Essays on Inequality and Social Cohesion

Anselm Frieder Rink

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Abstract

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This dissertation comprises three essays that explore determinants of inequality and social cohesion. The first essay explores the role of inheritance customs in spurring social equality. Using historical data on inheritance customs in Germany, I document that municipalities that historically fairly shared wealth among siblings see higher levels of social equality today. I point to two mechanisms that help explain the correlation: increased wealth equality and stronger pro-egalitarian preferences. Interestingly, I also find that equitably inheriting communities are associated with higher incomes and *greater* income inequality. I interpret this finding to mean that equitable inheritance levels the playing field by rewarding talent not hereditary status. The second essay analyzes how Protestant missions affect community cohesion. Exploiting variation in missionary activity in southeastern Peru, I document that villages exposed to missions have lower levels of community cohesion compared to non-exposed villages. I adjudicate between two mechanisms that may explain this finding—social networks and pro-social preferences—and find the latter to be more plausible. The third essay expands on this finding by implementing a field experiment with a missionary group in South Sudan in order to parse out the causal effect of Protestant evangelism on social capital. Using attitudinal and behavioral measures, I document that missionaries lower group-level social capital while increasing individual-level pro-social behavior. Taken together, my dissertation adds theoretical considerations and empirical evidence to a broad debate in the social sciences that tries to make sense of variation in social equality and cohesion.

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"The lucky ones are curious"

Friedrich Nietzsche

1

Introduction

This dissertation explores why some polities are more socially equal and cohesive than others. In so doing, I draw on evidence from three settings (Germany, Peru and South Sudan), discuss two explanatory variables (inheritance customs and Protestant missions) and use a variety of methodological approaches and data sources.

The first substantive chapter tackles a broad question that has interested scholars for the better half of two centuries: Why are some societies more socially equal than others? Drawing on the French and American revolutionaries, I point to equitable inheritance customs—the equal division of property among heirs—as a likely determinant.[1] I test this hypothesis by exploiting municipality-level variation in inheritance customs across German municipalities. Comparing neighboring municipalities that adopt different inheritance customs, I document that equitably inheriting municipalities are more socially equal: their local councils include more women and ethnic minorities, and local elite Rotary clubs include fewer Aristocrats. Drawing on historical sources, I explore two mechanisms that may underlie this relation. Using historical as well as contemporary data, I demonstrate that

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[1]This chapter builds on joint work with Hanno Hilbig who is a graduate student in political science at Harvard University.

equitably inheriting communities historically had lower levels of land inequality and lower support for Hitler's fascist NSDAP party in 1933. Finally, I explore the role of economic growth. Here, I find that equitably inheriting communities are associated with higher incomes and *greater* income inequality. I interpret this finding to mean that equitable inheritance levels the playing field by rewarding talent not hereditary status.

The second substantive chapter explores cohesion in the context of a developing country. Specifically, I hone in on the relation between Protestant missions and community cohesion. Drawing on a long standing theoretical literature and qualitative interviews conducted in southeastern Peru, I point out competing logics about Protestant missions' effects on community cohesion. One the one hand, Protestant missionaries teach charity and establish regular venues of social interaction. On the other hand, Protestant missionaries propagate individual salvation and provide an identity along which communities may separate. The effect of the missions on community cohesion is, hence, unclear. To make headway on these conflicting predictions, I take advantage of variation in missionary activity in southeastern Peru. I document that villages exposed to Protestant missions have lower levels of community cohesion compared to non-exposed villages. I empirically assess two potential mechanisms that may explain the reduction. In particular, Protestant missionaries may reduce community cohesion by lowering pro-social preferences and by weakening social networks. Using a mediation framework, I point to sparser networks as the more plausible mechanism.

The third substantive chapter is intended to solidify the causal argument distilled in Peru. I present evidence from a field experiment implemented in cooperation with a missionary group in South Sudan, which randomly assigned sixty local villages to a two-week intensive evangelism intervention. Using attitudinal and

behavioral evidence gathered after the missionaries leave, I find no differences in a pre-registered aggregate social capital index across treatment and control villages. However, I document that group-level indicators of social capital are lower, while individual-level indicators of social capital are higher in treated villages. The findings imply that missionaries are akin to a network interruption, buttressing the results from Peru. Missionaries seemingly undermine structural determinants of social capital, but increase individual-level pro-social behavior.

Drawing on evidence from three diverse contexts, my dissertation cautions that social inequality and cohesiveness exhibits profound variation even within small polities. I find that neighboring villages in Peru are shaped by starkly different cooperation patterns owing to the presence of Protestant missions. Similarly, in Germany I see differences from one village to the next in the degree to which women are represented in local councils due to different inheritance customs. What may be most surprising is that differences in cohesiveness are not only pronounced, but can change at short notice. As much demonstrates the field experimental evidence from South Sudan. Taken together, I hope that the three chapters— in addition to their careful treatment of causality and mechanisms—add to a literature on social equality cohesiveness (and related outcomes) by demonstrating that both outcomes exhibit profound variation at the local-level, and are more volatile than I would have expected before writing this dissertation.

In addition to these academic insights, the empirical findings may also be of interest to policy makers. Perhaps most intuitively, the chapter on inheritance customs may add to a debate whether equitable inheritance should be legally required. Given the normatively desirable effects of equitable inheritance *within* families on social equality, one may also wonder whether a more equal distribution of wealth across families might further push social equality. What is more, how-

ever, inheritance also seemingly affects broader social outcomes such as income and income inequality. Given that inheritance, to this day, is shared inequitably among heirs in many societies, my results may help inform a debate about the societal consequences of passing on wealth.

The chapters exploring the repercussions of Protestant missionaries may add to a policy debate on religious freedom and tolerance. Traditional missionaries may be less widespread these days. But religious entrepreneurs are many and active—not just in developing countries. A variety of studies have linked Protestant (and other) missionaries to desirable social outcomes. My dissertation marks a cautionary counterpoint. I show that missionaries fragment social networks and lower community cohesion. Thus, my dissertation may help forge a more nuanced debate about religious freedoms, including conversion. After all, the freedom to convert may come at the expense of societal cohesion.

Following this short introduction are the three substantive chapters of my dissertation. Each chapter is self-containing with its own supplementary information. I offer concluding remarks in chapter five.

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Inheritance and Inequality

2.1 Introduction

Why are some societies more socially unequal than others? While inequality, broadly conceived, occupies social scientists and commentators alike, "research on the roots of inequality," laments Boix (2010), "remains rather limited." Given that egalitarian-spirited societies witness higher levels of self-reported happiness, Acemoglu, Robinson and Verdier (2012, 3) pointedly ask: "can't we all—meaning all nations of the relatively developed world—be more like Scandinavians?" What, then, explains variation in social inequality?

One salient determinant of social equality, highlighted during the French and American revolutions, are inheritance customs. Many influential revolutionaries, like Alexis de Tocqueville (2003, 31), argued that to achieve social equality society had to fairly distribute wealth within families (known as *equitable inheritance*). By contrast, *inequitable inheritance*—particularly primogeniture, the passing of wealth to the firstborn child—was seen as the main culprit for the strict social hierarchies of the *ancien régime* (Beckert, 2007, 91-92). In 1790, the French rev-

olutionaries therefore abolished inequitable inheritance. Their declared goal was to ascertain "'equal rights' and 'equality before the law' " (King and Smith, 2005, 80), to empower women, and to put an end to aristocratic domination that had shaped France for centuries.

But does equitable inheritance succeed in leveling the societal playing field? This chapter empirically explores Tocqueville's hypothesis, analyzing whether equitable inheritance customs are associated with greater social equality. My empirical focus is on historical agricultural inheritance customs in Germany. I scrutinize agricultural inheritance because the passing of wealth, throughout most of modern history, took the form of transferring agricultural property (Giesey, 1977). I use Germany as the primary empirical case because it exhibits pronounced municipality-level variation in inheritance customs (see Figure 2.1). Particularly in Germany's South-West, inheritance customs regularly vary from one village to the next.

The historical roots of this variation are debated among scholars. Three broad theories can be delineated. The traditionalist theory contends that equitable inheritance is a remnant of Roman culture (e.g., Schulz, 1926). A peasant liberation theory argues that equitable inheritance developed where farmers obtained sufficient economic autonomy (e.g., Kölb, 1978). A final economic theory stipulates that equitable inheritance is linked to profitable crops (e.g., Röhm, 1957). While the theories help explain broad trends across Germany, they fall short of explaining pronounced local-level variation within counties and even municipalities.

Moreover, when regressing an indicator of equitable inheritance on proxies for the three theories, I reject the null hypothesis of joint significance. As such, inheritance customs may be sufficiently old and sticky to function as an independent variable. Several historians support such an interpretation. Forst, for example,

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argues: "institutions as deeply linked to the ethical perceptions and economic necessities, as long standing and intertwined with the lives of peasants as inheritance customs cannot be fundamentally changed" (1921, 21).

Having made a plausible case for exogeneity at the local level, I then test whether inheritance customs are associated with modern-day outcomes pertaining to social equality. Using data on inheritance customs across 24,500 historical German municipalities and a novel geographic matching procedure, I demonstrate that equitably inheriting communities show greater levels of social equality today. Specifically, these communities elect more women and ethnic minorities into local councils. And, members of local elite clubs (Rotary International) are less likely to be aristocrats. In sum, I present evidence that equitable inheritance plausibly increased social equality by attenuating differences across socially salient strata.

In a second step, I explore two mechanisms that may help explain this finding. Specifically, Tocqueville and other revolutionaries argued that equitable inheritance fosters social equality by fairly apportioning wealth and by engendering pro-egalitarian preferences. I test these mechanisms using historical as well as contemporary data. Regarding wealth inequality, I confirm that equitably inheriting municipalities had lower levels of land inequality in 1895. Regarding pro-egalitarian preferences, I confirm that equitably inheriting communities were more likely to oppose Hitler's fascist NSDAP party in the decisive 1933 election. And, to this day, these communities vote in greater numbers for left-leaning political parties and express greater support for disadvantaged groups. Taken together, I thus point to wealth equality and pro-egalitarian preferences as two plausible intermediate outcomes that may link equitable inheritance to social equality.

In a final step, I explore the role of economic growth. The French and American revolutionaries did not form testable hypotheses about equitable inheritance

customs' effect on income, while historians and social scientists express conflicting predictions. One set of scholars argues that equitable inheritance lowers labor supervision costs, given that all children have an incentive to work, which increases incomes. These scholars also contend that equitable inheritance plausibly lowers population growth, given that families have an incentive to prevent the atomization of their property, which, too, increases incomes. By contrast, a different set of scholars maintains that equitable inheritance leads to fragmented farms, thus lowering incomes.

We explore these hypotheses using historical census-based data on per capita income in 1892 and 1901 as well as data from 50 million German tax records in 2014. Our models show a consistent positive correlation between equitable inheritance and per capita income. Interestingly, however, I also find that equitable inheritance correlates with *greater* levels of income inequality in 2014 (historical income inequality data is not available). I interpret this finding to mean that equitable inheritance fosters social, but not economic equality. It thus achieved what the American revolutionaries had intended: an "equalitarianism [...] among pioneers, [whose] only inequalities were those of ability" (de Visme Williamson, 1976, 102).

The outcomes discussed in this chapter connect to several distinct literatures in political science. First, I present a new explanation for variation in representation of ethnic minorities and women in political offices (e.g., Kostadinova, 2007; Roberts, Seawright and Cyr, 2013). Second, I add to a literature scrutinizing wealth and income inequality, pointing to inheritance customs as a salient determinant (e.g., Keister and Moller, 2000; Boix, 2010). Third, I connect inheritance customs to pro-egalitarian preferences and, by extrapolation, preferences for redistribution (Corneo and Grüner, 2002; Alesina and La Ferrara, 2005; Scheve,

Stasavage et al., 2006). More broadly, the chapter draws attention to inheritance as a pivotal institution that shapes societal outcomes. While sociologists and historians have recognized the profound influence of inheritance customs, political scientists have mostly focused on the effects of inheritance on regime stability (Shin and Kim, 1985; Gates et al., 2006; Kokkonen and Sundell, 2014), and political violence (Midlarsky, 1988; Muller, Seligson and Midlarsky, 1989; Lichbach, 1989). In so doing, I offer support for an old explanation why some polities are more socially equal than others: equitable inheritance customs. Last, the chapter makes a methodological contribution by presenting a new way to analyze geographic discontinuities. Based on Keele and Zubizarreta (2015), I present an adjacent geographic unit matching design, which compares bordering municipalities in order to isolate causal effects.

To assess the relation between inheritance and social inequality, this chapter studies two mutually exclusive and widely practiced inheritance customs, equitable and inequitable inheritance. Research on inheritance abounds in the social sciences (e.g., Stiglitz 1969; Alston and Schapiro 1984; Todd 1994; Baker and Miceli 2005). To our knowledge, however, no empirical study has systematically assessed the link between inheritance customs and the broader political development of modern society, particularly inequality. This is surprising. After all, writes Habakkuk (1955, 4), "differences in inheritance systems [...] left permanent marks on [society's] social and economic structure." In what ways, then, may inheritance customs affect the social fabric of society?

2.1.1   Inheritance and Social Inequality

In 1790, at the height of the French revolution, the French Constituent Assembly partly abolished primogeniture—the passing of wealth to the firstborn son. The

revolutionaries' declared goal was to overcome the rule of the feudal aristocracy, and the strict social hierarchies of the *ancien régime*. The new French society, in Marx' words, marked the victory of "bourgeois ownership over feudal ownership [...], of the division of land over primogeniture" and of "the family over the family name" (cited in McPhee 1989, 1266). The abolishment of primogeniture—a structurally unequal manner of transferring wealth—was widely believed to be a pivotal mean to achieve social equality and became a center point of the heated revolutionary debates. Beckert (2007) writes:

> "*In France, inheritance law also became an important focus of political debate at the time of the Revolution. Particularly, the institutions of primogeniture and entail were rejected as structural elements of the ancien régime which were seen as incompatible with the revolutionary principles of freedom, equality, and fraternity.*"
> Beckert (2007, 91-92)

The American revolutionaries, too, believed in the effectiveness of fair inheritance customs to achieve social equality. In *Democracy in America*, Alexis de Tocqueville writes that "the law of inheritance was the last step to equality" (2003, 31). In particular, Tocqueville argues that equal inheritance customs had democratized the United States. He writes: "the last trace of ranks and hereditary distinctions is destroyed; the law of successions has hastened the process of leveling everywhere" (cited in Giesey 1977, 271). Tocqueville goes so far to claim that equitable inheritance is a sufficient condition to achieve social equality. He writes: "[once] the legislator has [...] regulated the law of inheritance, he may rest from his labor" (2003, 32).

In revolutionary France, the most immediate effect of the abolition of primogeniture was felt among women and aristocrats. Equal inheritance meant that

women could inherit. It also meant that the feudal class was hard pressed to maintain the cohesion of family wealth. After all, writes Hurwich (1993, 699) in a careful historical analysis of the German nobility, "primogeniture or other forms of impartible inheritance was [...] [a] method by which noble families could avoid subdivision of their estates and consolidate wealth to hand on to future generations." Ekelund, Hébert and Tollison second that "[primogeniture] concentrated wealth in the hands of a few dynastic families," which "were entrenched within a centralized power system" (2002, 658). With reference to France, Rose (1986, 178) writes:

> "*In March 1790 the Constituent Assembly began by abolishing primogeniture for formerly noble property, at the same time as it abolished nobility itself. This meant that all the heirs of a property-owner could inherit, including daughters, and not just the eldest son or other male descendent, the previous practice. Two years later, in March 1793, the Convention extended equal inheritance rights to all kinds of property, and moreover this legislation was made retroactive to 1789. So that legally, brothers had to hand back a share of their property to their sisters; and many women exercised their rights, to the confusion of the courts.*"
>
> Rose (1986, 178)

Taken together, the historical experience in France and the U.S. points to a logic whereby equitable inheritance customs attenuate social inequalities. This holds particularly true for traditionally disadvantaged groups such as women. The "losers" in this process are first-born sons. Moreover, equitable inheritance undermines the hereditary social classes of higher status, notably, aristocrats. Such

families are no longer able to keep economic assets apiece, thus decreasing their economic and social clout. Our main hypothesis is thus as follows:

- Hypothesis 1: Equitable inheritance customs reduce social inequality

2.1.2 Mechanism 1: Wealth inequality

The most prominent logic linking equitable inheritance to social equality pertains to the distribution of wealth. Inequitable inheritance, by apportioning the entire plot of land to the eldest son, leaves the remaining children with little or no wealth. Equitable inheritance, on the other hand, achieves wealth equality by splitting property fairly among all children.

