0 in Evangelical and 2.1 in Pentecostal villages.

Second, Protestant missionaries reduce converts' persuadability by fostering *intellectual substitution*, which they induce by shifting attention from

secular topics to questions of theological importance (Deiros, 1991). Protestant missionaries are known to propagate that the scripture is adequate for addressing all issues of life, claiming a "self-sufficiency of the bible" (Birch, Rasmussen, & Wogaman, 1989, p. 152). Webber (1978) argues that this focus on scripture is the quintessence of Protestantism, that is, "deep conviction that faith in Jesus Christ . . . produces life-changing effects" (p. 17). Martin Luther, too, demanded such a focus on *sola fide* (belief alone), which broadly sets Protestants apart from other religions, particularly Catholics. Specifically, Luther, referring to Romans 3: 21-28, argued that God's pardon for sinners is received through faith alone. Consequently, Protestant missionaries preach that belief alone (*sola fide*) is the answer to worldly matters (Garrard-Burnett & Stoll, 1993). In contrast to the Catholic Church, which focuses on this-worldly matters, Protestant missionaries replace secular topics with theology.

Both arguments give rise to our main persuasion hypothesis (H4), namely, that Protestant missionaries reduce converts' persuadability with regard to topics of secular importance. Again, one may justly ask whether these effects are an exclusive product of Protestant missionaries. After all, some strands of Protestantism, particularly in England, rejected the notion of *sola fide* (Baker, 1985). As such, it is important to reiterate that the arguments rely on a probabilistic logic. Once again, however, the mechanisms predict effect sizes to be particularly pronounced among Pentecostals, which forms the basis for our fifth hypothesis (H5).

Hypothesis 4 (H4): Protestant missionaries reduce individuals' persuadability to political authorities in villages where they evangelize as compared with villages where they do not evangelize.

Hypothesis 5 (H5): Pentecostal missionaries reduce individuals' persuadability to political authorities more so than mainline Evangelical missionaries.

Empirical Design

Testing hypotheses about the effects of Protestant missionaries on political authority requires a causally identified research design. Most studies on the effects of missionaries, thus far, have used observational evidence. Such data, though historically extensive, may fall victim to unobserved confounding. Notably, Protestant missions have historically clustered in colonized areas, which differ from noncolonized areas in several ways of which some are difficult to observe. This article circumvents the problem of unobserved confounding by exploiting exogenous variation in missionary activity, which took place in the Peruvian Andes in the late 1980s.

The Shining Path and Protestant Missionaries

The rise and fall of the guerrilla movement Shining Path (*Sendero Luminoso*) afforded temporal variation in the exposure of communities in southeastern Peru to Protestant missions. The Shining Path, an offshoot of the Communist Party of Peru, was founded in the late 1960s. The group fought against Peru's supposedly bourgeois democracy. Its goal was to implement a dictatorship of the proletariat (Stern, 1998). To do so, in 1980, the group formed a "revolutionary directorate." The subsequent burning of ballot boxes during the democratic election marked the inception of a violent insurgency.

In the following years, the group made significant territorial advances. The rebels gained support from peasants, particularly in economically disadvantaged regions of Peru's central provinces. Initially, government response to the uprising was limited, which allowed the Shining Path to steadily increase its influence (Switzer, 1993). By 1990, the group had gained control over large areas of Peru's central and southern districts. The capture of its leader, Abimael Guzmán, in 1992 marked the beginning of the groups' gradual decline, exacerbated by increasing military action from the Peruvian government.

In contrast to the rise of Evangelical proselytism across South America in the 1970s and 1980s, the Shining Path marked a hindrance to Protestant missionaries in several regions of Peru. While the interaction between the rebels and missionaries has received little scholarly attention (Ferguson (2005, p. 248), most existing accounts highlight the hostile relationship between the two groups. One account by del Pino (1996) points out that in the Apurimac region—the bordering region of this article's sample—Pentecostals viewed the Sendero as "demons," and took up arms to fight them. In a more systematic overview, Klaiber (1988) documents systematic Protestant victimization by rebel and counterinsurgency forces. According to Strong (1992), this victimization was inevitable because the Evangelicals, too, aspired to be the leaders of the peasant class.

Vivid accounts are also found among Latin American theologians. Escobar (1986) writes,

> The militants of Sendero have . . . been ruthless in their effort to eliminate any opposition in the areas that came under their control. Hundreds of policemen and peasants have been killed in cruel spectacular ways by the terrorists. Evangelical pastors and lay leaders who were perceived as ideological enemies were also killed mercilessly. (p. 10)

Protestant missionaries were, indeed, a prime target of the rebels; several leading figures were killed (Cadorette, 1994). Switzer (1993) recounts a particularly gruesome attack:

> In response to increased foreign presence, Sendero Luminoso stepped up its violence against foreigners. In August 1990, for example, it attacked and killed two Mormon missionaries near Huancayo. A handwritten note left near the bodies demanded that all "Yankee invaders" leave Peru. (p. 61)

While the Cusco region—the case study of this article—was only mildly affected by the insurgency, it, too, witnessed less missionary activity than was common in counties not bordering the rebel's territory.

It was not until the capture of Guzmán that missionaries rediscovered the region as an area for proselytism. Once the area was considered safe, several Protestant churches embarked on a race to bring the gospel to the communities, which, at that point in time, were nominally Catholic. This Catholicism, however, was dormant. The missionary hectic was particularly prevalent in villages near the city of Cusco. Road access was decent, and villagers knew enough Spanish to attend services. The hectic created a situation where church planting followed no discernible strategy; conversion was pursued independent of village characteristics. Just as important, the missionary race also left some communities untreated, which serve as the control category in this article.

Interviews conducted by the authors with local and international missionaries in the region confirm the random and hectic nature of the church planting. A Baptist missionary from the United States conveyed to the authors that "missionaries trickled in and picked communities in an arbitrary manner." In a similar vein, a minister of the Seventh Day Adventist church stated that Protestant missionaries "chose the ones [villages] no one had gone to as quickly as they could." When asked why a Pentecostal church had picked one particular village, a Catholic nun replied, "The missionaries are only driven by the holy spirit."

In addition to conducting statistical tests that underline the exogeneity of the process, we conducted structured interviews with church officials and village presidents in all communities studied in this article. These interviews, which are presented in detail below, yield two important pieces of information regarding the church planting process.