Before exploring this logic further, however, it should be noted that in Germany non-inheriting children at times received monetary compensations, calculated either on the basis of the property's value (*Verkehrswert*) or its profits (*Ertragswert*) (Habakkuk, 1955). Even within aristocratic families monetary compensation was not unheard of. Ekelund, Hébert and Tollison (2002, 658) write: "primogeniture could induce landless sons to remain bachelors, thereby reducing the pool of eligible heirs. Dynastic families considered it a duty not only to have children but also to care for them, which meant [...] launching them into the adult world with some measure of economic security." Besides monetary compensations, the authors point to the church as an insurance for non-inheriting sons. They write: "the medieval church served the landed aristocracy as a kind of insurer, or employer of last resort [...]. Moreover, female children could also find ready 'employment' in the church." This held particularly true in Catholic areas. As Fichtner (1989, 52-53) notes: "Catholics had a better way to enjoy the advantages of primogeniture, yet live with their consciences over the treatment of younger sons. They could still arrange appropriate livings for their offspring in the church." Last, it should

also be pointed out that in other European countries, primogeniture was some-
times avoided (Brodrick, 1881, 99). Evidence for systematic avoidance, however,
is scarce in the German context.

The link from equitable inheritance to a more equal distribution of wealth is a
common argument among social scientists. Blinder (1973), for instance, presents
a theoretical model and Menchik (1980) delivers empirical evidence demonstrating
a firm link between equitable inheritance and a more equal distribution of wealth
within families and within society at large. Thus, Menchik concludes: "economies
that feature primogeniture will have a greater degree of inequality than those fea-
turing equal division" (1980, 299). Similarly, discussing primogeniture in Europe
more broadly, Ekelund, Hébert and Tollison (2002, 67) write: "primogeniture en-
couraged the concentration of wealth". And, in a historical analysis of Lombardy,
Roberts (1953) notes that its nobility constituted a mere 1 percent of the popula-
tion, but owned nearly half of its property. The author attributes this inequality
in land to one particular institution: primogeniture (1953, 67). Our second hy-
pothesis is thus as follows:

- Hypothesis 2: Equitable inheritance customs reduce wealth inequality

2.1.3   Mechanism 2: Pro-egalitarian preferences

Besides a rather mechanical economic effect on the distribution of wealth, histo-
rians also point to habitual effects of equitable inheritance customs. Habakkuk
(1955, 4), for instances, writes that "inheritance systems exerted an influence on
the structure of the family, that is, [...] on the relations of parents to children
and between children." French sociologist Todd (1994), in his book *La destin des
immigrés*, argues that equitable inheritance customs foster a symmetric family
structure where the equality of siblings is taken for granted. By contrast, in-

equitable inheritance leads to an asymmetric family structure, which prizes the firstborn son. As a result, argues Todd (1994), equitable inheritance foster preferences for equality among children. Ekelund, Hébert and Tollison second that inequitable inheritance caused "untold bitterness within the family" (2002, 658).

Beckert (2007) makes a similar argument drawing on evidence from revolutionary France. He demonstrates that equitable inheritance was widely seen as instilling in people a sense of the equality of all citizens. Beckert writes: the "unequal legal treatment of different social ranks and of family members based on ascriptive characteristics was seen as a violation of natural equality" (2007, 92). A debate in the Assembleé Nationale in 1791 underlines this logic:

> "*I would not know, Gentlemen, how it should be possible to reconcile the new French constitution, where it heads with regard to the great and admirable principle of equality, with a law that allows a father, a mother, to forget in relation to their children, these sacred principles of natural equality, and to enlarge thereby in society the differences that result from the diversity of talents and from industry, instead of correcting them through the equal division of the household wealth.*"
> Mirabeau in Assemblée Nationale, April 2, 1791, 513 [cited in Beckert 2007]

Similar arguments about equitable inheritance's influence on pro-egalitarian preferences are found among political theorists. Hirschmann (2008), for instance, highlights that John Stuart Mill's *On Liberty* singles out inequitable inheritance, particularly *primogeniture*, as an immoral institution. According to Hirschmann (2008), Mill "opposes primogeniture and claims that parents have a moral obligation to bequeath to their children who are 'unable to provide for themselves' ".

The idea, here, is that equitable inheritance customs induce in children an idea to care for others less fortunate.

The link between equitable inheritance and pro-egalitarian ideals has also been pointed out among scholars of Protestantism in Germany. Fichtner (1989), for instance, argues that Protestant areas encouraged equitable inheritance as it epitomized the Protestant ideal of "equality among brothers" (1989, 14). Her arguments, however, have come under criticism for lacking a sound statistical basis (Soergel, 1990). Moreover, as I demonstrate in Table 2.2, Protestantism is a poor predictor of equitable inheritance customs.

Taken together, equitable inheritance may increase social equality by fostering a belief in the fundamental equality of all humans. I therefore formulate the following secondary outcome, which possibly mediates the relation between equitable inheritance and social inequality.

- Hypothesis 3: Equitable inheritance customs increase pro-egalitarian preferences

2.1.4   Inheritance and Income

The French and American revolutionaries strongly believed in the power of equitable inheritance to ensure social equality, by fairly distributing wealth and by spurring pro-egalitarian preferences. Meanwhile, the effects of equitable inheritance on per capita incomes was a contested issue. Social scientists, too, have formed conflicting predictions about the effect of equitable inheritance on incomes. On one hand, Alston and Schapiro (1984, 281) note that inequitable inheritance leads to "larger estates and agricultural wealth." Evidence from the United States buttresses this conjecture. Large plantations, particularly in the South, were historically almost exclusively inherited using primogeniture (Gray, Thompson and

Esther, 1933). Conversely, equitable inheritance is argued to fragment farms, which renders them economically futile.

On the other hand, there are also arguments that link equitable inheritance to income growth. Alston and Schapiro (1984), for instance, argue that equitable inheritance may lower labor supervision costs as all children have an incentive to work. Bertillon (1911), too, argues that income increases under equitable inheritance given that it leads to lower population growth as peasants aim to counter the atomization of their property.[1] And Ekelund, Hébert and Tollison (2002) argue that "partible [equitable] inheritance should also play a role in economic growth," by fostering "competition and capitalist exchange" (2002, 666). In sum, then, it is unclear whether equitable inheritance customs de- or increase per capita income. I formulate the hypothesis that has received stronger support in the historical literature.

- Hypothesis 4: Equitable inheritance customs increase per capita income

Besides an effect on incomes, an inquiry into the interplay of inheritance and inequality should explore whether inheritance affects income inequality. Income inequality is distinct from social inequality—defined here as a state where a society's resources are distributed unevenly along lines of socially salient categories of persons. Indeed, if equitable inheritance leaves but one inequality, namely ability, it may well have a positive effect on income inequality. Such an argument is in line with a common finding that economic growth increases income inequality (Kuznets, 1955; Aghion, Caroli and Garcia-Penalosa, 1999).

Such considerations were also common among the commentators of the French and American revolutions. As Martineau (1842, 15) writes, in America "[f]ew

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[1]This link, however, is disputable. For instance, the German kingdom *Baden*, which relied on equitable inheritance, witnessed substantial population growth, while the neighboring *Wü*, which predominantly relied on inequitable inheritance, did not.

are very wealthy; few are poor; and every man has a fair chance of being rich."[2] Indeed, de Visme Williamson (1976, 102) argues that the early American society was shaped by a "equalitarianism [...] among pioneers, [whose] only inequalities were those of ability and work", which may lead to an unequal distribution of incomes if talent is sufficiently skewed. I therefore formulate our final hypothesis as follows:

- Hypothesis 5: Equitable inheritance customs increase income inequality

2.2   Determinants of Inheritance Customs

To assess the relation between inheritance and inequality, I draw on empirical evidence from Germany. The country exhibits pronounced historical municipality-level geographic variation in inheritance customs (see Figure 2.1, which depicts inheritance customs circa 1900). Particularly in Germany's South-West, inheritance customs regularly varied from one village to the next. Red and yellow areas (equitable inheritance) are juxtaposed with green and light-green areas (inequitable inheritance). While there are broad trends (e.g., Bavaria in the south-east adopting inequitable inheritance), the variation is clearly visible in the federal states Baden-Württemberg, Hesse, Rhineland-Palatinate, Saarland and North Rhine-Westphalia.

Importantly, the historical inheritance customs continue to be implemented to this very day. While there is undoubtedly a general move toward equitable inheritance, vast areas of Germany still see wealth passed on to the firstborn child. Contemporary evidence, however, is scarce. For this reason, I rely on the data

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[2]It should not be forgotten, however, that such economic equality only prevailed among white men and "was surrounded by an array of other fixed, ascriptive systems of unequal status, all largely unchallenged by the American revolutionaries" (Smith, 1993, 549).

produced in Figure 2.1. But, I conducted five qualitative interviews with farmers to buttress that both inheritance customs are still practiced today. One farmer hailing from the Münsterland, for instance, stated: "*It may come as a surprise, but agricultural inheritance is still done like we did it two hundred years ago: it's given to the oldest son. We just want to keep the farms intact. [...] As a matter of fact, it's not just agricultural inheritance. In this area, even non-farmers inherit like that. It's partly due to the fact that the oldest son has a responsibility for the family's wealth.*"

What explains the variation in inheritance customs across Western Germany? Historians have proposed three competing theories. First, a *cultural* theory stipulates that inequitable inheritance is a remnant of Germanic customs, while equitable inheritance has its roots in the Roman legal tradition. Second, a *political* theory contends that equitable inheritance was driven by peasant liberation during the Middle Ages, while inequitable inheritance is observed in areas where peasants did not experience such autonomy. Third, an *economic* theory argues that equitable inheritance is linked to fertile soils, while inequitable inheritance is present in areas less suitable to agriculture.[3] The theories, which are mostly at odds with one another, are laid out and assessed in turn. To adjudicate between them empirically, I estimate a series of empirical models to explore how much of the regional variation of inheritance can be explained by the theories (Section 2.4.5).

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[3]A possible fourth theory is that equitable inheritance is linked to Protestantism. The logic here is that Protestants value human equality more so than Catholics and therefore prefer equitable inheritance (see, Fichtner, 1989). This argument, however, is widely disputed. I therefore do not discuss this explanation in depth. I do, however, control for Protestantism (specifically, a commonly used instrument for Protestantism—the distance to Wittenberg) in the empirical analyses in Table 2.2.

Figure 2.1: Map of West-German inheritance customs
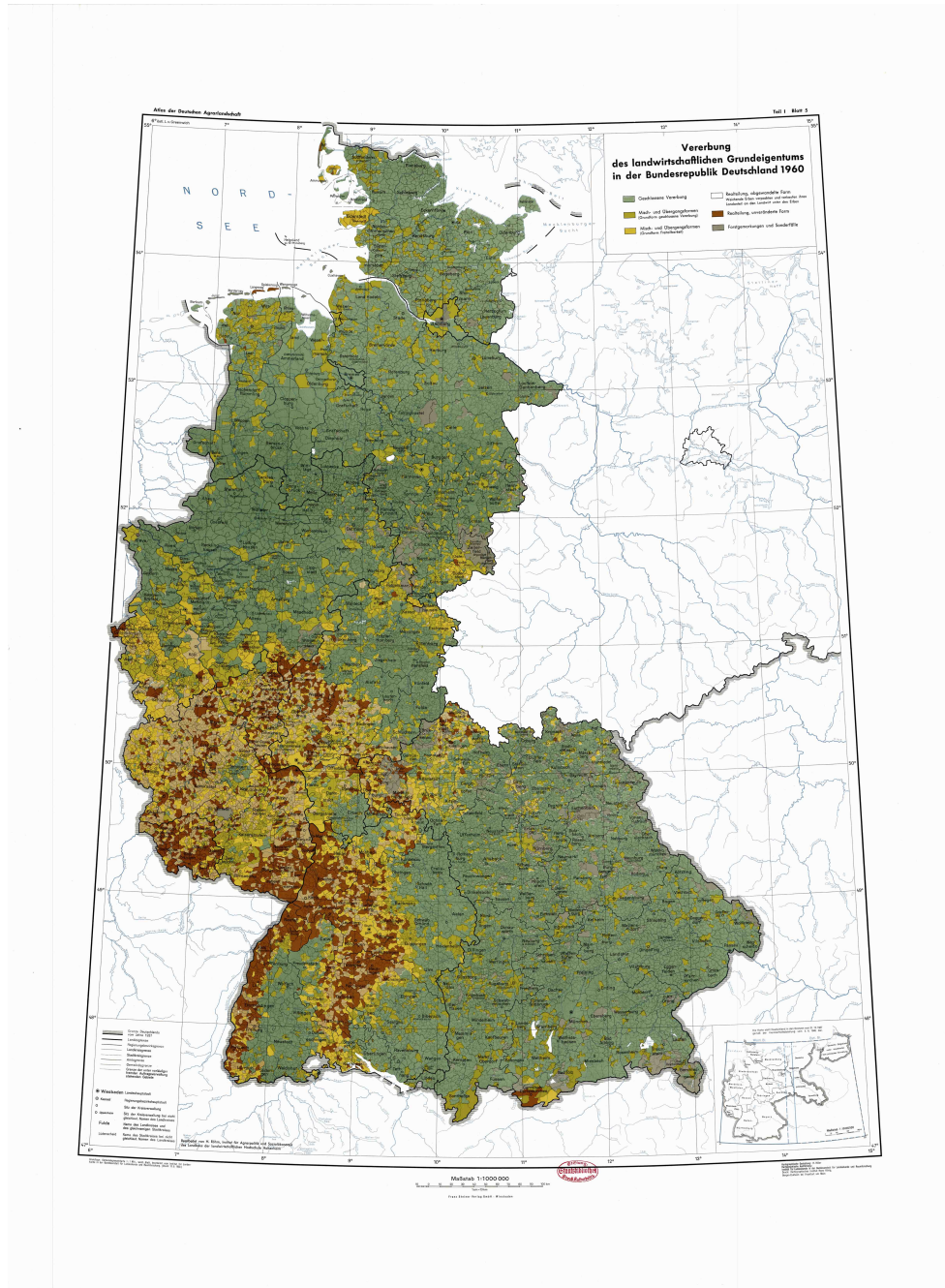
Notes: Based on (Röhm, 1957). Dark green depicts inequitable inheritance, red depicts equitable inheritance, light green depicts mixed (historically inequitable) inheritance, yellow depicts mixed (historically equitable) inheritance.

2.2.1 Culture

A first set of scholars argues that cultural differences explain variation in inheritance customs. According to this theory, inequitable inheritance is a remnant of Germanic practices, while equitable inheritance is a vestige of Roman culture (Schulz, 1926; Huppertz, 1939). While reliable historical evidence is sparse,[4] Germanic inheritance customs are widely believed to have been structured in reference to families (*Sippe*). All members of German families—save families in serfdom—owned the land together. The death of the patriarch did not substantially alter the structure of the land; it remained in the family, which was led by the eldest son (Grönbech, 1937). On the contrary, Roman inheritance customs were rooted in a firmly individualist tradition (Kölb, 1978). Property belonged to individuals, not entire families. As such, Roman inheritance customs would make it more likely for property to be divided among children, given the absence of any norm stipulating that land must stay apiece within the family.

Taken together, the cultural theory argues that areas in modern-day Germany with greater exposure to the Roman empire (i.e., areas south of the *Limes*) are more likely to adopt equitable inheritance customs. In contrast, areas with a firmly Germanic tradition are more likely to rely on inequitable inheritance. A cursory analysis of inheritance practices (see Figure 2.1), shows that equitable inheritance is, indeed, more prevalent in the former Roman areas of modern-day Germany. Yet, there is still sizable historical variation in Germanic as well as

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[4]One of the first accounts on German inheritance customs can be found in Tacitus' *On the Origin and Situation of the Germanic Peoples*, published around 98 AD. Tacitus remarks that the Germans "till every year new fields, and there is still (untilled) land left over" (Tacitus 1999; Chapter 26). Early commentators interpreted Tacitus' remark to mean that the Germans engaged in a form of agricultural communism whereby properties are kept in one piece within families and communities (Ernst, 1926). Most modern historians, however, agree that the Germans endorsed private property early on. Only select areas of villages, such as hunting grounds, were common property (Weller, 1927).

Roman areas, which the theory fails to explain. The Rhineland, for instance, is shaped by equitable inheritance despite having little relation to the Roman empire. Bavaria, by contrast, mostly relies on inequitable inheritance despite having been part of the Roman area.