First, to understand whether missionaries followed a specific strategy when selecting villages, we asked the following open-ended question to the church officials: "Why did you set up the church in this village?" Buttressing our argument that villages were treated independent of village characteristics, no church official listed a concrete example why a given village had been chosen. Two officials could not think of a reason, four said that the village simply had had no mission, four stated that other missions were too far away for individuals to go to, and five mentioned abstract reason of the form, "it is necessary to be in all parts where God is."[5]

Second, to understand whether the treatment was exclusive, we asked village presidents whether there had ever been another mission: "Have there ever been other missions present in this village?" Fifteen out of 16 presidents said no. One president claimed that a mission had been present, but had discontinued its operation after a few months. In addition, all officials negated the question "Have there ever been other missionaries present trying to convert people." The assignment of the missions can therefore be viewed as exclusive, which allows us to attribute observed differences across villages to the Protestant missions.

Sample

The overall population of interest consist of the Agrarian communities in the state of Cusco in southeastern Peru. The sample in this study includes 16 villages, which were carefully selected from over 50 communities so as to guarantee isomorphic pretreatment conditions. Specifically, selection of villages followed two main criteria. First, we chose communities that are similar regarding observed pretreatment covariates, including geographic, linguistic, and agricultural characteristics. Second, we chose communities large enough to allow random sampling of at least 64 respondents, a size necessary to administer several randomized instruments. The sample sites are reported in Figure 6 in the supplementary material. We should also point out that 16 villages is a rather low number of clusters, which puts statistical power at risk. Yet, the case at hand did not allow for selecting more than 16 villages, given our strict sampling procedure and the historic setup. For this reason, we opted to buttressing any observed differences across the villages using qualitative interviews.

Treatment

The final set of 16 villages includes five conceptually different treatments reported in Table 1. First, the *Control* treatment depicts villages that never received a mission. Akin to all communities in the sample, the villages in this category have old Catholic Church buildings that are no longer in use. The reasons for the Catholic Church's gradual decline in the communities under study are manifold. They range from poor central planning, limited resources, to recruitment shortages.[6] *Control* villagers, all of which still identify as Catholic,[7] sometimes visit Catholic parishes in cities where services are still offered. The *Control* communities thus serve as our counterfactual. They allow us to understand what evangelized villages would look like today had they not been exposed to Protestant missionaries. The reasons why these

Table 1. Treatments.

Treatment	Compliance (%)	n	Theology
Control	95.8	192	Limited exposure to Catholic gospel
Adventists	51.0	192	Limited spirituality, no speaking in tongues, shorter service
Peruana	57.3	192	High spirituality, some speaking in tongues, long service
Maranatha	49.6	256	High spirituality, speaking in tongues, very long service
Mixed	57.3	192	Combination of the above

villages never received a mission are unclear. In qualitative interviews, the author could not obtain a satisfactory answer from village presidents and church officials. Suffice it to say that the Control villages are statistically indistinguishable from the proselytized villages (see "Balance" section).

The *Adventist* treatment depicts villages where the mainline Evangelical Seventh Day Adventist church set up missions. These villages are exposed to rather mild forms of Evangelical theology. Church services are socially considered mandatory, but the sermons are shaped by a somewhat more critical engagement with the scripture. Speaking in tongues and other charismatic practices are largely absent.[8] Adventist villages are also shaped by more outgoing and politically active church leaders who take an active interest in their communities.

The *Peruana* treatment depicts villages that were exposed to a Peruvian Pentecostal church, called Iglesia Evangélica Peruana. Spirituality in the Peruana church is noticeably more pronounced. Church services include charismatic elements like speaking in tongues and divine healing. The Peruana church is a strongly biblical church. As Olson (2006, p. 890) writes, "IEP follows the 'biblical Christianity' commonly associated with Evangelical Christian churches in the United States and other Evangelical churches of Latin America" (see also Barreda, 1993). In comparison with Adventists, Peruana services are also significantly longer, and regularly last for more than 3 hours Church leaders are less politically active and demand strict obedience to the scripture and an active engagement with the church. As such, the Peruana prizes "personal relationships with God through the 'Word' as put forth in the Bible" and "events in society are to be judged and interpreted through the Bible" (Olson, 2006, p. 890).

The *Maranatha* treatment depicts villages that received a mission post from a Pentecostal church called Iglesia Cristiana Maranatha. The church

labels itself as charismatic. Church services are exceedingly long and include manifold charismatic elements such as widespread use of speaking in tongues, shared crying of the congregation, divine healing, and divine revelations. Church leaders and ministers in the Peruana church are even less concerned with secular issues as compared with the Peruana church. In contrast to all other churches, interview requests by the authors were met with skepticism, and regularly denied.

Finally, the *Mixed* treatment category includes villages that received several Evangelical missions, all of which are either Adventists, Maranatha, or Peruana missions.[9] The treatment strength (i.e., the intensity of the Evangelical preaching) therefore ranges somewhere in the middle.

In Table 1, we summarize the five treatment statuses. As can be seen in column 2, compliance rates—villagers reporting to belong to the church—is around 50%. The third column states how many individuals were sampled in each treatment category. Table 7 in the supplementary material gives a more comprehensive overview of the sample's treatment status and religious affiliation.

Sampling Strategy

In addition to qualitative surveys, we administered a large-scale structured survey in all 16 villages, enrolling 64 subjects per village. To do so, the author visited each of the 16 villages prior to surveying and obtained consent from village presidents. In 12 villages, the author presented the work in front of the village assembly. In the remaining four villages, the presidents informed villagers that surveying would take place. The author was assisted by 21 local Master's students who were carefully selected from a pool of 50, and were covered under Columbia University's Institutional Review Board (IRB).

The entire sample includes 1,024 respondents. In all, 1.3% of all outcome measures are missing. In additional analyses, available upon request, we show that attrition is independent of treatment status. To ensure as representative a sample of the villages as possible, the sampling employed two levels of randomization. First, all 64 respondents per village were sampled using a random-walk procedure.[10] Second, enumerators were randomly assigned to villages and to each block within a village to avoid enumerator effects.

Surveys were conducted in the late afternoons and at night to maximize participation. Although very time-consuming, sampling during the day was not possible as most villagers take their cattle to the fields. Nonparticipation was at 7.8%. Failure to participate was largely due to respondents' time constraints. Given the small size of the villages (an average of 68 families), the vast majority of eligible respondents was sampled. Respondents were paid 5 Peruvian Sols (US$1.75)—about a day's earnings.
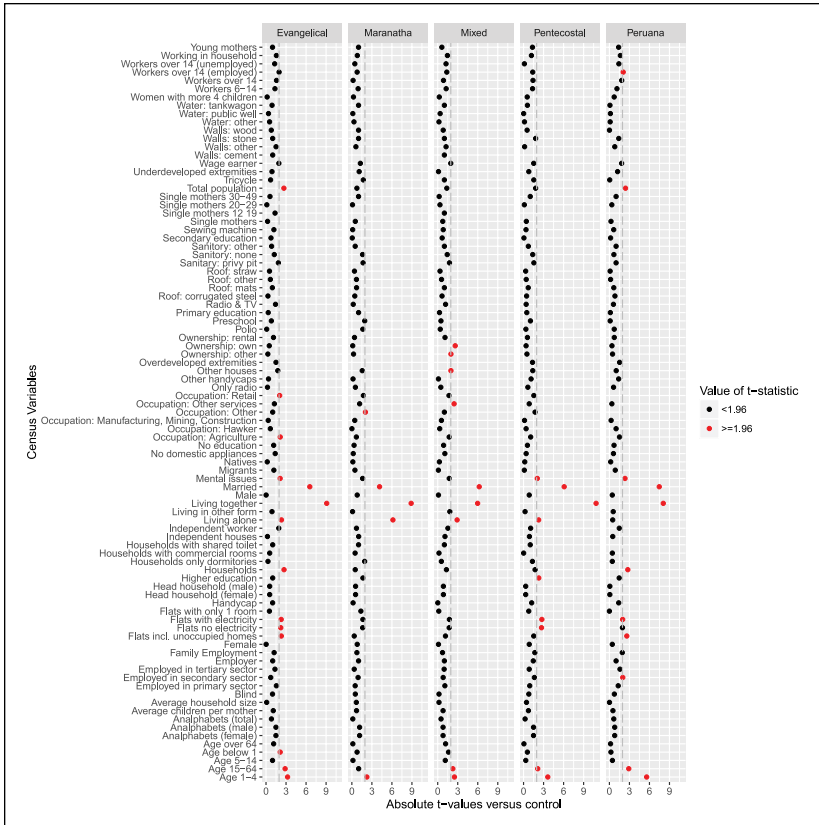
Figure 2. Balance across treatment statuses.

Balance

Our key identifying assumption is that assignment to treatment was independent of potential outcomes. We took several steps to buttress this assumption empirically.

First, we gained access to the Peruvian census from 1993. The data, thus, fall broadly within the period when the missionaries began their conversion in the communities. The census includes data on 91 variables including indicators on education, demographics, and economic development. We report these data in Table 10 in the supplementary material. To assess balance across the different treatment statuses as compared with the control villages, in Figure 2, we plot absolute *t* values assessing differences-in-means across the

five treatment statues and the nontreated control villages. As can be seen, the vast majority of variables are not significantly different. Indeed, only three variables are consistently different in the treatment villages, namely, individuals between 1 and 4 years of age, married individuals, and individuals living together. The low incidence of systematic pretreatment differences on a host of administrative variables, thus, buttresses our key identifying assumption.

Second, we gathered additional information on pretreatment variables in semistructured qualitative interviews with history experts in each village. These individuals were recommended to us by village presidents. We asked these individuals to think back to the year 1992 and asked them whether their village, at the time, had had a school, hospital, post station, or football field. We report these variables in Table 10 in the supplementary material. To ensure that the experts recalled the correct villages, we reminded them that the year 1992 was the year prior to the census, which all experts recalled. These variables, too, confirm that the villages were broadly similar pretreatment. None of the villages had schools, hospitals, or post stations, except for one village in the Peruana treatment category. Football fields were more widespread at the time, ranging from 33% in the untreated control villages to 100% in the Peruana villages. Importantly, all villages confirmed that the Shining Path had been active in the village in the 1980s. These data, then, provide additional qualitative evidence regarding pretreatment balance.

Third, we collected five geographic variables from all villages. We recorded a given village's road and geodesic distance to Cusco (in km) as well as a given village's elevation and its longitude and latitude. These data are reported in Table 9 in the supplementary material and offer an objective way to assess balance given that the data cannot be affected by the treatment. The table underlines that there are no apparent imbalances between treatment and control villages. On average, distance to Cusco is 22 km in Control villages as compared with 28 km in Adventist and Mixed villages, 34 km in Peruana villages, and 17 km in Maranatha villages. Elevation is roughly 12,000 feet in all villages, except for the Peruana villages (13,000 feet).

Last, we can scrutinize balance across treatment and control villages in our random population sample. The sample's individual-level variables are reported in Table 8 in the supplementary information. Many of these variables could plausibly be affected by the treatment (e.g., education, income, relationship status, professions, and propensity to travel). The variables that cannot be affected by the treatment, however, are perfectly balanced across the treatment statuses. In particular, the average age is 41 years in Control villages, 39 years in Adventist villages, 40 in Mixed villages, 37 in Peruana villages, and 38 in Maranatha villages. Gender, too, is perfectly balanced.

Finally, 5% of residents in Control villages have Internet access compared with 8% in Adventist villages, 3% in Mixed villages, 5% in Peruana villages, and 2% in Maranatha villages.

Taken together, we interpret the balance across a host of pretreatment variables—census data, expert interviews, geographic data, and the population survey—in favor of our key assumption, namely, that missionaries visited communities independent of potential outcomes. We should also highlight that the few variables that do exhibit imbalance (notably, marital status, and individuals between 1 and 4 years of age) are unlikely to explain diverging levels of persuadability and obedience.

Measurement

We use experimental and attitudinal evidence to test the degree to which Protestant missionaries affect obedience and persuadability, which we outline in turn.

We experimentally tested obedience using a novel lab-in-the-field experiment. Since Milgram's (1974) pioneering research, experiments of obedience have been scarce. We therefore developed a new obedience experiment, aimed at seamless implementation during the survey. In particular, before commencing the surveys, research assistants obtained statements from local church and state authorities, in which they insisted that surveys were to be conducted in public places for security reasons. At a random point during the survey,[11] enumerators were instructed to state the following sentence: "As a matter of fact, [church/state] authorities have demanded that we conduct this survey in a public place. Should we move? We are happy to do it at either place." All endorsements, including those listed below, were fully factorially randomized within a given village level. As enumerators were instructed to commence the surveys in private homes, the statement placed respondents in a realistic dilemma. Respondents had to decide whether to follow the command from the respective authority and thus suffer the nuisance of relocating. After answering the question, the enumerators recorded whether subjects, indeed, followed the authority's request. As the left column in Table 2 demonstrates, the experiment worked well, inducing pronounced variation among participants. In all, 26% of subjects followed the command from the respective authority.

In addition to the experimental measure, we assessed obedience using one attitudinal measure. Specifically, we asked subjects whether they would accept wrongdoings of [church/state] authorities: "Would it be OK for you if you knew a [church/village] authority was doing something dubious?" As the right column in Table 2 shows, a surprising 23% of individuals stated they

Table 2. Obedience—Average Experimental and Attitudinal Results.