2.2.2 Politics

A second set of scholars contends that political developments, particularly the liberation of peasants, gave rise to variation in inheritance customs. According to this *peasant liberation* theory, equitable inheritance is closely linked to peasants' freedom to own (Abel, 1956). Scholars adopting this view commonly take the advent of the Carolingian dynasty as their starting point. The rule of Charles the Great (c. 742 to 814 AD) led to a profound re-configuration of the peasant class. While in classical antiquity, peasants and noblemen had not differed substantially in terms of their income and political clout (Lütge, 1966), the rein of Charles marked the German nobility's steady rise to power. The elite's appropriation of new lands led to an ever-growing number of serfs who cultivated the *demesne* lands. Importantly, the logistical burdens implicit in this growth meant that serfs had to be granted significant personal liberties. The result was the emergence of semi-free peasants.[5] The growth in personal liberties and economic prowess significantly increased the peasants' confidence. They began to demand what they deemed rightfully theirs: the right to own and inherit property. As a result, several regions of modern-day Germany witnessed new forms of peasant inheritance. Examples are the *Landsiedelleihe* or the *Freistift*—two forms of leasing property. Thus, writes Kölb (1978, 58), "the improvement of property rights in the form of

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[5]This included semi-free peasants such as *Ackerbürger* and *Grundholde*. Of particular importance for the Carolingians were *Königsfreie* (King's freemen)—a class of semi-free peasants who were part of the Carolingian army.

inheritance led farmers to partibly inherit their property, because they wanted all their descendants to share in their possession."[6]

In sum, peasant liberation theory contends that equitable inheritance is more likely in areas where peasants demanded or were granted significant autonomy. In contrast, areas where peasants lacked such autonomy were more likely to rely on inequitable inheritance, where property stayed apiece so as to maintain power of (aristocratic) families. Taking the exposure of municipalities to the German peasant wars (circa 1522 to 1525) as a proxy for peasant's propensity to demand basic property rights, equitable inheritance is more likely close to the historical center of the wars. Yet, peasant liberty theory fails to explain the significant variation that remains. Notably, as can be seen in Figures 2.8 and 2.10 in the supplementary information, the starting region of the Peasant wars—the area around the town St. Blasien—historically relied on both equitable and inequitable inheritance.

2.2.3 Economy

A final group of scholars has proposed an *economic* theory of inheritance customs, whereby equitable inheritance is linked to fertile soils and profitable crops. Scholars proposing such economic considerations typically use the High Middle Ages as their starting point—a period of increasing peasant migration and professionalization. Schulze (1974), for instance, demonstrates that the *Königsfreie*—peasants who were granted significant autonomy by the Carolingians, which included the right to inherit property—mostly settled in border areas of Francia. These areas had traditionally offered little profit, partly owing to poor infrastructure; Agri-

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[6]Some rulers, particularly in Northern Germany, reacted to the growing demands from peasants by instituting the *Meierhof* system, whereby farms of the noble or ecclesiastical classes were occupied by administrators. The system was aimed at effectively countering inheritance, particularly of the equitable kind.

culture was notoriously hard. As a result, these areas did not witness equitable inheritance as would have been predicted by peasant liberation theory (Abel, 1956). Rather, the low returns on investment meant that property was passed on to only one son (inequitable inheritance) so as to prevent the land from getting too small to make a profit. Röhm notes that "where parceled corridors existed or still exist [...], the division of land [...] is observed more often than in areas with more or less scattered settlements." (1957, 3).

Taken together, scholars proposing an economic theory argue that favorable climate and soil conditions determine whether a given area can afford to adopt equitable inheritance. Areas with unfavorable soil and climate conditions, on the other hand, must rely on inequitable inheritance. Though theoretically intuitive, this theory does not seem compelling when comparing climate and soil conditions in modern-day Germany (see Figure 2.9 in the supplementary information) to historical inheritance customs. There is no apparent correlation between inheritance customs and agricultural suitability at the municipality-level. As such, Kölb (1978) argues that "poor soil and unfavorable climate did not spread closed inheritance customs".

2.2.4 Inheritance Data

To judge the explanatory power of these theories more rigorously, I collected data from a variety of sources. Before laying out these data sources, however, I will first introduce the key data source of this chapter, namely, our measure for inheritance customs.

To our knowledge, the most comprehensive data on German inheritance customs was collected by Helmut Röhm (1957) as part of an effort by a history

commission to map Germany's agricultural landscape.[7] Röhm sent a detailed questionnaire to all 24,547 German municipalities (*Verwaltungsgemeinde*)—the smallest administrative unit—which was to be answered by local-level administrative officials.[8]

The resulting map was printed in the *Atlas der Deutschen Agrarlandschaft* and is provided in Figure 2.1. The map provides information on agricultural inheritance customs historically prevalent in a given municipality. The two key variables of interest are (1) *equitable*, which refers to municipalities where inheritance was historically evenly split among heirs, and (2) *inequitable*, which refers to municipalities where inheritance was given to the eldest born. In addition, the map conveys information on whether a municipality has witnessed a change in inheritance customs since circa 1900, producing two further variables: *Mixed (inequitable)* refers to communities that historically relied on inequitable inheritance, but have gradually witnessed the adoption of equitable inheritance customs. *Mixed (equitable)* refers to communities that traditionally adopted equitable inheritance, but have witnessed the adoption of inequitable inheritance in recent years. Finally, the map has information on areas where no agricultural inheritance was passed on. This mostly refers to public and forestry areas (*Forest*). Once again, it should be pointed out that inheritance customs were never standardized across Germany. To this day, agricultural (and non-agricultural) property is frequently passed on to the firstborn child. Systematic evidence about inheritance customs today, however, is not available.

Since West Germany continuously reduced the number of municipalities (8,670

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[7]A variety of historical sources have assessed German inheritance customs. Notably, Sering (1900) collected detailed information around the turn of the century. These records, however, did not use uniform measurements across municipalities and were insufficiently granular to afford a local-level empirical design.

[8]To our knowledge, the survey was answered by all officials. The questionnaire is provided in Figure 2.12 in the Supplementary Information.

Table 2.1: Inheritance customs across German municipalities

	ca. 1900	ca. 1950
Equitable	28%	16%
Inequitable	66%	52%
Mixed (equitable)		12%
Mixed (inequitable)		14%
Forest	5%	5%

as of 2015), I overlaid Röhm's map with Germany's current municipal administrative boundaries. To convert the map's information into a numerical vector I used an algorithm that counts the number of pixels associated with a given inheritance custom. Given that the 2015 municipalities tend to be larger than the municipalities of 1959 and given that the algorithm has random measurement error,[9] the resulting treatment measure is continuous. However, I also construct a dichotomous treatment indicator, where the treatment variable with the highest share of pixels gets a 1, while all others are assigned a 0. The dichotomous measure is shown in Table 2.1.

As can be seen, in 1900 inheritance customs were split between equitable (28%) and inequitable (66%). By 1950, these numbers had reduced to 16% and 52%, respectively, where the remaining municipalities are either public forest land or adopt a mix of both systems. I use the 1900 inheritance customs as our key independent variable. This variable, notes Röhm (1957), is a good proxy for historical customs and had, at the time, been stable for centuries. Yet, future work may find it worthwhile to explore why certain municipalities have shifted from one form to the other between 1900 and 1950.

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[9]The algorithm counts the number of pixels of a given treatment color in Röhm's map. Thus, areas with imperfect coloring or with city names are not counted appropriately. Such error, however, is plausibly random.

2.2.5 Balance

Having laid out the inheritance data sources, I turn to our key quantitative check regarding the determinants of the customs. Specifically, I collected data on Roman rule (cultural theory), peasant liberation (political theory) and agricultural suitability (economic theory).

We measure Roman rule using data from the Digital Atlas of Roman and Medieval Civilizations (McCormick, Huang and Gibson, 2007). I construct a dichotomous Roman rule variable, which takes on the value one if the municipality is situated within the 137 CE borders of the Roman Empire, and zero otherwise.

We measure peasant liberation by digitizing a map provided in Putzger (1970), which contains information on the extent of the peasant wars between 1522 and 1525. Since the Putzger data is less detailed than the Röhm data, I measure peasant war involvement on the county level. For each county, I obtain a variable ranging from zero (not involved in the peasant wars) to one (wholly involved in the peasant wars). The Putzger map and our adaption are printed in the supplementary information (Figure 2.8 and 2.10, respectively).

To measure agricultural suitability, I have collected several variables: mean and SD of elevation (the source is NASA; Jarvis et al. 2008), mean temperature (the source is the German National Climate Data Center; Kaspar et al. 2013), and soil organic carbon content (the source is the European Soil Database; Panagos 2006).

To assess to what degree the three theories can explain variation in inheritance customs across municipalities, I regress a dummy for equitable inheritance customs on proxies for the three theories (Table 2.2) state fixed effects. In so doing, I also include the variable *Distance to Wittenberg* in order to proxy for Protestantism (see Footnote 3). As can be seen, the coefficient sizes are consistently small and

Table 2.2: Pre-treatment covariates and equitable inheritance

	Equitable
	(1)
Cultural: Roman rule	-0.005
	(0.018)
Political: Peasant wars	0.804
	(0.402)
Economic: Mean elevation	-0.734
	(0.095)
Economic: SD of elevation	0.001
	(0.0002)
Economic: Ruggedness	-0.002
	(0.001)
Economic: Mean temperature	0.001
	(0.006)
Economic: Soil carbon content	-0.007
	(0.006)
Protestantism: Distance to Wittenberg	0.002
	(0.005)
Observations	8,657

Notes: Regression of salient pre-treatment covariates depicting theoretical determinants on binary equitable inheritance dummy. County fixed effects included. Models control for longitude and latitude.

measured with noise. What is more: the effect signs are not in line with the theoretical predictions. The cultural theory stipulates that Roman rule led to equitable inheritance. Yet, I estimate a negative coefficient for the Roman rule variable. The political theory argues that peasant liberation led to equitable inheritance. While the coefficient for peasant wars points in the right direction, it is estimated with significant uncertainty, despite an overall sample of over 8,500 municipalities. Finally, the economic theory argues that suitable soil conditions led to equitable inheritance customs. However, the estimates for the soil data are consistently small, insignificant and tend to point into the wrong direction. The most direct measure, soil carbon content, for instance, yields a negative coefficient.

2.2.6 Exogeneity

Taken together, the historical discussion coupled with the empirical analysis confirms that the precise determinants of inheritance customs remain unclear. The three theories, given their weak empirical backing and conflicting logics, raise an important question: To what degree can inheritance customs be conceptualized as an "independent" variable that affects social outcomes? Two epistemological positions are defensible.

First, one may take the view that inheritance customs cannot, by principle, be viewed as an independent variable, given that they were not exogenously manipulated. Here, then, the theories can provide useful guidance as to which causal pathways must be adjusted for in an empirical analysis. An empirical model linking inheritance to social equality should then adjust for three important causal pathways. These are a region's (1) exposure to Roman culture, (2) the degree to which peasants were free during the Middle Ages, and (3) the geological suitability of agricultural lands.

Second, given that the three most salient theories could not be confirmed quantitatively, one may argue that inheritance customs are sufficiently old and sticky to act as independent variables.[10] Historians support such an interpretation. Röhm, for instances, notes that up until the 1930s German "agricultural inheritance customs were neither influenced by scholarly doctrines nor by legislative changes" (1957, 2).[11] Forst (1921) takes the view that German inheritance customs have not fundamentally changed, whatsoever: "institutions as deeply linked to the ethical perceptions and economic necessities, as long standing and intertwined with the lives of peasants as inheritance customs cannot be fundamentally changed – not even by law." (1921, 21).[12]

In the empirical section, I try to accommodate both views by conducting simple regression analyses as well as a careful geographic matching designs, whereby adjacent villages are paired and observable differences minimized across communities adopting equitable and inequitable inheritance.

2.3   Data

2.3.1   Social inequality

My main outcome of interest is the degree to which local German municipalities can be characterized as socially equal. I define social inequality as a state where a society's resources are distributed unevenly along lines of socially salient categories of persons. Two groups are particularly salient in the historical discourse: women

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[10]We define exogeneity as $E[\epsilon|X] = 0$.

[11]The Prussian Enquete in 1929 *"Agricultural Inheritance Customs in Germany"* came to a similar conclusion.

[12]Such a reading is supported by case studies outside of Germany. For example, in South Tyrol-Alto Adige—Italy's northernmost province—inequitable inheritance (*geschlossener Hof*) was introduced in the 6th century by the Bavarians. Since then, the institution has been firmly entrenched (Mori and Hintner, 2013). Attempts by the Italian government to impose Italian inheritance customs—i.e., equitable inheritance—were unsuccessful.

and aristocrats. The former marks a historically disadvantaged group, while the latter marks a historically favored group. I measure social equality in three ways, capturing the status of women, ethnic minorities and aristocrats.

Female representation. My first measure of social inequality is the share of women in municipal councils (Caul, 1999, 2001; Htun and Weldon, 2010). In so doing, I draw on fine-grained data collected by Ruth Ditlmann and Rafaela Dancygier (ongoing research project). For each municipality, I calculate (1) the percentage of female members in municipal councils, and (2) the percentage of female members in municipal councils, restricted to women that are members of the five largest parties. Currently, the data by Ditlman and Dancygier is limited to municipalities with more than 2000 inhabitants. Therefore, all results on female representation are based on a subset of about 46% of all municipalities in West Germany.

Minority representation. My second measure of social inequality is the share of ethnic minorities in municipal councils (Togeby, 2008; Dancygier, 2010). Here, I use the same dataset as outline above. In particular, for each municipality I calculate (1) the percentage of members in local councils that do not have a German last name, and (2) the percentage of members in local councils without a German last name, restricted to the five major parties.

Aristocratic representation. My third measure of social inequality is the share of aristocrats in local elite networks. To measure elite networks, I obtained a list of all members of *Rotary International* in Germany. Rotary members are mainly composed of business and civic leaders and consider themselves to be part of the social elite. Based on this list, I am able to determine (1) the total number of members of each chapter as well as (2) the number of members from aristocratic families. To identify aristocratic members, I use German naming conventions that

are unique to aristocrats.[13] For each municipality where a Rotary chapter exists, I calculate the share of aristocratic members. Rotary is only active in roughly 600 (mostly larger) German municipalities, which reduces our sample size significantly.

2.3.2 Mechanism 1: Wealth inequality

My most immediate mechanism, which plausibly links equitable inheritance to social equality, is the fair transmission of wealth. In order to scrutinize the historical effect of equitable inheritance on wealth equality, I use data on agricultural land inequality in 1895. The data was originally collected by Ziblatt (2009), who obtained it from the 1898 agricultural census in the German Empire. The census contains information on the size of over 5 million agricultural units in 1,004 counties in 1895. In each county, agricultural units are divided into 18 size categories. For each of these categories, the total number of farms, as well as the area held by those farms, is recorded. Ziblatt aggregates the data to the level of the electoral district (*Reichstagswahkreis*) and then calculates the GINI index of agricultural landholding. To combine our inheritance data with the landholding GINI, I aggregate Röhm's measure of inheritance customs to the level of the 1895 German electoral districts. This leaves us with an overall sample size of 219 electoral districts.

2.3.3 Mechanism 2: Pro-egalitarian preferences

A second hypothesized intermediate outcome, linking equitable inheritance to social equality, are pro-egalitarian preferences. I use three measures for pro-egalitarian preferences in order to capture historical as well as contemporary pref-

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[13]For example, names may include the words "von" or "zu" (meaning "of"), which indicate the geographic origin of an aristocratic lineage. Oftentimes, titles are also officially part of a name, like "Graf" (count) or even "Prinzessin" (princess).

erences.

First, to parse out the historical effects of equitable inheritance on pro-egalitarian preferences, I obtained data from the 1933 election. A long literature has associated preferences for social equality with parties on the political left (see Orren, 1985, 387). As Boix (1998, 5) writes "left-wing or social democratic parties [...] especially care about [...] equality," while conservative parties tend to "reject any equalization process" (see also, Hewitt, 1977). I use the 1933 election given its decisive influence on world history. Specifically, I assess the vote share for Hitler's fascist NSDAP party as a revelation of anti-egalitarian preferences. In so doing, I use data collected by Hänisch (1989). The data contains detailed information on election results in 925 German counties (*Kreise*). For each county, I define NSDAP vote shares as the number of votes for the NSDAP, divided by the total number of votes cast. Since our inheritance custom data is on the level of current German municipalities, I aggregate it, and then merge it with 1933 election data. In the end, I have complete information for 605 counties. The remaining counties are located in the Eastern parts of Germany, for which I do not have data on inheritance customs.