	Behavioral	Attitudinal
	Obeyed order from authority?	Accepts wrongdoing by authority?
Yes	26.0%	22.5%
No	74.0%	77.5%

Table 3. Persuasion—Aggregate Experimental and Attitudinal Results.

	Behavioral	Attitudinal I	Attitudinal II
	Persuaded by authority?	Deems state projects important?	Deems village projects important?
Yes	9.2%	37.2%	39.2%
No	90.8%	62.8%	60.8%

would tolerate wrongdoings of the respective authority figure. The number buttresses the findings from the behavioral experiment.

We experimentally tested persuadability by implementing a persuasion experiment. To do so, we built a typical persuasion experiment, in which we measured an attitude, intervened with new conflicting evidence, and then remeasured the attitude at a later point (Redlawsk, 2002; Taber, Cann, & Kucsova, 2009). In particular, we asked respondents at an early point during the survey whether they would like social spending in Peru to increase: "Do you want social spending to be higher in Peru (e.g., health, education)?" Depending on the answer, respondents were then given a newspaper article in which church/state authorities expressed the opposite opinion, namely, that social spending was too high/low. The newspaper articles were taken from international media, as relevant accounts were not available for Peru. For example, a respondent that deemed spending too low was read the following script: "I have here a newspaper article. It shows that a [state/church] authority believes that social spending is too high and that this is not good for the economy." The goal of the statement was to expose respondents to conflicting evidence delivered by an authority figure. At a later point in the survey, respondents were then asked the same initial question, and enumerators noted whether subjects had changed their mind as a result of the persuasive effort by the authority figure. As Table 3 shows, 9% of respondents were persuaded to an alternate opinion. It is worth pointing out that the experiment was unobtrusive in nature, and the persuasive effort rather mild.

In addition, we assessed persuadability using two attitudinal measures. First, we asked subjects an open-ended question regarding possible infrastructure projects they would like state authorities to implement: "Which projects would you like state authorities to implement?" Subjects were then given four projects (hospital, school, roads, and water), and enumerators noted which projects the respondents deemed important. The question, thus, aimed at measuring the degree to which subjects take an interest in secular issues. The average affirmation for the projects was (Table 3) was 37%. Second, enumerators inquired about projects that subjects deemed worthy of implementation at the village level: "What is the most important thing this town needs to improve?" Here, too, subjects were given a list of five projects (health, education, trust, water, and the church), and enumerators noted how many were affirmed. On average, 39% of projects were affirmed.

Results

In estimating the degree to which Protestant missionaries affect obedience and persuadability, we analyze the intention-to-treat effect at the village level. Our primary linear model—estimated using ordinary least squares (OLS)—is as follows:

$$Y_{il} = \beta_0 + \beta_1 \text{Adventist}_{il} + \beta_2 \text{Mixed}_{il} + \beta_3 \text{Peruana}_{il} + \beta_4 \text{Maranatha}_{il} + \mathbf{X}_{il}\beta + \in_{il}, \quad (1)$$

where Y_{il} represents the outcome of individual i in village l, Adventist, Mixed, Peruana, and Maranatha represent dummies for the respective treatment statuses, and $\mathbf{X}_{il}$ represents a vector of individual-level and community-level control variables.

Given the large number of possibly prognostic control variables, there is significant discretion over which variables to include in our models. We therefore focus on variables entirely unaffected by the treatment. At the individual level (Table 8 in the supplementary material), this includes *age* and *gender*. At the village level (Tables 9-10 in the supplementary material), this includes all geographic variables (i.e., road and geodesic distance to Cusco, elevation, longitude, and latitude). In addition, it includes historic pretreatment variables drawn from interviews with history experts (i.e., whether a village had a school, hospital, post station, and football field in 1992, and whether the Sendero Luminoso was active in the village). Finally, it includes all census variables listed in Table 10 in the supplementary material. Overall, there are, thus, well over 100 possible pretreatment covariates at our disposal. To minimize "researchers degrees of freedom" when determining the prognostic value of covariates and to avoid multicollinearity, we therefore report models

without covariate adjustment as our preferred benchmark specification. In addition, we report models that include individual-level control variables unaffected by the treatment, namely, age and gender. In the supplementary information, we perform sensitivity analyses, estimating models where covariates are gradually and randomly added to our models. Figure 11 in the supplementary material presents the average estimated effects and standard errors randomly drawing one to 10 number of covariates, 1,000 times each, which buttresses our reported effect sizes and uncertainty levels.

The estimation of standard errors merits discussion. As is widely known, OLS tends to underestimate the true standard errors when errors are correlated within a cluster. The common approach to tackle this problem is to apply sandwich estimators, which permit for the errors to be heteroskedastic and also to be correlated within clusters. Problematically, these estimators are only correct when the number of clusters approach infinity. The present study, however, only has 16 clusters. To bridge this problem, a recent influential study by Cameron, Gelbach, and Miller (2008) proposes the use of wild cluster bootstrap t procedures to estimate the standard errors. Using Monte Carlo simulations, the authors show that this procedure performs significantly better even when the number of clusters is less than 10. In this article, I therefore apply their method to estimate standard errors. We corroborate effect sizes and uncertainty levels using cluster randomization inference in Figures 9 and 10 in the supplementary material.

Obedience

Our first hypothesis states that Protestant missionaries increase obedience to *both* state and church authorities. In Figure 3a, we report the results of our obedience experiment. Villages exposed to Protestant missionaries are shaped by systematically higher levels of obedience. The treatment effect is not discernible among the mainline Evangelical Adventist church. However, the Pentecostal churches (Peruana and Maranatha) who propagate a particularly strict interpretation of and alignment with religious scripture lead to distinctly higher levels of obedience. Indeed, in the combined sample (left plot of Figure 3), respondents in Pentecostal villages were 19% more likely to leave the situation when informed that authorities demanded to do so. The results are also robust to the inclusion of individual-level covariates (circle), and hold across state and church authorities. The effects, though estimated with significant variability due to the low number of clusters, are unusually large in size. They highlight the profound effects missionaries exhibit in local communities. In Table 8 in the supplementary material, we report the outcome measures across the treatment statuses to facilitate the interpretation of
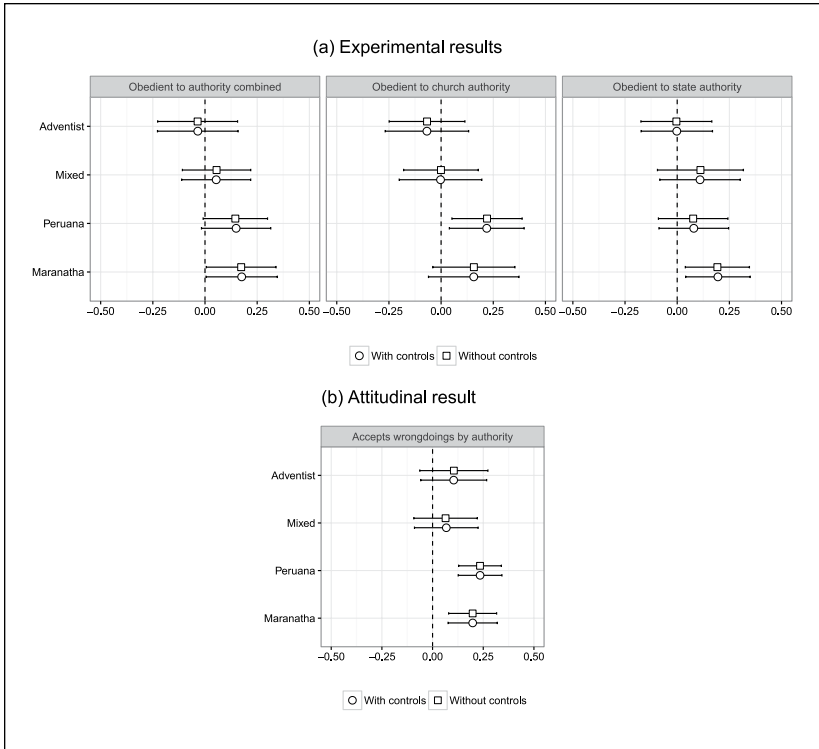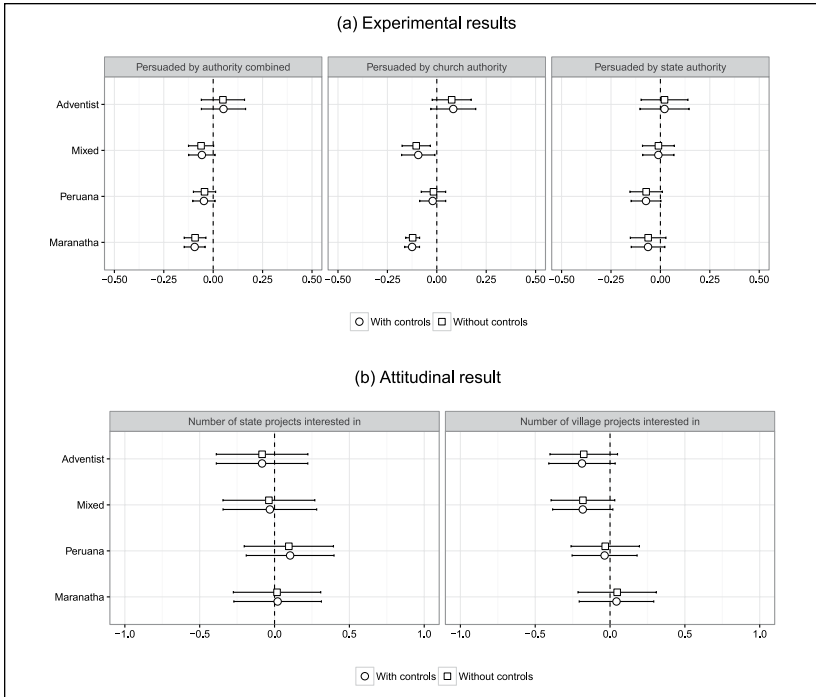
Figure 3. Obedience results.
The figures plot point estimates (dot/square) and 95% confidence intervals (lines) of regressions of the four treatments on the indicated outcomes of the obedience measures. Dots represent regressions with covariate adjustment, squares represent regressions without covariate adjustment (age and gender). Standard errors estimated using wild cluster bootstrap *t* procedure.

substantive effect sizes. As can be seen, in control villages, 18% obeyed the authority figure as compared with 33% and 36% in the Peruana and Maranatha villages, respectively.

The same result emerges when scrutinizing the attitudinal measure. In Figure 3b, we report the results from the question regarding the acceptance of wrongdoings by authority figures. As can be seen, acceptance of wrongdoings is positively correlated with the presence of a Protestant mission. Again, the treatment effect is most pronounced for Pentecostal missions. Here, the likelihood to accept wrongdoings is roughly 24% higher as compares with Control villages, which have not received a mission post. Mean outcome

Figure 4. Persuasion results.
The figures plot point estimates (dot/square) and 95% confidence intervals (lines) of regressions of the four treatments on the indicated outcomes of the obedience measures. Dots represent regressions with covariate adjustment, squares represent regressions without covariate adjustment (age and gender). Standard errors estimated using wild cluster bootstrap *t* procedure.

values, reported in Table 8 in the supplementary material, highlight that 10% in control villages accept wrongdoings as compared with 33% and 30% in Peruana and Maranatha villages, respectively. The large treatment effects thus corroborate the effect sizes found in the experiment.

Persuadability

Our second hypothesis states that Protestant missionaries reduce persuadability with regard to topics of secular interest. In Figure 4b, we report the results of the persuasion experiment. Villages exposed to Protestant missionaries are, overall, shaped by lower levels of persuadability. Interestingly, the mainline Adventist church yields a positive (insignificant) coefficient. The

three remaining treatment statues, particularly the strict Pentecostal villages, display distinctly lower levels of persuadability. The effect is particularly pronounced for the Maranatha church—the strictest mission in the sample. Again, the pattern is similar across respondents primed with state or church authorities. Somewhat surprisingly, persuadability regarding secular topics seems less pronounced for respondents primed with church authorities. We should note, however, that the differences between estimates are not themselves statistically significant. Notwithstanding, the results show a pattern that religion leaves respondents (only roughly 50% of which have been proselytized) less persuadable with regard to topics of secular importance such as social spending. Although estimated with significant variability, these effects are, again, sizable in scope: Pentecostal mission reduces persuadability by as much as 10%. The estimates are also highly similar across models where individual-level covariates are included. When looking at means across the treatment statues (Table 8 in the supplementary material), 13% of individuals in control villages are persuaded during the experiment compared with 8% and 3% in the Peruana and Maranatha villages, respectively.