My second measure of pro-egalitarian preferences is today's vote share of left-leaning parties. Here, I collected the vote share for all major German parties in the four most recent elections: The federal elections in 2009 and 2013, as well as the European elections in 2009 and 2014. Older electoral outcomes are not available for the 2015 municipalities. The data was obtained from the *Statistisches Bundesamt*.[14] In the empirical analysis, I focus on the five most important parties: the conservative *CDU* (and its Bavarian sister party *CSU*), the liberal-conservative *FDP*, as well as the three left-leaning parties: *SPD*, *Bündnis 90/Die Grünen*

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[14]https://www.destatis.de

(Green party) and *Die Linke* (Left party). The summary statistics of the electoral outcomes are presented in the supplementary information (Table 2.5).[15]

My third measure for pro-egalitarian preferences is a survey item administered in the German Socio-Economic Panel Survey (SOEP). SOEP is a longitudinal survey containing socioeconomic information on private households in Germany. The SOEP has been fielded since 1984. It contains information on the location of all households in the survey. Therefore, I was able to exactly match the equitable inheritance variable to the SOEP. Given circa 8,500 municipalities, I thus have an average of 2.5 respondents per municipality. Following Alesina and Fuchs-Schündeln (2007), I measure pro-egalitarian preferences using four items, asked in the year 2002, concerning the role of the state in providing social security. The main question reads "At present, a multitude of social services are provided not only by the state but also by private free market enterprises, organizations, associations, or private citizens. What is your opinion on this? Who should be responsible for the following areas?" I selected four areas: "financial security in case of unemployment," "financial security in case of illness," "financial security for persons needing care" and "creation of employment opportunities." Answers are recorded as five-point Likert scales ranging from one (only the the state is responsible) to five (only private organizations are responsible).

2.3.4   Income

To measure inheritance customs' effects on income, I use two measures—one historical and one contemporary.

I measure historical incomes using data from the Prussian Economic History Database (iPEHD; Becker et al. 2014). The iPEHD data base contains a rich set

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[15]We use the so-called second vote (*Zweitstimme*) when analyzing electoral results at the federal level, given that it is the more salient electoral outcome.

of county-level variables taken from Prussian censuses in the years between 1816–1901. Specifically, I obtain average daily wages for male laborers in 1892 and 1901, measured in Marks. For each year, wages are recorded separately for urban and rural laborers, which leaves us with a total of four outcome variables. The Prussian territory, however, only covered part of contemporary Germany, leaving out large areas in Southern Germany including Bavaria, Baden and Württemberg. Moreover, I only have income data for 190 of the available 281 Prussian counties in the data base. The overall sample size is thus rather low. In 21% of those 190 counties, equitable inheritance is the most prevalent inheritance custom.

I measure contemporary incomes using data from the German Federal Statistical Office. Specifically, when preparing their tax forms, German citizens communicate their annual income to the relevant tax authorities. I gained access to these data—subject to strict privatization restrictions. I constructed a variable, monthly income, that divides the yearly income by 12. The data is summarized in Tables 2.6 to 2.7 in the Supplementary Information.

2.3.5   Income Inequality

My final outcome of interest—though analytically distinct from social equality—is income inequality. I was unable to obtain reliable historical data on income inequality. Moreover, measuring today's income inequality at the local level is similarly daunting task; in our case it requires data on the distribution of incomes at the municipality level. Most surveys that include information on personal income have too few observations to make statements about income distributions at the municipality level.[16] To bridge this problem, I was able to gain access to

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[16]To give an example, the 2002 German *Socio-Economic Panel* has about 19,000 observations. Given 8,500 German municipalities, this leaves two individuals per municipality, assuming that participants are distributed equally, which makes statements about distributions impossible.

data from all German tax records. The data was provided to us by the research data center of the German Federal Statistical Office. The office keeps records of roughly 50 million tax records. I collaborated with the office, providing them with appropriate statistical code in order to construct local GINI indexes at the municipality level. This unique source of data—heretofore untapped—is our primary measure for income inequality. I summarize the data in Tables 2.6 to 2.7 in the Supplementary Information.

2.4 Results

In this section, I estimate the association between inheritance customs and social equality. In a first step, I regress the respective measure of social equality on the equitable inheritance variable using the following benchmark specification:

$$Y_i = \alpha + x'_i\beta_1 + \beta_2 Equitable_i + \epsilon_i \tag{2.1}$$

where, Y_i represents the outcome of interest. The variable $Equitable_i$ is a dummy that takes on the value one if equitable inheritance is the dominant custom in municipality i, and zero otherwise. Finally, x_i is a vector of covariates presented in Table 2.2, and ϵ_i is an error term. In a second step, I then adopt a geographic matching procedure in order to establish more suitable counterfactuals.

2.4.1 Social Inequality

In a first step, I regress the share of women in local councils on the equitable treatment dummy. In Table 2.3, I show that equitable inheritance is associated with a rise in the share of women by roughly four percentage points (Model 1). The estimated effect is smaller when restricting the analysis to the five largest

parties (Model 2). The difference may be owing to the fact that gender norms are more standardized within large parties, particularly on the political left.

In a second step, I regress the share of members with non-German names in local councils on the equitable treatment dummy. Model 3 in Table 2.3 shows an estimated effect of roughly 1 percentage point, with a small accompanying standard error. The effect is, again, slightly smaller when restricting the analysis to the five major German parties, though it remains highly significant (Model 4).

In a third step, I regress the share of aristocrats in Rotary chapters on my treatment indicator (Model 5 in Table 2.3). This analysis is less precise, given that there are only roughly 600 chapters in Germany. In line with my theoretical expectation, I find that equitably inheriting communities see significantly fewer aristocrats in local elite clubs, like the Rotary. Specifically, I estimate that equitable inheritance customs are associated with a 1.4 percentage points reduction of Aristocrats in Rotary chapters, which is precisely estimated.

Taken together, the linear models presented in this section provide a first piece of evidence for my first hypothesis. Equitable inheritance is associated with an improved representation of women and minorities in local parliaments. At the same time, members of the nobility are less likely to be members of local Rotary clubs—an institution that prides itself as representing the social and economic elite.

Table 2.3: Equitable inheritance and social inequality

	Female representation (all parties) (1)	Female representation (5 largest parties) (2)	Migrant representation (all parties) (3)	Migrant representation (5 largest parties) (4)	Aristocratic representation in Rotary clubs (5)
Equitable inheritance	0.044 (0.006)	0.006 (0.006)	0.007 (0.001)	0.005 (0.001)	-0.014 (0.006)
Observations	3,944	3,950	3,909	3,909	600

Notes: OLS regressions of social inequality measures on the equitable inheritance dummy. Standard errors given in parentheses.

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Matching procedure

In a next step, I develop a new geographic matching procedure in order to create more suitable counterfactuals at the local level. As was highlighted above, the distribution of inheritance customs in Germany has the unique property that the spatial variation is pronounced even on small geographical levels. It is not uncommon that two neighboring municipalities historically followed opposite inheritance customs.

I use this fact to develop a new geographic discontinuity matching method, which, to my knowledge, has not been implemented before. Conceptually speaking, if two adjacent municipalities follow different inheritance customs, this can be thought of as both being close to the "cut-off" between equitable and inequitable inheritance. Comparing such municipalities invokes a crucial assumption, namely, independence of conditional outcomes and treatment given that two municipalities are adjacent:

$$Y(T = 1), Y(T = 0) \perp\!\!\!\perp T | A$$

Let A be a vector that includes longitude and latitude, so that adjacent municipalities get very similar values for A. The two adjacent municipalities are thus highly comparable, and treatment assignment can be considered "as-if random."[17] I implement this geographic discontinuity method by matching each treated (equitable) municipality to an untreated adjacent (inequitable) municipality. If there are multiple matches, I choose the match with the longest common border. I then

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[17]The precise matching procedure is as follows: Let i, j be two municipalities with treatment statuses $T(i), T(j)$. The function $A(i, j)$ takes on the value one if the two municipalities are adjacent, and zero otherwise. Finally, let $B(i, j)$ denote the length of the common border between municipalities. I match municipality i to j^* if all of the following two conditions hold: (1) $T(i) \neq T(j^*)$ (different treatment statuses) and (2) $A(i, j^*) = 1$ (municipalities adjacent). If this is the case for one i and multiple j, I match i to j^* if $j^* =_{j^*} B(i, j^*)$. The matching algorithm is greedy, i.e. I always choose the 'best' match for a given municipality i.

estimate the average differences between matched municipalities using a paired *t*-test.

In Figure 2.2, I revisit the results presented in Table 2.3. The adjacency matching procedure corroborates the evidence from the linear OLS models. Indeed, all estimated coefficients gain in strength and precision when using the local-level matching procedure. First, I confirm that municipalities that rely on equitable inheritance are significantly more likely to elect women into local councils. Female representation is two to three percentage points higher in equitably inheriting municipalities. The effect holds across both the five largest parties as well as all represented parties. I should remind the reader, however, that this analysis suffers from a loss of precision, given that the local council data covers only about 46% of all West German municipalities. Second, I confirm that equitably inheriting communities have a 0.4 to 0.5 percentage points higher representation of minorities in local councils. Again, the result holds within the five major parties as well as all represented parties. Finally, I repeat the matched analysis with the Rotary data. Since the data is skewed, I log-transform the aristocratic share in local chapters. I confirm that equitable inheritance is associated with about 30% fewer aristocrats in Rotary clubs. In sum, the fine-grained adjacency matching analysis confirms that equitable inheritance is consistently associated with a more socially equal society.

2.4.2 Mechanisms

Having provided evidence in favor of Tocqueville's hypothesis, I now turn to the two main hypothesized mechanisms: wealth equality and pro-egalitarian preferences.

The most immediate mechanism is wealth equality. Both the French and Amer-

Figure 2.2: Equitable inheritance and social inequality
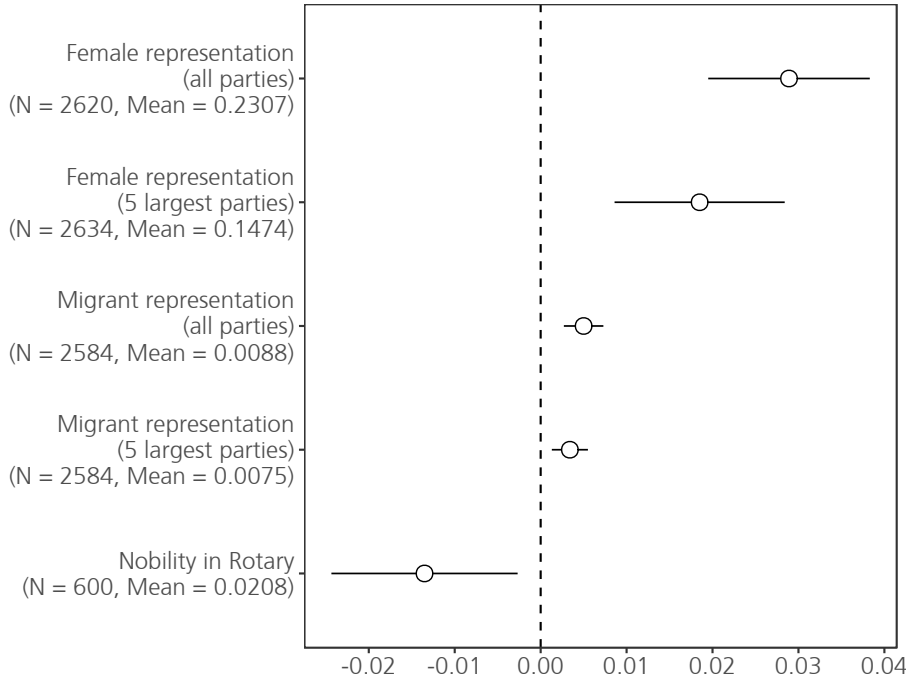
Notes: The Figure plots the difference (dot) between equitably and inequitably inheriting municipalities regarding the three social inequality headline outcomes. For the first four coefficients, the estimates are based on paired t-tests after the adjacency matching of municipalities as described in Section 2.4.1. The coefficient for the last outcome, nobility presence in Rotary clubs (log-transformed), is taken from an OLS regression. Here, the unit of observation is the Rotary chapter. The horizontal lines represent 95% confidence intervals.

ican revolutionaries hypothesized that equitable inheritance spurs social equality by reducing wealth inequality. In Figure 2.3, I provide evidence from a linear model, where I regress inequality in land ownership in 1895 on the equitable treatment dummy. The analysis, though it merely includes 295 units, showcases that equitably inheriting communities are associated with a drop in the land inequality Gini index by 0.8. The result thus shows that equitable inheritance is associated with lower levels of wealth inequality. I note that I am unable to repeat the geographic matching analysis given the low number of clusters.

A second less immediate downstream outcome, which plausibly reinforces the

Figure 2.3: Equitable inheritance and land inequality in 1895

Notes: The Figure plots the difference (dot) between equitably and inequitably inheriting municipalities regarding land inequality in the German Empire in 1895. Estimates are based on OLS regressions. The line represents the 95 percent confidence interval.

link from equitable inheritance to social equality, are pro-egalitarian preferences. As indicated above, I utilize three measures for pro-egalitarian preferences.

First, in Table 2.4 I report estimates from linear models regressing the vote share for the fascist NSDAP party for the decisive 1933 election on the treatment dummy. Model 1 shows that equitably inheriting communities were 2.5 percentage points less likely to vote for Hitler's fascist NSDAP party. The effect is precisely estimated, though the sample size is rather low. For that reason, I cannot repeat the analysis using the geographic matching design.

Second, in Table 2.4 I regress the vote share for the main German leftist parties in the four most recent Germany-wide elections. Here, too, I report strong treatment effects that range from 1.9 percentage points in the 2013 federal election (Model 3) to 6.5 percentage points in the most recent EU election (Model 5). I confirm these findings in Figure 2.4 using the aforementioned adjacency matching. The Figure shows that equitable inheritance is associated with a rise in the vote share of left-parties by up to two percent in the 2009 EU election. The only election for which the effect cannot be traced is the 2013 federal election.

Finally, in Figure 2.5 I measure pro-egalitarian preferences using data from the SOEP. The Figure shows a coefficient plot from a regression of the indicated outcomes depicting pro-egalitarian preferences on equitable inheritance customs.

Table 2.4: Equitable inheritance and left-parties' vote shares

	NSDAP % Federal 1933 (1)	Leftist % Federal 2009 (2)	Leftist % Federal 2013 (3)	Leftist % EU 2009 (4)	Leftist % EU 2014 (5)
Equitable	−0.025 (0.013)	0.033 (0.004)	0.019 (0.004)	0.066 (0.005)	0.065 (0.005)
Observations	605	8,262	8,360	8,262	8,340

Notes: OLS regression of the indicated vote share on equitable inheritance.

Standard errors are given in parentheses.

Figure 2.4: Equitable inheritance and left-parties' vote share (matching)

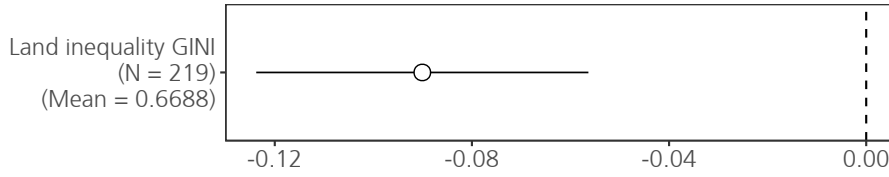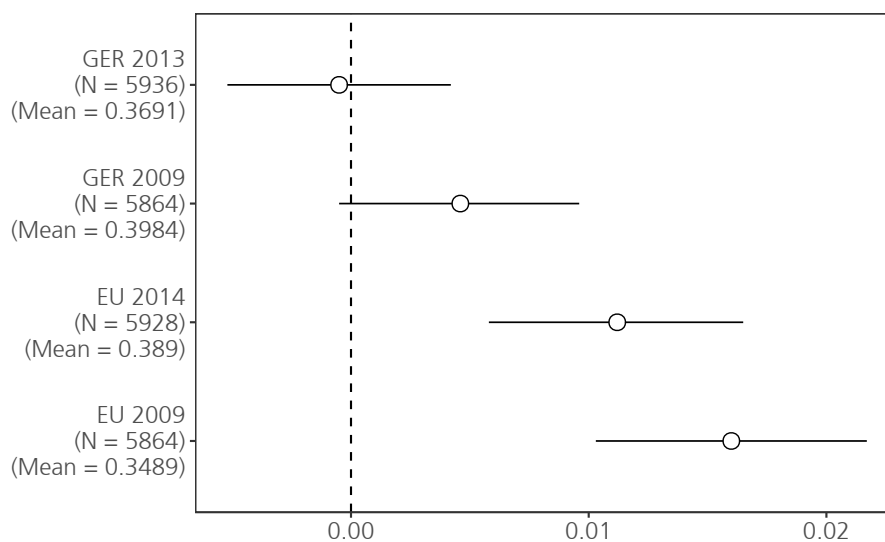*Notes:* The Figure plots the difference (dot) between equitably and inequitably inheriting municipalities regarding the leftist-parties' vote shares based on a paired t-test after adjacency matching of municipalities. The line represents the 95 percent confidence interval.