The attitudinal measures (Figure 4b) yield a somewhat ambiguous pattern. Most coefficients are close to zero. One notable exception is Adventist villages, which display markedly less interest in village projects. Qualitatively speaking, this may be due to the fact that Adventist villages have high conversion rates (see Table 7 in the supplementary material). Moreover, the author witnessed these villages as particularly homogeneous. Indeed, because the Adventist church is less other-worldly than its Pentecostal counterparts, it seems to have taken a more thorough stance in improving the livelihoods in a given village. Overall, however, the results taken together showcase that the attitudinal measures did not produce significant variation, and are estimated with high variability. This substantive conclusion also becomes apparent when looking at means of the attitudinal outcome measures across the treatment statuses (Table 8 in the supplementary material). On average, individuals mention roughly one project across all treatment statuses.

Subset of Converts

Thus far, the analyses have scrutinized the effects of missionaries for the entire sample. A similarly interesting question, however, is what effects missionaries exhibit on converts. For this reason, in Figures 7 and 8 in the supplementary material, we conduct the same analyses for the subset of converts. Not surprisingly, the results consistently gain in strength when focusing on proselytized individuals. The results thus confirm our theoretical intuition that the missionaries exhibit the most pronounced psychological effects for those that attend

the church. In addition, the coefficients from these models are estimated with less variability given that the sample is more homogeneous.

Qualitative Analysis

In addition to the experimental and attitudinal evidence, we conducted qualitative, semistructured interviews with all community presidents and one missionary in case a community was treated. The interviews were conducted 6 months after the initial surveying activities to cross-check the causal estimates and to provide a means to integrate the cross-case evidence with the qualitative information.[12]

In line with our quantitative findings, the qualitative interviews with village presidents confirmed that control communities have witnessed little behavioral change. When asked what had changed in recent years—perhaps due to the religion of the village—all three presidents of Control villages could not think of any significant differences. One president simply stated, "nothing really." Inquiring about obedience, all three Control presidents confirmed that residents in their villages were critical of authority figures. A president from a different Control village stated, "we are witnessing that the community is not taking the government seriously."

In the Evangelized villages, this picture was reversed. While Adventist presidents discerned only mild behavioral changes, presidents in the Mixed and Pentecostal villages overwhelmingly noted significant behavior changes. Two trends stood out. First, presidents stated that the residents worked harder since being proselytized. Second, presidents noted that residents consumed less alcohol. One Peruana president stated, "The behavior of people has changed. In the past, they used to drink; now they only drink at times." The rise of obedience was also acknowledged, and several presidents linked it directly to the charisma of the pastors. As one president noted, "we simply obey the church, and the pastor gives us his opinion."

The qualitative interviews with preachers in treated villages buttressed these patterns. However, one should use this evidence with caution as pastors are not objective analysts of behavior changes due to the advent of a mission. Notwithstanding, with regard to the psychological mechanisms, every single interviewed pastor singled out obedience as a quintessential factor of their mission. For example, one pastor argued that obedience is a key Protestant trait: "yes, the bible teaches us to be correct and understand, and to lead our lives in faith." Another stated, "obedience guides us on our way to truth."

To assess potential biases in the assessment of village presidents about the causal process between missionaries and state authorities, we added four questions to the history expert interviews. These data, reported in Table 9 in

the supplementary material, show that the presidents are likely similarly objective and knowledgeable across treatment statuses. Specifically, average tenure of the presidents ranges from 18 months (Adventist villages) to 22 months (Maranatha and Catholic). The number of village assemblies where presidents discuss communal matters, too, is equal across all treatment conditions, namely, 12 per year. The relation between presidents and the village, assessed on a scale from 1 (*very poor*) to 10 (*excellent*), differs across treatment statuses, but not in a way that would be predicted by the assignment to treatment. In particular, the history experts rated the relation as 8.3 in Catholic villages, 4.3 in Adventist villages, 9.3 in Mixed villages, 10.0 in Peruana villages, and 6.2 in Maranatha villages. Finally, and interestingly, the relation between the presidents and converts, assessed on the same 10-point scale, was stated to be rather poor. In the mixed and Peruana villages, it was at 1.0, while the experts gauged it at 4.4 and 3.2 in Mixed and Maranatha villages, respectively. Taken together, we interpret the qualitative information about village presidents by history experts such that causal estimates from presidents about missionaries do not suffer from explicit biases.

Integration

To integrate the evidence from the qualitative interviews with the quantitative evidence, we use Bayesian Integration of Qualitative and Quantitative Data (BIQQ)—a new method proposed by Humphreys and Jacobs (2015). BIQQ allows researchers to draw integrated inferences from correlational (cross-case) and process-level (within-case) observations. The framework relies on the potential outcomes framework popularized by Rubin (1974).

Let the treatment, *X*, be the exposure of a village to a Protestant mission. Let the outcome, *Y*, be obedience or persuadability. Because treatment is assigned at the village level, in this section, we aggregate the experimental outcomes at the village level. To reduce complexity of causal types (Humphreys & Jacobs, 2015, p. 662), we dichotomize the aggregated outcomes. In particular, we code a village as "obedient" (or "non-persuadable")[13] when the village-level mean is above (below) the population-level mean. This approach is rather punishing, biasing our integrated inferences downward. The resulting aggregated outcomes for the villages are given in Table 4.

In accordance with Rubin (1974), there are four potential types of villages, reported in Table 5. Adverse villages are those where inhabitants become obedient (non-persuadable) when *not* treated and disobedient (persuadable) when treated (Type *a*). Beneficial villages are those where inhabitants become obedient (non-persuadable) when treated and disobedient (persuadable) when not treated (Type *b*). Chronic villages are those where inhabitants

Table 4. Quantitative Evidence Aggregated at Village Level.

Obedience	$Y = 0$ (Disobedient)	$Y = 1$ (Obedient)
$X = 0$ (Not evangelized)	2	1
$X = 1$ (Evangelized)	9	4
Persuasion	$Y = 0$ (Persuadable)	$Y = 1$ (Nonpersuadable)
$X = 0$ (Not evangelized)	3	0
$X = 1$ (Evangelized)	12	1

Table 5. Types of Villages.

	$Y = 0$	$Y = 1$
$X = 0$	beneficial; chronic	adverse; destined
$X = 1$	adverse; chronic	beneficial; destined

become disobedient (persuadable) whether treated or not treated (Type *c*). Finally, destined villages are those where inhabitants become obedient (non-persuadable) whether treated or not treated (Type *d*).

When testing our hypotheses that Protestant missionaries increase obedience and reduce persuadability, we want to estimate the share of beneficial types in the population, that is, the share of villages that turn obedient (non-persuadable) if and only if there is a Protestant mission. The average causal effect is, hence, $b - a$. As Table 5 demonstrates, the *fundamental problem of causal inference* is that one can only observe the outcome and the treatment status, but not the type. A researcher observing change is thus uncertain whether citizens in a village turned obedient (non-persuadable) because of the mission (Type *b*), or whether such change would have occurred regardless (Type *d*).

In traditional quantitative cross-case estimation, researchers do away with this problem by ruling out destined types (monotonicity). An alternate approach is to leverage fine-grained local-level evidence in determining the type of a case. Such process-tracing allows researchers to draw "causal inferences from diagnostic pieces of evidence" with regard to one single case (Collier, 2011, p. 824). Process-tracing, in a word, attempts to determine the type of a given case by observing the underlying causal process. Traditionally, process-tracing and cross-case estimation have been conducted separately. The BIQQ framework, however, allows one to combine both approaches. Specifically, BIQQ uses the variable K to depict the outcome of the process

Table 6. Qualitative Clues.

Obedience	$Y = 0$	$Y = 1$	Persuasion	$Y = 0$	$Y = 1$
$X = 0$					
$K = 0$	2	0	$K = 0$	2	0
$K = 1$	1	0	$K = 1$	1	0
$X = 1$					
$K = 0$	1	0	$K = 0$	2	1
$K = 1$	4	8	$K = 1$	3	7

tracing. $K = 1$ indicates that a clue is searched for and found. Here, the researcher gained evidence that a village is beneficial, for example, it turned obedient (non-persuadable) *because* of a Protestant mission. $K = 0$ indicates that a clue is searched for and not found. Here, the researcher gained evidence that the village is a destined type.

To implement such integration, we concluded the above semistructured qualitative interviews by quantifying the causal "clues" contained therein using two structured questions: "Do you think people in this village have become more obedient to state authorities [less interested in secular topics] because of religion?" Note that we did not inquire about church authorities to avoid placing village presidents in an uncomfortable situation given that many are part of the mission. If village presidents said "yes," they characterized the village as beneficial. If they said "no," they saw no causal relation between the two variables.

The results of the two questions are given in Table 6. Most presidents in treated villages deemed their village as beneficial (12 individuals), while only one president deemed it destined (i.e., did not confirm a causal link between Protestant missionaries and obedience). The same picture emerges regrading persuasion. Ten presidents confirmed a causal link between Protestant missionaries and less concern for secular topics. Two presidents did not confirm such a link. The three presidents in non-proselytized villages gave a isomorphic picture across the two outcomes: Two saw no causal link between religion and obedience/persuadability, while one in each village did.

With both pieces of evidence at hand—cross-case evidence yielded from the two experiments, and within-case evidence from village presidents—BIQQ adopts a Bayesian framework to provide one unified causal estimate. To do so, three assumptions must be spelled out.

First, one must characterize priors on the distribution of causal types in the population. They are described by $\lambda = (\lambda_a, \lambda_b, \lambda_c, \lambda_d)$.[14] Here, we use flat priors for λ_a, λ_c, and λ_d. We let the prior for λ_b range from uninformative (i.e., 1) to

20. Thus, $\lambda_{Obedience} = (1, x, 1, 1)$ and $\lambda_{Persuadability} = (1, x, 1, 1)$, where $x \in [1, 20]$. As such, the analysis allows one to interpret the evidence for different priors over b types. Second, one must characterize priors on the treatment assignment probabilities for each village type, described by $\pi = (\pi_a, \pi_b, \pi_c, \pi_d)$. Here, too, we use flat priors. Third, one must characterize the probability of observing a clue for a given village type, j, under the two different treatment statuses ($X = 1; X = 0$), which we label $\phi = \phi_{jx}$. These priors express the likelihood of gathering evidence. We believe that the likelihood to pick up clues does not differ meaningfully across treatment statuses, given that all presidents agreed to give interviews. In line with Humphreys and Jacobs (2015, p. 664), we hence incorporate flat over ϕ.

Overall, these three parameters yield $\theta = (\lambda, \pi, \phi)$, which characterizes the nuts and bolts of the BIQQ framework.[15] In a final step, we then specify a likelihood function. Here, we adopt the following function:[16]

$$Pr(D \mid \theta) = \text{Multinom}(n_{XY*} \mid n - k, w_{XY*}) \times \text{Multinom}(n_{XYK} \mid k, w_{XYK}). \quad (2)$$

Figure 5 shows the results of the integrated inference of the two sources of evidence. In particular, the graph shows the estimated treatment effect under different priors for λ_b. Higher values for λ represent certainty that all villages are b types, and vice versa. The integration buttresses the quantitative point estimates. Indeed, the treatment effect goes as high as 50% for reasonably high levels of λ_b. We should note, however, that almost all presidents in Evangelized villages confirmed that citizens had become more obedient.

Discussion and Conclusion

This article has brought theoretical considerations and empirical evidence to a long-standing question: whether religious groups undermine political authority. Exploring the case of Protestant missionaries, we argued that they affect political authority via two psychological mechanisms. First, the advent of the Protestant belief system, which prizes the supernatural and exposes converts to charismatic preachers, increases *obedience* to authority. Second, Protestantisms' focus on the afterlife coupled with its "political apathy" reduces converts' propensity to be *persuaded* by political authorities. The psychological effect of Protestant missionaries on political authority is thus ambiguous.

Before recommending avenues for future research, two words of caution are in order. First, this article has primarily relied on experimental measures of obedience and persuadability. While experiments offer a good way to measure behavior in a realistic manner, missionaries could have have affected
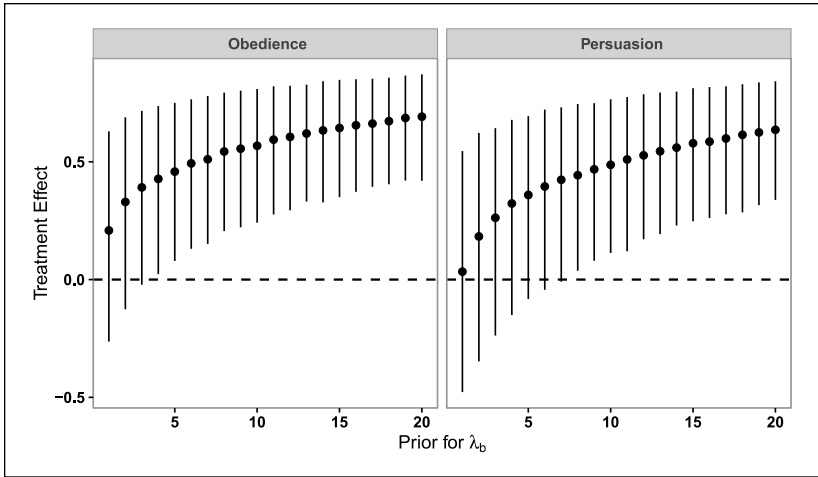
Figure 5. Integrated inference—Sensitivity of λ_b.
The two figures plot the integrated treatment effect under different priors for λ_b (*x* axis). Dots represent the estimates, the lines represent 95% highest density intervals. For the quantitative experimental measures, we collapsed state and church endorsers.

how respondents respond to researchers. To alleviate such concerns, our experimental measures were unobtrusive in nature. Thus, while averages may have been different across villages, treatment effects, as a result of small experimental primes, are unlikely to have differed across treatment statuses. Moreover, enumerators did not notice different attitudes toward researchers across treatment statuses, and research permits were granted in all selected villages.