Figure 2.5: Equitable inheritance and pro-egalitarian preferences (regression)
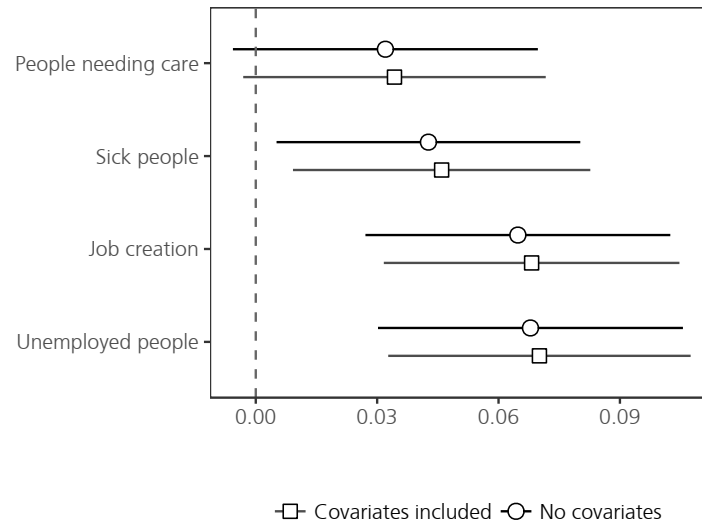
To ease interpretation, I first reverse the scale of the equality items, and then standardize them. The Figure shows that pro-egalitarian preferences are consistently more pronounced in municipalities that adopt equitable inheritance. Estimates range between 0.03 and 0.07 standard deviations. This final piece of evidence thus points to pro-egalitarian preferences as a plausible mechanism. Its theoretical direction is clear, and empirical backing is solid—though effect sizes are rather small.

2.5 Income and Income Inequality

Thus far, I have largely confirmed Tocqueville's hypothesis. Equitable inheritance is associated with more socially equal municipalities. Two plausible mechanisms—

wealth equality and pro-egalitarian preferences—were also shown to be positively associated with equitable inheritance. The hypothesized association between equitable inheritance, income and income inequality, however, was less clear. In this section, I therefore assess the association between equitable inheritance and incomes, using historical and contemporary data. Thereafter, I turn to income inequality.

In a first step, I assess whether equitably inheriting communities had higher per capita incomes in the 19th century. To do so, I rely on data from the Prussian census. In Figure 2.6, I show that equitably inheriting communities are associated with higher wages in 1892 as well as 1902. The positive estimates are detectible for both urban as well as rural wages. The confidence intervals, however, are wide due to the low number of clusters. For this reason, I am also unable to conduct local adjacency matching. Still, the evidence is in line with my third hypothesis, namely, that equitable inheritance leads to higher incomes.

Second, I assess whether equitable inheritance customs are associated with greater per capita income in 2014. In Figure 2.7, I show that equitable inheritance customs are associated with an increase in the logarithm of income, based on a paired t-test after adjacency matching of municipalities. Specifically, equitable inheritance is associated with a rise in mean incomes by 3 percent and a rise in median income by 1 percent.[18] As such, the analysis corroborates that income is likely positively affected by equitable inheritance customs, though the theoretical expectations were shown to be ambivalent.

Having established a positive historical and contemporary association between

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[18]In the Supplementary Information, I test the robustness of this finding using data from the Socioeconomic Panel (Table 2.8). Here, too, equitable inheritance predicts income positively. These data are, of course, less reliable as they rely on survey evidence, not administrative tax records. The analysis is worthwhile, however, as it addresses potential concerns about an ecological fallacy, given that I measure income at the municipality-level though the treatment operates on a family-level.

Figure 2.6: Equitable inheritance and wages in Prussia
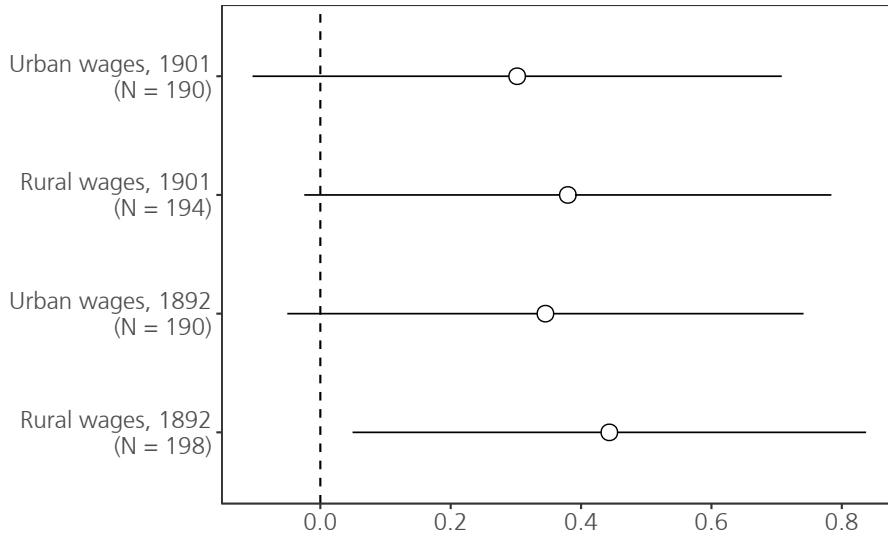
Notes: The Figure plots the difference (dot) between equitably and inequitably inheriting munic-
ipalities regarding wages in Prussia in 1892 and 1901. Estimates are based on OLS regressions,
controlling for longitude and latitude. Outcomes are standardized. The line represents the 95
percent confidence interval.

Figure 2.7: Equitable inheritance, absolute income and income inequality
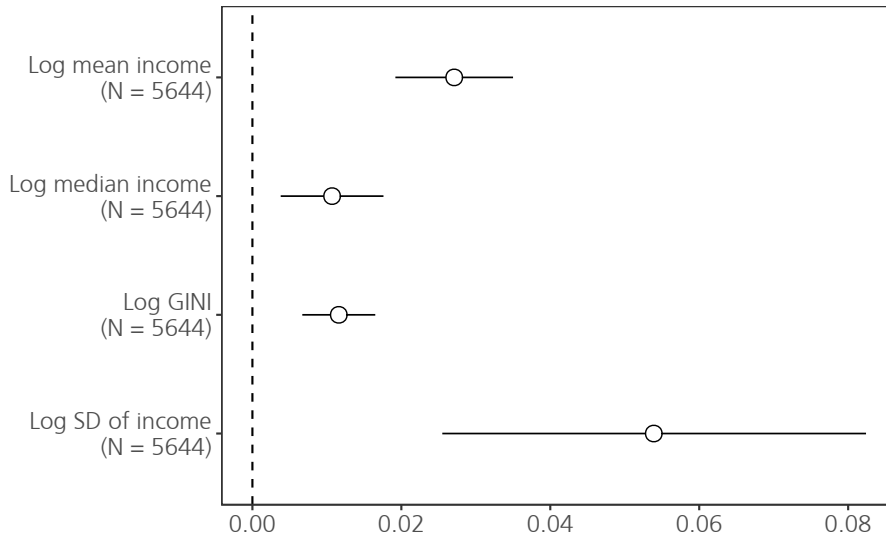
Notes: The Figure plots the difference (dot) between equitably and inequitably inheriting munic-
ipalities regarding absolute income and income inequality based on a paired t-test after adjacency
matching of municipalities. The line represents the 95 percent confidence interval. Coefficients
can be interpreted as a percentage change.

equitable inheritance and income, I turn to my final outcome of interest: income inequality. The French and American revolutionaries did not specifically expect income equality to rise as a result of equitable inheritance. On the contrary, if equitable inheritance ensures that the most able—not the firstborn son—advances, incomes and income inequality may well rise.

In Figure 2.7, I assess whether equitable inheritance is associated with greater income inequality in 2014 (data on historical income inequality is, to my knowledge, not available). Specifically, I use the aforementioned adjacency matching between equitably and inequitably inheriting communities, and conduct paired t-tests. The Figure shows that equitable inheritance is associated with an *increase* in inequality by roughly one percent (GINI) to six percent (log of SD). This marks a substantively meaningful estimate. It also underlines that social and economic equality are two distinct dimensions of equality. Indeed, the evidence implies that while equitable inheritance levels the societal playing field, it does not level incomes. The most likely explanation, in the words of de Visme Williamson (1976, 102), is that equitable inheritance leaves "inequalities of ability and work," which may in turn spur unequal incomes.

2.6   Conclusion

This chapter has revisited a century-old hypothesis about the determinants of inequality in Western societies. Namely, that social equality is a product of equitable inheritance customs. Exploiting fine-grained variation in inheritance customs in West Germany, I found that equitably inheriting municipalities have more women and ethnic minorities in local councils and fewer aristocrats in elite Rotary clubs. I assessed two mechanisms that may help explain this finding: Equitable inher-

itance may lower wealth inequality and foster pro-egalitarian preferences. Using historical as well as contemporary data, I confirmed that equitably inheriting communities had lower levels of land inequality in 1895 and more left-leaning political preferences in 1933 as well as in recent years. In a final step, I explored the effects of inheritance customs on incomes and income inequality. Here, I found that equitably inheriting municipalities have higher incomes as well as income inequality. As such, the overall effects of equitable inheritance can be interpreted as leveling the playing field by rewarding talent not hereditary status.

Three findings are particularly noteworthy. First, I demonstrated equitable inheritance to be associated with greater incomes and income inequality. The precise mechanism underlying this link, however, was beyond the scope of this study. I have alluded to two potential logics: Equitable inheritance may lower labor supervision costs as well as population growth. My own preliminary analyses (see Appendix Figure 2.11) show fertility to be *higher* in equitably inheriting municipalities. But, historical data is necessary to firmly rule out this link and to solidify the true causal relationship.

Second, my chapter has pointed out a novel determinant for pro-egalitarian preferences, and, by extrapolation, redistribution. This link adds to a literature on preferences for distribution (e.g., Alesina and Fuchs-Schündeln, 2007). Broadly speaking, I know too little about the determinants of preferences, particularly the impact of long established institutions such as inheritance. How inheritance customs shape pro-egalitarian preferences, again, was beyond the scope of this chapter. One hypothesis, cited above, is that it affects the structure of families, instilling in people a belief that equality is desirable. Just as important, however, inequitable inheritance customs could foster a desire to make society more equal. Future studies could help parse out with greater clarity how inheritance affects

preferences for equality, for instance, by using survey experiments that expose individuals to different scenarios of inheritance.

Last, however, I should add a skeptical remark. All estimates in this chapter were "statistically significant." But, are they substantively significant? I estimated the effect of inheritance on the representation of women and minorities to be between 1 and 3 percentage points. The effect on the mechanisms—land inequality and pro-egalitarian preferences—was more pronounced. The mere fact that estimates are significant—in a sample of over 8,000 observations—tells us little about substantive significance. It is here that qualitative studies that delve more deeply into the societal effects of inheritance customs could provide a valuable contribution. There can be little doubt that inheritance customs had stark effects on social outcomes throughout history. But just how politicized inheritance customs are today remains unanswered and is worthy of further scholarly attention.

2.7   Supplementary Information

Table 2.5: Descriptive statistics of electoral outcomes

Party	Election	Sample			Partible			Mixed (partible)			Mixed (impartible)			Impartible			Forest		
		N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
SPD	EU 2014	8340	25.8	10	2732	28.5	9	2979	28.7	9.4	6084	26.5	10.1	6483	25	10	7213	25.4	10
SPD	GER 2013	8360	24.6	8.7	2730	25.2	7.6	2978	25.7	8.1	6097	25	8.8	6505	24.3	8.8	7234	24.4	8.7
SPD	GER 2009	8262	21.5	8	2705	22.2	7	2963	22.6	7.4	6043	21.9	8.1	6418	21.2	8.1	7144	21.3	8
SPD	EU 2009	8262	20.3	10.1	2705	23.1	9.3	2962	23.2	9.6	6043	21	10.3	6417	19.2	10	7144	19.8	10
CDU	EU 2014	8340	43.3	11	2732	41	10.7	2979	40.7	10.8	6084	42.6	11	6483	43.9	10.8	7213	43.5	10.8
CDU	GER 2013	8360	48.1	9.8	2730	46.5	9.2	2978	46.1	9.2	6097	47.5	9.7	6505	48.5	9.6	7234	48.3	9.6
CDU	GER 2009	8262	38.9	9.5	2705	36.3	8.5	2963	36.1	8.7	6043	38.3	9.4	6418	39.8	9.3	7144	39.4	9.3
CDU	EU 2009	8262	45.1	11.7	2705	41.7	10.8	2962	41.6	11	6043	44.2	11.7	6417	46.3	11.3	7144	45.6	11.5
Green	EU 2014	8340	7.9	4.2	2732	8	3.8	2979	8.2	3.8	6084	8.1	4	6483	8	4.2	7213	7.9	4.2
Green	GER 2013	8360	7.1	3	2730	7	2.8	2978	7.1	2.9	6097	7.1	2.9	6505	7.3	2.9	7234	7.2	2.9
Green	GER 2009	8262	9.5	3.7	2705	9.3	3.6	2963	9.5	3.7	6043	9.4	3.6	6418	9.6	3.7	7144	9.5	3.7
Green	EU 2009	8262	9.5	4.6	2705	9.4	4.2	2962	9.6	4.3	6043	9.5	4.4	6417	9.7	4.6	7144	9.6	4.6
Left Party	EU 2014	8340	3.1	1.8	2732	3.4	1.9	2979	3.5	1.9	6084	3.2	1.8	6483	3	1.7	7213	3.1	1.7
Left Party	GER 2013	8360	4.4	2.1	2730	5.1	2.1	2978	5	2.2	6097	4.5	2.1	6505	4.2	1.9	7234	4.3	1.9
Left Party	GER 2009	8262	7.7	3.5	2705	8.8	4	2963	8.7	3.8	6043	7.9	3.5	6418	7.2	3	7144	7.5	3.2
Left Party	EU 2009	8262	3	2	2705	3.4	2.3	2962	3.4	2.1	6043	3.1	2	6417	2.8	1.7	7144	2.9	1.9
FDP	EU 2014	8338	4	3.6	2732	3.3	1.9	2979	3.4	2.3	6084	3.7	3.1	6481	4.1	3.9	7211	4	3.7
FDP	GER 2013	8360	5	2.2	2730	5.3	2.2	2978	5.3	2.6	6097	5	2.3	6505	4.8	2.1	7234	4.9	2
FDP	GER 2009	8262	16.2	4.3	2705	17.6	4.3	2963	17.2	4.6	6043	16.3	4.3	6418	15.7	3.9	7144	16	4.1
FDP	EU 2009	8262	10.9	4.1	2705	12	4	2962	11.9	4.2	6043	11	4.1	6417	10.6	3.9	7144	10.8	4
Turnout	EU 2014	8340	43.7	9.1	2732	42.8	9.4	2979	43.5	9.5	6084	43.8	9.1	6483	43.7	8.9	7213	43.6	8.8
Turnout	GER 2013	8361	65.4	8.2	2730	61.2	8.4	2978	62.4	8.6	6097	65.2	8.4	6506	66.8	7.9	7235	66	8.1
Turnout	GER 2009	8262	66	8.3	2705	61.6	8.4	2963	62.8	8.8	6043	65.7	8.5	6418	67.4	7.9	7144	66.6	8.1
Turnout	EU 2009	8262	43.6	8.7	2705	45.2	8.9	2962	45.1	9.2	6043	43.8	8.7	6417	42.7	8.3	7144	43.2	8.5
Left	EU 2014	8340	36.8	11.1	2732	39.9	9.9	2979	40.3	10.3	6084	37.7	11.3	6483	36	11.2	7213	36.4	11.1
Right	EU 2014	8338	47.2	10.9	2732	44.3	10.7	2979	44.1	10.8	6084	46.3	11	6481	48	10.6	7211	47.6	10.7
Left	GER 2013	8360	36.1	10.3	2730	37.2	8.9	2978	37.7	9.3	6097	36.6	10.3	6505	35.8	10.5	7234	35.9	10.3
Right	GER 2013	8360	53	9.8	2730	51.8	9.5	2978	51.4	9.6	6097	52.5	9.8	6505	53.3	9.6	7234	53.2	9.6
Left	GER 2009	8262	38.6	10.5	2705	40.3	9.4	2963	40.8	9.6	6043	39.2	10.4	6418	38.1	10.5	7144	38.3	10.4
Right	GER 2009	8262	55.1	9.9	2705	53.9	9.7	2963	53.3	9.6	6043	54.5	9.8	6418	55.5	9.6	7144	55.4	9.6
Left	EU 2009	8262	32.8	12.2	2705	35.9	10.6	2962	36.1	10.9	6043	33.5	12.2	6417	31.8	12.3	7144	32.3	12.2
Right	EU 2009	8262	56	11	2705	53.7	10.9	2962	53.4	10.9	6043	55.3	11.1	6417	56.8	10.5	7144	56.4	10.7

Table 2.6: Descriptive statistics of income data (part 1)

	Sample			Partible			Mixed (partible)		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
GINI	0.48	0.05	8,025	0.47	0.05	1,387	0.47	0.05	1,020
SD of income	45,143.82	80,613.13	8,029	48,837.84	147,457.60	1,389	40,619.22	51,442.65	1,022
Mean of income	26,263.32	4,592.05	8,029	26,726.62	4,393.34	1,389	25,839.24	4,583.81	1,022
Median of income	20,287.94	2,393.45	8,029	20,742.09	2,278.00	1,389	20,059.72	2,745.49	1,022

Table 2.7: Descriptive statistics of income data (part 2)

	Mixed (impartible)			Impartible			Forest		
	Mean	SD	N	Mean	SD	N	Mean	SD	N
GINI	0.47	0.05	1,153	0.48	0.04	4,206	0.46	0.05	259
SD of income	39,380.95	43,488.66	1,153	47,273.96	62,933.94	4,206	34,249.32	35,554.12	259
Mean of income	25,831.69	4,368.07	1,153	26,422.31	4,706.52	4,206	24,791.58	4,200.43	259
Median of income	20,105.46	2,600.54	1,153	20,270.56	2,228.58	4,206	19,847.52	2,774.14	259

Table 2.8: Equitable inheritance and SOEP household income

| | Log monthly household income | | | |
	(1)	(2)	(3)	(4)
Equitable	0.038	0.029	0.076	0.045
	(0.010)	(0.010)	(0.014)	(0.013)
Male		0.018		0.018
		(0.009)		(0.009)
Age		0.001		0.001
		(0.000)		(0.000)
Unmarried		-0.308		-0.307
		(0.010)		(0.010)
Student		0.032		0.031
		(0.029)		(0.029)
Retired		-0.386		-0.384
		(0.018)		(0.018)
Unemployed		-0.456		-0.451
		(0.022)		(0.022)
Years of education		0.068		0.069
		(0.002)		(0.002)
State FEs	No	No	Yes	Yes
Observations	17,758	13,573	17,758	13,573

Notes: Coefficient of equitable refers to change in log monthly household income associated with switching from equitable to inequitable inheritance. Data source is SOEP 2002. Standard errors in parentheses.