Second, we have argued that the assignment to treatment in our sample was as-if random due to the unexpected withdrawal of the Maoist rebels. Every attempt was made to solidify this assumption. Qualitative interviews with missionaries, village presidents, and history experts revealed that no one had a clear answer as to why a given community was chosen by a missionary group. Detailed balance checks using pretreatment census data support this claim. Nonetheless, selection may have occurred in unobserved ways. It may, for instance, be the case that missionaries chose particularly obedient (non-persuadable) communities. Indeed, at first glance, obedient individuals are a likely target for Protestant missionaries. Yet, we also showed the persuad-ability was significantly lower, which should work to decrease the effect of Protestant proselytism. More importantly, the missions in our sample have the declared aim to empower the people theologically. It is their intention to

spark people to engage with the scripture, and to share and teach beliefs in a vertical manner. Given these calculi, it therefore seems unlikely that Protestant missionaries would pick communities that are particularly obedient. Obedience is conducive to hierarchical structures, to uncritical engagement with authority—both factors would make it less likely for missions to treat particularly obedient communities.

Having discussed these caveats, we believe that our findings have three broad implications for the study of religion and politics. First, our evidence shows that the relation between religion and politics may be determined as much by individual-level psychological mechanisms as by group-level structural factors. Religious institutions, and missionaries in particular, affect individuals' cognition and behavior. They shape how individuals relate to authority figures, and what topics they take interest in. We hope that by underlining the importance of individual-level psychological processes, this study may motivate future researchers to take the cognitive effects of religion more seriously, and to explore it in other contexts.

Second and related, this article has demonstrated that religion's psychological effects are as much the product of ideas as they are of hard-wired institutions. While missionaries do add new local-level institutions that shape political authority—notably, church buildings—we found that Pentecostal missions exhibit consistently more pronounced effects than their Evangelical counterparts. This is surprising given that Pentecostal and mainline Evangelical missions do not differ on a host of dimensions save their theological strictness. The evidence, thus, shows that religious teachings and ideas shape society more than some institutionally minded scholars have one believe. The very notion that "ideas matter" is thus a fruitful avenue for future research. Above all, future accounts may find it useful to analyze sermons—a task that was beyond the scope of this study.

Third, the present research project exclusively focused on Protestant missionaries, assessing psychological mechanisms. Further research that more fully maps the relation between religion and authority, perhaps at a macrolevel, is therefore the logical next step. For example, the present project did not assess the manner in which religious and state authorities interact. Interestingly, two village presidents in the present study lamented that church leaders and ministers had gained increasing influence. Other village presidents became part of the mission and used the religious networks to their advantage. This anecdotal evidence demonstrates the intricacies at play in the relationship between religious and state authorities—which may help explain why a century-old question continues to spark the interest of social scientists and theologians alike.

Author's Note

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Funding

Notes

1. For example, in his *Letter Concerning Toleration*, John Locke goes to great lengths to point out that religious groups are voluntary organizations, which have no right to use coercive power over their own members or those outside their group. Modern political theorists share Locke's skepticism toward religion in the political sphere. Perhaps most vigorously, John Rawls (1993) demanded that societies "take the truths of religion off the political agenda" (p. 151).
2. Since the pioneering study by Adorno, Frenkel-Brunswik, Levinson, and Sanford (1950), political scientists and psychologists have recently taken an increasing interest in the topic of authority. See, for example, Altemeyer (1998); Jost, Glaser, Kruglanski, and Sulloway (2003); Feldman (2003); Stenner (2005); Graham, Haidt, and Nosek (2009); and Hetherington and Suhay (2011).
3. Own translation based on the German original: "Aufs erste müssen wir das weltliche Recht und Schwert gut begründen."
4. Persuasion arguably is a quintessential facet of religion (Weithman, 2002). This holds particularly true for conversionary Protestantism (Meyer, 2010). Not surprisingly, some of the most vigorous debates among Protestant thinkers revolve around the topic (Bayle, 2005). A particularly contested passage is found in the New Testament. In the *Gospel of Luke*, for instance, Jesus tells his disciples: "Go out to the roads and country lanes and compel them to come in, so that my house

will be full" (Luke 14:23). This very notion of "compulsion" laid the ground-work for Christian missionary efforts around the globe.

5. The qualitative interviews, including all quotes, are available in the supplementary material.

6. For detailed analyses regarding the Catholic Church's decline in Latin America, see Stoll (1990) and Gill (2008).

7. Only 1.4% of 1,024 survey respondents belong to no church. Similarly, 95.4% of the sample pray regularly.

8. In the supplementary material, we provide transcripts of recorded sermons in all villages that buttress the descriptions in this section.

9. In one village, 18 converts belonged to a small mission called Israelita, which is comparable with the Adventists in theological fervor.

10. In particular, the research team divided each village into four equally sized blocks. In each block, the surveyors randomly determined which road to follow. Individuals on each road were drawn using a detailed randomization dictionary, which randomly determined the next respondent's houses number, gender, and age-bracket. The age-brackets reflected the recent Peruvian census, leading to a rather young sample with 50% below the age of 38.

11. Enumerators were instructed to randomize the order of the question. However, they were not given an exact predetermined time frame. This decision was taken to lighten the burden of the enumerators, who had to implement many different randomizations already.

12. All qualitative interviews are available upon request from the authors.

13. Note that by obedient (persuadable), we mean more obedient (persuadable) than the average population. Without loss of generality and to make the following analysis seamless, we conceptualize "non-persuadable" as the positive causal outcome.

14. Note that Bayesian Integration of Qualitative and Quantitative Data (BIQQ) implicitly invokes a stable unit treatment value assumption (SUTVA) assumption.

15. BIQQ also allows one to characterize uncertainty over θ, using a prior probability distribution over these parameters. For simplicity and because the decision to use BIQQ was taken *after* the data were collected, we claim to be certain about all three parameters. Realistically, however, the parameters associated with meaningful uncertainty are π and λ. For this reason, we run sensitivity analyses.

16. See supplementary material for details regarding the likelihood function.

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