Figure 2.8: Map of German peasant wars
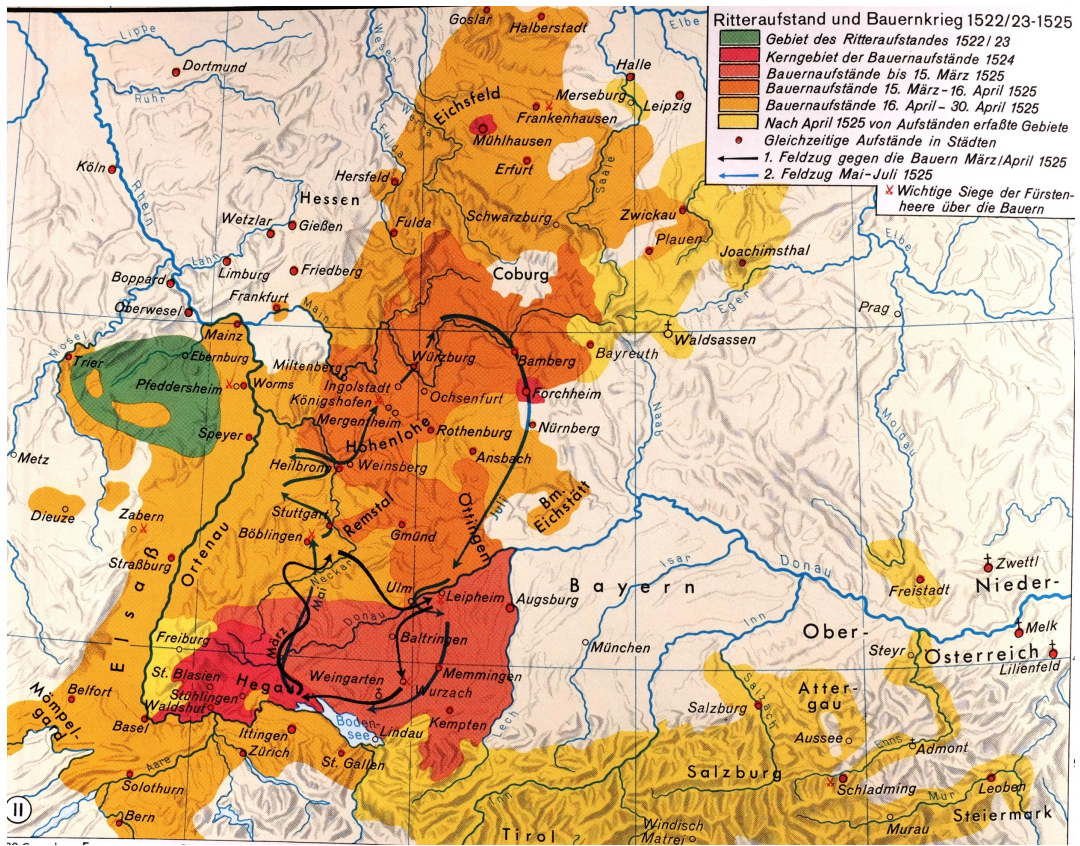
Notes: German peasant wars (c. 1522-1525), taken from Putzger (1970)

Figure 2.9: Map of German soil suitability
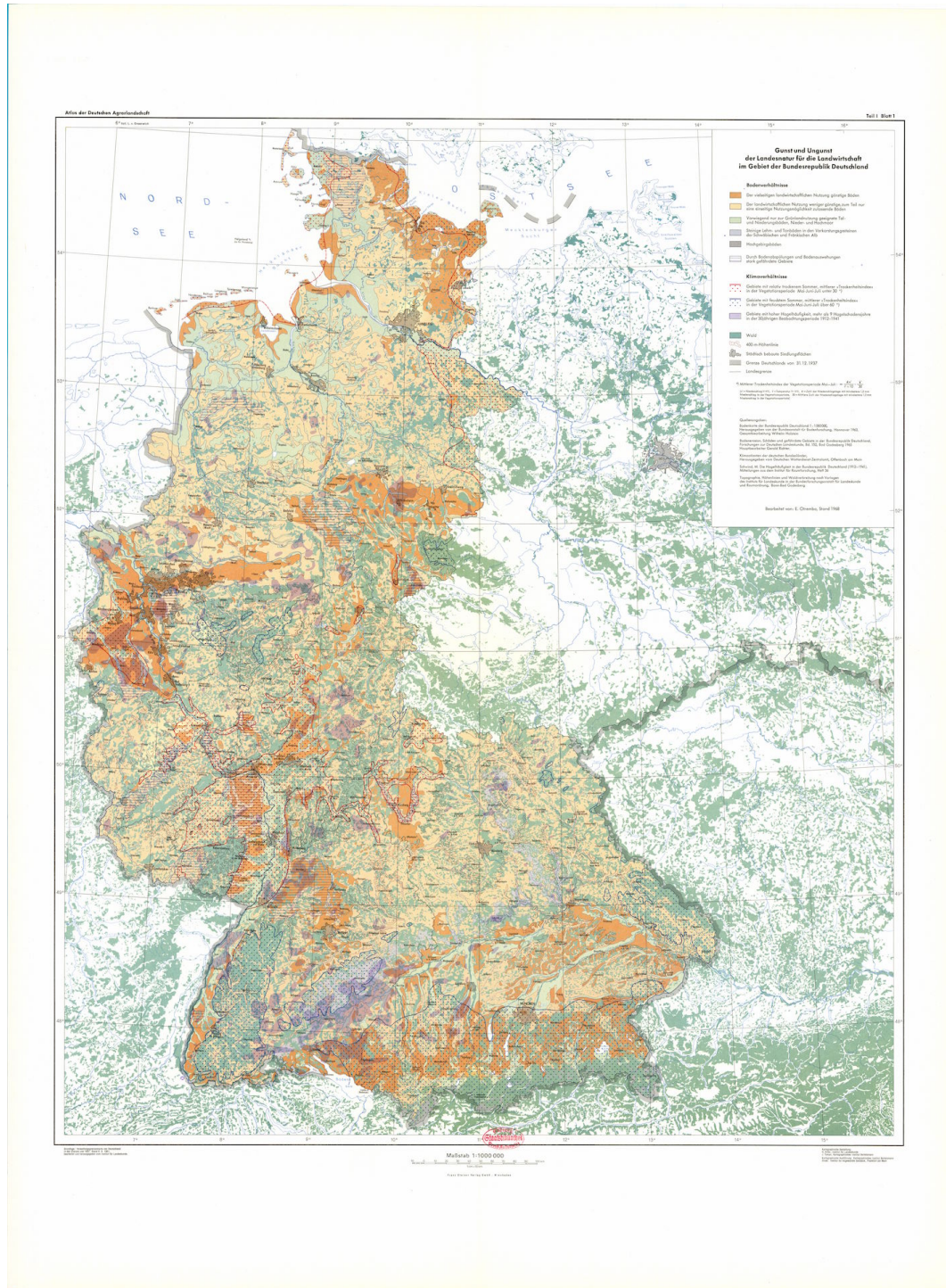
Notes: Taken from *Atlas der Deutschen Agrarlandschaft*)

Figure 2.10: Map of peasant wars by county
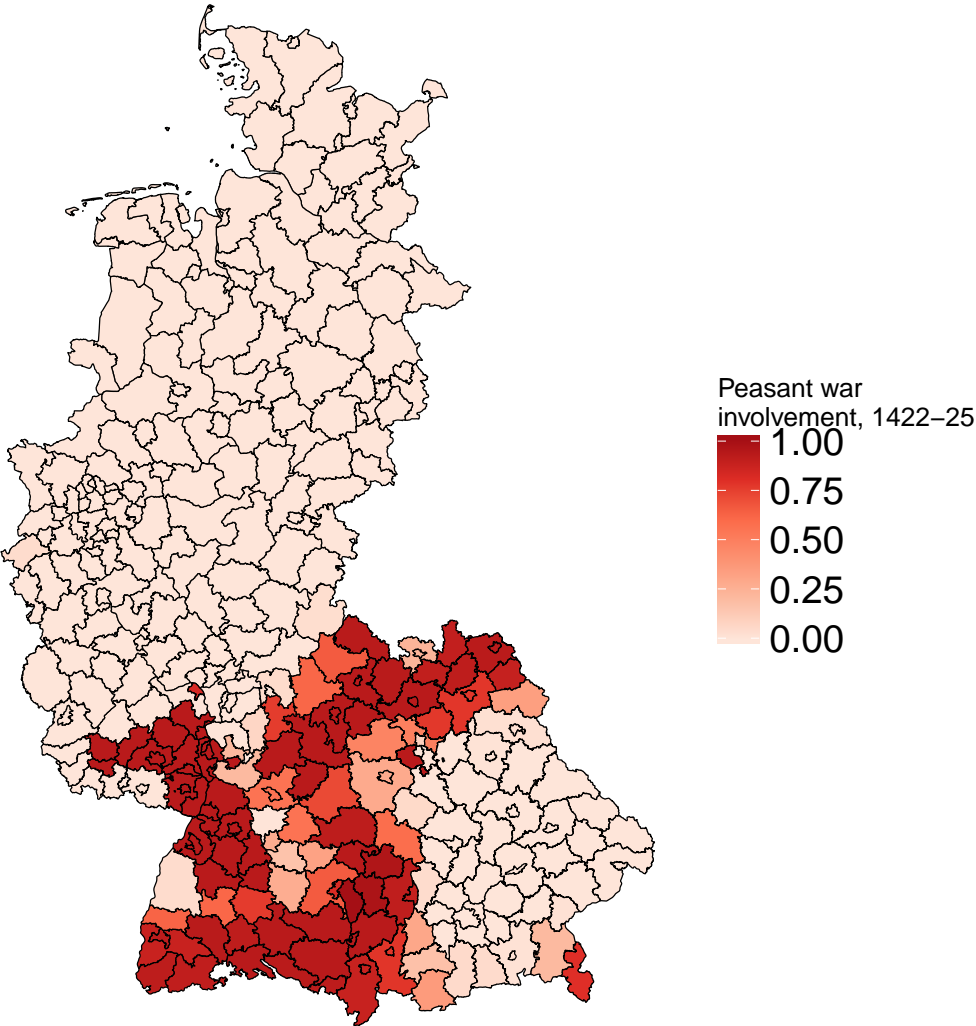
Notes: Peasant war involvement by county. Digitized map based on Putzger (1970). See also Figure 2.8.

Figure 2.11: Equitable inheritance and fertility rates (matching)
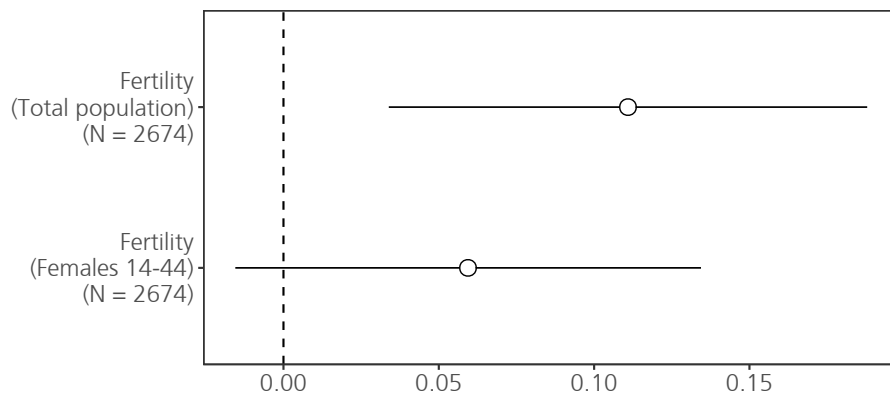
Notes: The Figure shows the difference (dot) between equitably and inequitably inheriting municipalities regarding fertility rates after adjacency matching of municipalities. The first outcome is the *crude fertility rate*, i.e. the total number of newborns divided by total municipality population. The second outcome is the *general fertility rate*, i.e. the total number of newborns divided by the number of women that are of reproductive age (aged 14–44). Effect sizes are changes in standard deviations.

Figure 2.12: Survey instrument of Röhm.

3

Missionaries and Community Cohesion

3.1 Introduction

Religious beliefs and practices have long been argued to affect community cohesion (Putnam, Leonardi and Nanetti, 1993; Wuthnow, 2002). Protestantism, in particular, has been found to correlate positively with generalized trust (Guiso, Sapienza and Zingales, 2003; Inglehart, 1999; Veenstra, 2002) and cooperation (Anderson and Mellor, 2009; Silva and Mace, 2014; Fernández, 2014). In the context of developing countries, scholars have repeatedly pointed out the role of Protestant missionaries in spurring outcomes pertaining to community cohesion. Lankina and Getachew (2011, 475), discussing Protestant missions in India, write that "missionary organizational activity [...] contributed to the development of civil society." And Woodberry (2012, 253) notes that Protestant missionaries "instill[ed] voluntarism and charity in their congregants to survive," given that they were unable to tax their members.

A different set of scholars, however, points out that religious groups, including Protestant missions, have a tendency to disintegrate, and subsequently divide communities along religious lines (Zaleski and Zech, 1995). Historical evidence is ample. It ranges from the Great Schism to the French Wars of Religion (Lewis and Lewis, 2009). The logic behind such religious splintering is simple: Because religious groups offer excludable goods, religious entrepreneurs gain from founding new churches (Berman, 2000). The tendency to split apart is particularly pronounced among Protestants, who "tend toward separation and independence" (Shah and Woodberry, 2004, 48). As a result, argues Schwadel (2005, 159), churches "limit network heterogeneity and thus limit the opportunity for bridging social capital."

How does one make sense of these conflicting accounts? In this chapter, I revisit the relation between Protestant missions and community cohesion. I define community cohesion as levels of trust, cooperation and identification in a local community (Chan, To and Chan, 2006). I add to the current research agenda by focusing on modern-day Protestant missions, and by exploring causal mechanisms. Specifically, I observationally explore to what degree the two indicated mechanisms—pro-social preferences and social networks—may mediate the relation between Protestant missions and community cohesion. I do so through the prism of an in-depth study of 16 villages in southeastern Peru, which were exposed to different Protestant missions.

Combining qualitative interviews and a review of the theoretical literature, I point out competing logics about Protestant missionaries' effects on pro-social preferences, social networks and community cohesion. On the one hand, I note that Protestant missionaries' focus on charity and forgiveness plausibly strengthens pro-social preferences. They may also exhibit positive effects on social networks given

that they establish regular venues of social interaction such as church services and reading groups. On the other hand, Protestant missionaries may plausibly weaken pro-social preferences given that they tend to shift converts' attention toward individual salvation and the after-life. Moreover, they may also weaken social networks by dividing villages along newly established religious lines. Taken together, the main effect of Protestant missionaries on community cohesion is hence unclear.

To empirically test these conflicting logics, I exploit exogenous variation in missionary activity in Peru's Cusco region. Activities of the guerrilla movement Shining Path (*Sendero Luminoso*) meant that the valleys under study were off limits to missionaries throughout the 1980s. The defeat of the rebels in the early 1990s led to a sudden rise in Protestant missionary activity in the region. Importantly, the proselytism was haphazard. Church planting by competing missionary groups followed no discernible strategy. As a consequence, villages were near-exogenously exposed to missionaries. Using detailed census data from 1993, geographic variables and qualitative interviews with history experts, I buttress this exogeneity assumption and show that villages are statistically indistinguishable but for their exposure to the missionaries.

The evangelized villages were exposed to two different kinds of missionary groups: mainline Evangelicals and Pentecostals. My comparison group are villages that were not evangelized and remained nominally Catholic. Importantly, the two groups differentially activated both hypothesized mechanisms. Mainline Evangelical missions arguably had a greater impact on social networks due to their broad proselytism strategy. And, Pentecostal missions arguably exhibited more pronounced effects on pro-social preferences, given their distinct focus on salvation. The differential activation of the two mediators thus affords a more credible

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mediation analysis.

Using lab-in-the-field experiments and survey evidence, I find that villages exposed to Protestant missions have lower levels pro-social preferences and sparser social networks. I also find that community cohesion is less pronounced in these villages. When moving to a causal mediation framework, I estimate that the aggregate treatment effect is predominantly mediated through the network mechanism. The pro-social preference mechanism plays a negligible role. As such, the estimates support a logic whereby Protestant missions reduce community cohesion by dividing social networks and, to a lesser degree, by lowering pro-social preferences. Interestingly, using a priming experiment, I find that the reduction in community cohesion is equally present within the respective missionary communities. The finding calls the "religion as a club good"-hypothesis into question (e.g., Berman and Laitin, 2008), which would predict that Protestant missions merely shift cohesion from the village to the church community.

This chapter adds to a literature on religious organizations in comparative politics in three ways. First, relying on fine-grained local-level qualitative evidence, I present novel and rivaling logics about the effects of Protestant missions on community cohesion so as to strengthen the theoretical literature. Second, to my knowledge, the chapter is the first to provide causally identified evidence regarding the effects of Protestant missions on community cohesion. Third, my analysis adds to a broader academic literature that assesses the determinants of community cohesion. Recent empirical studies have found community cohesion to correlate with economic development (Casey, Glennerster and Miguel 2012; Avdeenko and Gilligan 2015, 428), governance (Mansuri and Rao, 2012) and lower wartime violence (Cohen 2013, 464). In another vein, the findings may also contribute to a debate whether governments and local communities should grant access to Christian

evangelists (Grant, 2001). In recent years, several governments have denied access to Protestant missionaries. The Chinese government, for example, has forced hundreds of South Korean missionaries out of the country on the grounds that they disturb the public order (Vanderklippe, 2014).

3.2 Protestant Missionaries and Community Cohesion

The relation between Protestantism and community cohesion (as well as social capital) is a recurring topic in the social sciences (e.g., Putnam, Leonardi and Nanetti, 1993; Wuthnow, 2002; Guiso, Sapienza and Zingales, 2003; Inglehart, 1999; Veenstra, 2002; Silva and Mace, 2014; Fernández, 2014). In developing countries, a number of authors have pointed to Protestant missionaries as a likely source of variation in community cohesion (Woodberry, 2012; Lankina and Getachew, 2011). Focusing on Latin America, Bot (1999, 165) pointedly asks: "Should the upsurge of Protestantism be seen as a community movement with a religious dimension, or rather as an 'anti-social-movement', the expression of sectarian, inward-looking behaviour"? Before presenting different logics that may explain associations between Protestant missions and community cohesion, I first lay out my definition of community cohesion.

3.2.1 Definition

This chapter's main outcome of interest is community cohesion. I follow Chan, To and Chan (2006) and adopt a definition of community cohesion that is minimal in scope, and close to ordinary usage. In particular, I define community cohesion as *levels of trust, cooperation and identification in a local community.* This defini-

tion reflects core elements adopted in several influential studies. Notably, Fearon, Humphreys and Weinstein (2009, 288) measure community cohesion by "assessing levels of trust, patterns of community activity, and the extent of associational life." Gilligan, Pasquale and Samii (2014, 609) focus on four components, namely "(1) altruism, (2) a sense of obligation to contribute to ones community welfare, in particular to contribute to the community's collective good, (3) trust in one's community members, and (4) trustworthiness with one's community members."

To what degree, then, do Protestant missionaries affect community cohesion? And what mechanisms give rise to any observed correlation? In the following, I lay out my theoretical arguments. Based on three months of qualitative field work and a review of the scholarly literature, I argue that two mechanisms are particularly pivotal in mediating a potential relationship between Protestant missions and community cohesion. The first mediator, which I label the *preference* mechanism, captures the effects Protestant missions have on pro-social preferences. The second mediator, which I label the *structure* mechanism, captures the missionary impact on local-level networks. The theoretical arguments are condensed in the graph in Figure 3.1. For both mediators, I uncover two competing logics that may help explain why community cohesion could fall or rise as a result of missionary activity.

3.2.2 Preference Mechanism: Pro-Sociality

The first mechanism by which Protestant missions may affect community cohesion are pro-social preferences. Pro-social preferences—preferences over another individual's material payoffs—may drive community cohesion for a number of reasons. For one, taking an interest in the material benefit of others is arguably a necessary condition for individual-level identification with a greater community (Frey and Meier, 2004*a*). Pro-social preferences also likely spur trust in fellow community

members (Simpson and Willer, 2015). Similarly, pro-social preferences may induce individuals to cooperate as well as to forgive when such cooperation is not reciprocated (Frey and Meier, 2004*b*). There is compelling experimental evidence that religious organizations have profound effects on individual preferences (Wulff, 1991; Jelen, 1993; McKenzie and Rouse, 2013; McClendon and Riedl, 2015; Grossman, 2015). How, then, may Protestant missions affect pro-social preferences?

Figure 3.1: Conceptual model
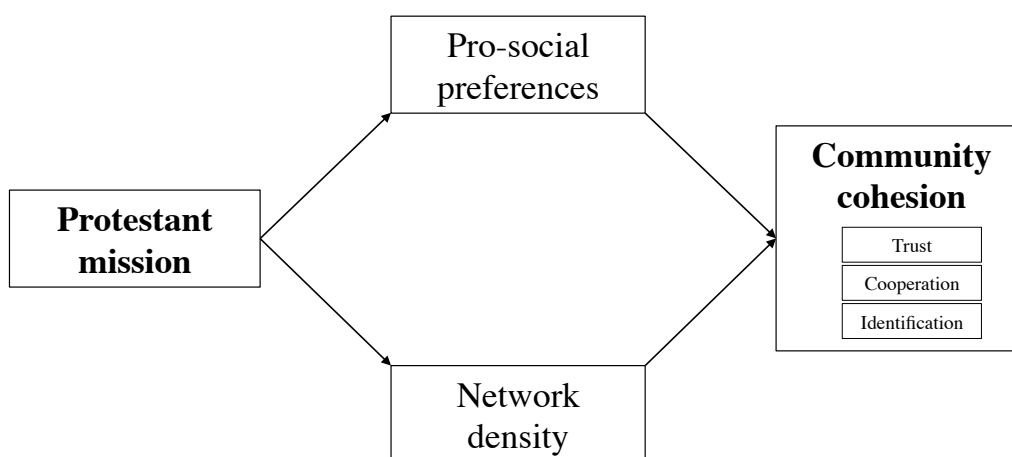
Charity. A core preaching of Protestant missions is that converts must do good unto others. Social scientific observational and experimental studies have linked pro-sociality among Christian believers to the belief in a God who demands "charity" and punishes deviant behavior (Saroglou et al., 2005; Tan, 2006; Purzycki et al., 2016). While systematic evidence from missions is sparse, historical accounts regularly display Protestant missions as strong advocates of pro-social preferences. As Little (2005, 207-208) notes, the "Protestant missionary ethos originally focused on the glory of God [and] [...] charity." Personal accounts from Protestant missions highlight the extent to which such charity is propagated by churches on the ground. The missionary S. Joseph Kidder, for example, writes that missionaries "need to come to the point of loving as Jesus loves; he commanded us to even love our

enemies" (2012, 125). Some authors have noted that the missionary propagation of charity may be a product of missionary self-interest. Woodberry (2012, 253), for instance, writes: "(b)ecause they do not have the ability to tax their members, nonstate religious groups had to instill voluntarism and charity in their congregants to survive."

Accounts from Latin America confirm this picture. Olson (2006, 897) notes that Protestant missions propagate "clear guidelines for behavior, including [...] [a] strong community." The author continues that such values "have been identified as characteristics of Evangelical movements [across] Latin America". In a study of Protestantism in rural Colombia, Brusco (2011, 128) notes that Protestant communities place strong emphasis on Biblical teachings, including forgiveness and charity. The author attributes this to Evangelicals' focus on Biblical teachings. He writes: "[a]mong evangelicals, then, the "priesthood of all believers" exists not only on a doctrinal level but also is put into extensive practice and establishes an ethnic of reading, contemplating and analysis."

In the communities studied in this chapter, I visited numerous church services in addition to conducting semi-structured interviews with missionaries and converts. Sermons frequently included reference to forgiveness and the need to "do good unto others." Ministers kept reminding fellow believers of the importance to put the community first. One minister said his prime social teaching was to abstain from "antisocial behavior such as drinking, stealing and all the things that are bad for the community." Such behavior, another minister argued, "is demanded from good Christians in order to make it to heaven." Taking these accounts together, Protestant missionaries' focus on charity may thus increase pro-social preferences. I formulate my first hypothesis accordingly.

- Hypothesis 1a: Protestant missions increase pro-social preferences in villages

where they evangelize as compared to villages where they do not evangelize.

Individualism. Protestantism, however, is also frequently described as a religion that prizes the individual. Widely known is Max Weber's thesis about the relationship between Protestantism and the entrepreneurial, individualistic spirit of capitalism (Weber, 2002). Protestant missions propel this spirit on the ground. Shah and Woodberry (2004, 48) note that Protestant missionaries believe that "people can acquire saving faith only as they personally and individually appropriate God's word." Reuschling attributes this individualism to a strong "experiential component in how Evangelicalism is defined with regard to one's personal acceptance of [...] Jesus Christ" (2005, 61). Similarly, Woodberry (2012, 249) writes that Protestants "expected lay people to make their own religious choices. They believed people are saved not through sacraments or group membership but by 'true faith in God'." The Protestant emphasis of individual liberation—traditionally defined as "the liberation of the individual believer from a mass of institutional controls and traditional restraints" (Wolin, 1957, 428)—is particularly pronounced and surprisingly homogenously advocated within Pentecostal denominations. McClendon and Riedl (2016), for instance, analyze a random sample of Pentecostal churches in Nairobi, Kenya, and demonstrate that "Pentecostal and Charismatic churches are leading their members to prioritize the individual."

In Latin America, several commentator have confirmed Protestantism's emphasis on individualism. In one anthropological study of conversion processes toward Protestantism among Mayas in Guatemala, Goldin and Metz (1991, 34) trace out a distinct emphasis on "individualism and the bettering of oneself by relying on one's own efforts and hard work." Bot (1999), too, notes that popular Protestantism in Latin America leads to a "new approach to religion [that] take[s] place at the level of the individual." She further writes that Protestantism

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is accompanied by "a radical transcendence: God is apart from the world, not immanent to it, and salvation is only to be found by renouncing the world." Gill (1993, 182) attributes this individualism to the increasingly atomized realities of modern life: "Given the growing atomization of social life, it should come as no surprise that the ideology of many new religious organizations, particularly those espousing Protestant fundamentalism, emphasizes individualism and self-sacrifice as a means of personal improvement." Other commentators have gone as far as labeling Latin American Protestants "apolitical conservatives who leave the injustices of the world to the Lord's care" (Ireland, 1993, 4). Perhaps as a result, argues Gill (2001, 129), "Latin American Protestantism has not shown the political activism of other fundamentalisms."

In the communities under study a similar picture emerged. I frequently witnessed ministers making references to individual salvation. The primacy of hard work—coupled with abstinence from social gatherings—was a recurring theme. All this based on the idea that "salvation lies within," as one minister said. One village president in an evangelized village lamented "some of the religious groups advocate against villagers speaking with one another." And almost all village presidents noted a tendency among Protestants to stay away from communal festivities. Taken together, then, Protestant missionaries' focus on individualism may lead to a reduction in pro-social preferences. I therefore formulate the following rivaling hypothesis.

- Hypothesis 1b: Protestant missions reduce pro-social preferences in villages where they evangelize as compared to villages where they do not evangelize.

3.2.3 Structure Mechanism: Social Networks

The second mechanism through which Protestant missions may affect community cohesion are social networks. Dense social networks have long been argued to be a key underpinning of social cohesion (Gargiulo and Benassi, 2000; Friedkin, 2004). Local-level cooperation thrives when individuals can rely on strong local ties in their many daily interactions (Tortoriello, Reagans and McEvily, 2012). While the effect on identification and trust is less mechanical, such outcomes, too, likely suffer when networks are sparse. To what degree may Protestant missions affect social networks?

A long research tradition, particularly in economics, has pointed out pronounced effects of religious organizations on network structures (Lewer and Van den Berg, 2007; Berman and Laitin, 2008; Becker and Woessmann, 2013). Historical evidence abounds. One apparent product of the European Reformation and counter-reformation was a profoundly re-configured central European society. The religious schisms led to prolonged political conflict over the distribution of the Holy Roman Empire and culminated in the Thirty Years' War (Wilson, 2009).

Political scientists, too, have assessed the structural repercussions of religion. Huber and Stanig (2011), for example, present evidence that organized religion lends itself to group-based distributive politics as the rich and the religious poor can form electoral coalitions at the expense of the secular poor. Similar arguments are made by Scheve, Stasavage et al. (2006), who demonstrate that religion and state spending are substitutes that insure believers against adverse events. Relatedly, Berman (2000) argues that religiosity signals commitment to the church community, which provides insurance to its members. Underlying these accounts is the view that churches work as exclusive "clubs" that re-configure and sometimes polarize the greater societal network.

Scholars of missions have made similar arguments. They highlight in particular the profound reconfiguration of social structures that results from missionary activity. (Rambo, 1993; Falola, 1998). Sanneh (2015), for example, argues that local communities that rely on dense social networks and carefully balanced political power-structures are particularly affected by missions. Missionary preachers aim to alter existing religious identities and may thereby weaken existing ties. It therefore comes as little surprise that missionaries were historically approached with skepticism and outright hostility from local power holders, who feared that social networks would be weakened as a result of the missionary activity (Forsythe, 1971). McCauley (2012), focusing on Pentecostal missions in sub-Saharan Africa, too, argues that missionaries prize establishing new patronage-like relationships, which may cut through existing networks.

In Latin America, the effect of Protestant missions on local networks is widely recognized. Bot (1999, 168) notes that missionary groups "proliferate in a disintegrating social and economic fabric" and have "success in penetrating even well structured and homogeneous indigenous communities." She further notes that "Pentecostalism first takes root among the poorest, the most marginalised and the most disempowered, along fault lines which for a time are thereby deepened even if at a later stage they are overcome and community cohesion is restored" (1999, 168). The author thus highlights that cohesion is undermined at first before, perhaps, being restored. In a similar vein, Goldin and Metz (1991, 334), in the aforementioned analysis of Guatemala, note that Protestant communities advocate "the promotion of solidarity and formation of clearly identifiable networks (e.g., addressing each other as "brother" and "sister')." These networks, however, are within the missionary group—not outside—thus showcasing the potential of missions to divide rather than unite. Stoll (1993, 15), reflecting on Latin America

more broadly, notes that "Evangelical groups have often been criticized in Latin America for separating their members from the larger society." Yet, the author also points out that "numerous local studies suggest that evangelical congregations have at least become a way for significant minorities of Latin Americans to reform themselves," thus creating "new kinds of social cohesion which empower members."

The degree to which Protestant missions affect social networks is, thus, a question of significant debate. The advent of a Protestant mission need not be detrimental to a local network. If missionaries only target a select few individuals, or, alternatively, convert an entire village, effects on network structures may be positive or, in the words of Stoll (1993, 15) "empower individuals." Yet, Protestant missions seldom convert entire villages, and may thus equally likely polarize and weaken existing networks. Taken together, these theoretical considerations point to two different ways in which Protestant missions may affect networks and, in turn, community cohesion. If missions fail to convert entire villages, or upset historical power-structures, their effect on network structures is likely detrimental. At the same time, if missions convert the masses, or target only a select few individuals, they may strengthen a local network.

In the communities under study, respondents in qualitative interviews were largely undecided about the effects of Protestant missions on village networks. In untreated control villages, village presidents deemed social ties to be strong. Asked about social networks in particular, one president said "everything is normal, everyone is talking to everyone." In evangelized villages the picture was ambivalent. In one village evangelized by the Seventh Day Adventist church, a respondent found converts to take a rather welcoming approach toward those that had not joined the mission. In another village evangelized by the Pentecostal Iglesia Cris-

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tiana Maranatha mission, respondents repeatedly complained about separation and sectarianism. In yet other villages, respondents noted few changes. In the words of one respondent: "religion isn't everything." Given these conflicting theoretical predictions and inconclusive local-level evidence, I therefore formulate the following two competing hypotheses.

- Hypothesis 2a: Protestant missions strengthen social networks in villages where they evangelize as compared to villages where they do not evangelize.

- Hypothesis 2b: Protestant missions weaken social networks in villages where they evangelize as compared to villages where they do not evangelize.

3.3 Empirical Design

Testing the mechanisms by which Protestant missions affect community cohesion requires a causally identified research design that can adjudicate between different causal mechanisms. Most studies on the effects of missions, thus far, have used observational evidence (e.g., Shah and Woodberry, 2004; Trejo, 2009; Nunn, 2010; Woodberry, 2012; Caicedo, 2014). Such research designs, however, may fall victim to unobserved confounding. Notably, Evangelical missions have historically clustered in colonized areas, which differ from non-colonized areas in several ways of which some are unobservable. This chapter addresses the problem of unobserved confounding by exploiting plausibly exogenous variation in missionary activity that took place in the Peruvian Andes in the late 1980's.

3.3.1 Variation in Protestant Missionary Activity

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 Peru's Communist Party, was founded in the late 1960s. The group fought against Peru's "bourgeois democracy". Its goal was to implement a dictatorship of the proletariat (Stern, 1998). The subsequent burning of ballot boxes during a national 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 80s, the Shining Path marked a hindrance to Protestant missions in several regions of Peru. While the interaction between the rebels and missionaries has received little scholarly attention (Ferguson, 2005, 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 my 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 Peruvian theologians. Samuel Escobar (1986, 10) 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.*"

Missionaries, indeed, were a prime target of the rebels. Several leading figures were killed (Cadorette, 1994). Switzer 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" (1993, 61). While the Cusco region—the case study of this chapter—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 re-discovered 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 largely dormant. The missionary rush was particularly prevalent in villages near the city of Cusco. The historical process thus prevented careful missionary planning, which might mean that conversion was pursued independent of village characteristics (Dunning, 2012). Importantly, the situation also meant that some communities were left untreated, which serve as the control category in this chapter.

Interviews conducted with local and international missionaries in the region confirmed the random and hectic nature of the church planting. A Baptist mis-

sionary from the US conveyed to me 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 randomness of the process (see Balance section), together with a team of research assistants I conducted structured interviews with church officials and presidents in all communities studied in this chapter. These interviews, which I revisit in the discussion section and in the supplementary information, yield two important pieces of information regarding the church planting process.

First, to understand whether missionaries followed a specific strategy when selecting villages, I asked the following open ended question: "Why did you set up the church in this village?" Buttressing the 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 believers to go to, and five mentioned abstract reasons of the form "it is necessary to be in all parts where God is."

Second, to understand whether the treatment was exclusive, I asked officials whether there had ever been another mission: "Have there ever been other churches present in this village?" Impressively, 15 out of 16 church officials said no. One pastor claimed that a mission had been present but had discontinued its operation after a few months. In addition, all officials said no to the question "Have there ever been other missionaries present trying to convert people." The assignment of

the missions can therefore be viewed as exclusive and exogenous, which allows us to attribute observed differences across villages to the Protestant missions.

3.3.2 Sample

The overall population of interest consist of the Agrarian communities in two valleys 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 pre-treatment conditions. Specifically, selection of villages followed two main criteria. First, I chose communities that are similar regarding observed pre-treatment covariates, including geographic, linguistic, and agricultural characteristics. Second, I 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 3.9 in the supplementary material. I 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 the strict sampling procedure, the historical setup, and my intention to do an in-depth field study.

3.3.3 Treatment

The final set of 16 villages includes five conceptually different treatments reported in Table 3.1. First, the *Control* treatment depicts villages that were never exposed to a Protestant 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 reason for the Catholic church's gradual decline in the communities under study are manifold. They range from poor central planning, limited resources to re-

cruitment shortages.[1] *Control* villagers, all of whom still identify as Catholic,[2] sometimes visit Catholic parishes in cities where services are still offered. The *Control* communities thus serve as the counterfactual. They allow us to understand what evangelized villages would look like today had they not been exposed to Protestant missions. The reasons why these villages never received a mission are unclear. In qualitative interviews I could not obtain definitive answers 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 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. Adventist villages are also shaped by more outgoing and politically active church leaders who take an interest in their communities.

Table 3.1: Treatment indicators

Treatment	Conversion	N	Clusters	Theology
Control	95.8%	192	3	Limited exposure to Catholic gospel; no church services
Adventists	51.0%	192	3	Limited spirituality, no speaking in tongues, shorter service
Peruana	57.3%	192	3	High spirituality, some speaking in tongues, long service
Maranatha	49.6%	256	4	High spirituality, speaking in tongues, very long service
Mixed	57.3 %	192	3	Combination of the above

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[1] For detailed analyses regarding the Catholic church's decline in Latin America see Stoll (1990) and Gill (2008).

[2] Only 1.4 percent of 1024 survey respondents belong to no church. Similarly, 95.4% of the sample pray regularly.

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, 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 to 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, 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 very 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, my interview requests with church leaders 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.[3] The treatment strength (i.e., the proselytism strategy and theological strictness) therefore ranges somewhere in the middle.

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[3]In one village, 18 converts belonged to a small mission called Israelita, which is comparable to the Adventists in their theological approach.

In Table 3.1, I summarize the five treatment statuses. The third column states how many individuals were sampled in each treatment category. Table 3.4 in the Supplementary Information gives a more comprehensive overview of the sample's treatment status and religious affiliation. Column 2 reports conversion rates. I define conversion as villagers reporting to belong to the mission in a given village. On average, 50 percent report to belong to a mission—a rather high number. Barro, Hwang and McCleary (2010), studying a sample of developed nations, show that conversion rates in Western countries, at most, reach 16 percent (United States; 1998). The conversion rate in this study is thus significantly higher. Two reasons may help explain the apparent missionary success. First, as indicated, the Catholic church had essentially stopped preaching in the area under study, which left believers without spiritual guidance and thus ready to accept new theological guidance. Second, the villages under study are secluded and rather cohesive to begin with. One would thus expect a fervent missionary to succeed in converting people in larger numbers. I should also note that there was likely little selection into treatment. The share of "migrants" in the Census data (see Table 3.7) is a mere 1 percent, on average, across villages. This number corresponds to my own survey instrument on traveling outside the immediate Cusco department (Table 3.5), which 50 percent claim to have done. Taken together, the concern of "selection into treatment" is thus likely limited and the missionaries seemingly succeeded in converting large numbers of individuals.

3.3.4 Sampling

In addition to qualitative surveys, I administered a population survey in all 16 villages, enrolling 64 subjects per village. To do so, I visited each of the 16 villages prior to surveying and obtained consent from village presidents. In 12 villages, I

presented the work in front of the village assembly. In the remaining four villages, the presidents informed villagers that surveying would take place. I was assisted by 21 local Masters students who were carefully selected from a pool of 50, and were covered under Columbia University's IRB.

The entire sample includes 1,024 respondents. 1.3 percent of all outcome measures among participants are missing. 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.[4] Second, enumerators were randomly assigned to villages and to each block within a village in order to avoid enumerator effects.

Surveys were conducted in the late afternoons and at night to maximize participation. Though very time-consuming, sampling during the day was not possible as most villagers take their cattle to the fields. Non-participation 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 ($1.75)—about a day's earnings.

3.3.5 Balance

My key identifying assumption is that assignment to treatment was independent of potential outcomes. I took five steps to buttress this assumption empirically.

First, I gained access to the Peruvian census from 1993, aggregated at the village-level. The data, thus, falls broadly within the period when the missionaries began their conversion in the communities. The census includes data on 91

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[4]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.

variables including indicators on education, demographics and economic development. These data are reported in Table 3.7 in the supplementary information. To assess balance across the different treatment statuses as compared with the control villages, Figure 3.2 plots absolute t-values assessing differences-in-means across the five treatment statues and the non-treated control villages. As can be seen, the vast majority of variables are not significantly different. Indeed, only 3 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 pre-treatment differences on a host of administrative variables, thus, buttresses my key identifying assumption.

Second, I gathered additional information on pre-treatment variables in semi-structured qualitative interviews with history experts in each village. These individuals were recommended to us by village presidents. We asked them 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. The variables are reported in Table 3.6. In order 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 pre-treatment. 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 percent in the untreated control villages to 100 percent 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 pre-treatment balance.

Third, I collected five geographic variables from all villages. I recorded a given village's road and geodesic distance to Cusco (in km) as well as a given

Figure 3.2: Balance across treatment statuses
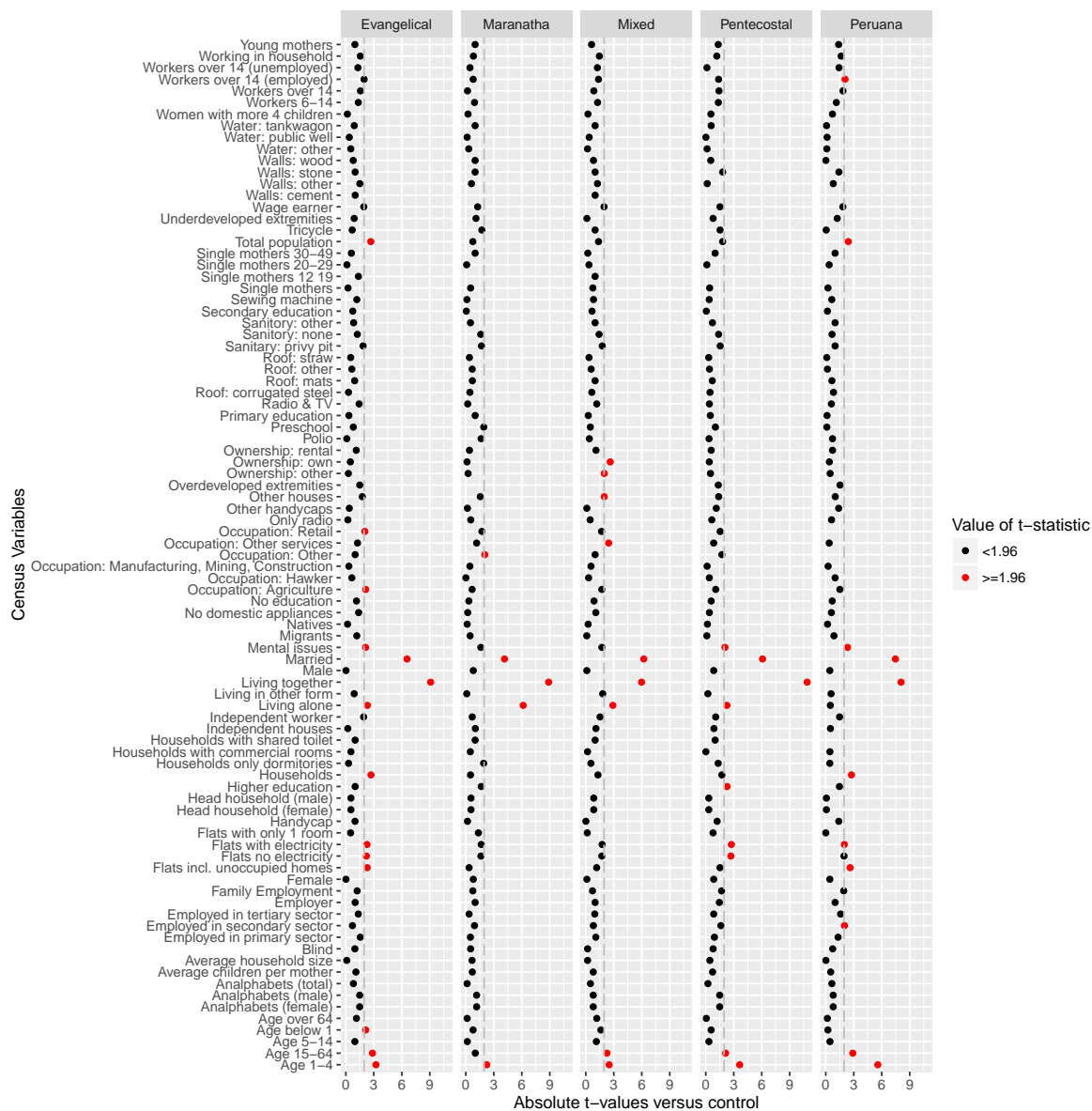
village's elevation and its longitude and latitude. These data are reported in Table 3.6 and offer an objective way to assess balance given that the data are administrative and cannot be affected by the treatment. The table underlines that there are no apparent imbalances between treatment and control 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 thousand feet in all villages, except for the Peruana villages (13 thousand feet).

Fourth, one can scrutinize balance across treatment and control villages in the random population sample. The sample's individual-level variables are reported in Table 3.5 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 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 Mixed villages. Gender, too, is perfectly balanced. Finally, 5 percent of residents in Control villages have internet access compared with 8 percent in Adventist villages, 3 percent in Mixed villages, 5 percent in Peruana villages and 2 percent in Maranatha villages.

Last, a word should be said about the potential for geographic clustering. My key identifying assumption is that Protestant missions selected villages independent of observable characteristics owing to a "missionary race." It could be the case, however, that assignment was exogenous, but at higher levels than the village. Put differently, it may be the case that assignment to treatment was subject to geographic clustering. A cursory glance at the map in Figure 3.9 does not indicate that such clustering is present. A more systematic manner to assess ge-

ographic clustering is to estimate the extent of spatial autocorrelation in my key outcome of interest. The most common measure of such autocorrelation is Moran's *I*. To calculate Moran's I, I need to construct a matrix of inverse distance weights between the villages. I do so using the available longitude and latitude data of the villages.[5] Specifically, I first create a distance matrix and then take the inverse of that matrix, replacing the diagonal entries with zero. In a next step, I then calculate Moran's I.[6] So doing, does not allow me to reject the null hypothesis that there is zero spatial autocorrelation (*p*-value of 0.88).

Taken together, I interpret the balance across a host of pre-treatment variables—census data, expert interviews, geographic data and the population survey—and the absence of geographic clustering in favor of my key assumption, namely, that missionaries visited communities independent of potential outcomes. It should also be highlighted that only two variables exhibit imbalance (notably, marital status, and individuals between 1-4 years of age)—a fraction to be expected by chance alone.

3.3.6 Measurement

This section introduces the strategy to measure community cohesion, pro-social preferences and network structures at the individual level. I exclusively rely on behavioral measures for all core theoretical concepts for two reasons. First, I want to circumvent social desirability bias, given that converted individuals may

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[5]NB: in so doing, I treat the villages as though lying on a plane. In reality, there are differences in altitude across the villages.

[6]Moran's I is defined as $I = \frac{\sum_i \sum_j \mathbf{w}_{ij}(y_i - \mu)(y_j - \mu)}{\sum_i (y_i - \mu)^2}$, where y is my outcome of interest (i.e., social cohesion) and μ is the average of y in my sample. Doing so requires that I define a connectivity matrix $\mathbf{w}$, which denotes the degree to which villages are connected, which I discuss in the text.

have internalized a norm that pro-social behavior is desirable.[7] Second, I want to measure actual behavior that is of relevance in the communities under study, rather than attitudes about behavior. In the following, I begin by discussing my measures for community cohesion—trust, cooperation, and identification. I then go over pro-social preferences and network density. All behavioral measures were applied during a random point during the survey,[8] unless otherwise specified. The instruments are available upon request.

I measure **trust** using the well-known trust-game (Berg, Dickhaut and McCabe, 1995). In particular, each survey respondent was given the opportunity to send any amount of the 2 Dollar participation compensation to the next respondent. The sender was informed that, should the receiver send any money back, the researchers would double that amount. In order to streamline the surveying activities, enumerators did not implement the second part of the trust experiment (the sending back of money by the next respondent) in order to streamline the surveying and because I am only interested in the decision of the respondent whether to invest. As can be seen in Table 3.2, 28 percent of respondents sent money to the next respondent.

Table 3.2: Community cohesion measurement

	Trust	**Cooperation**	**Identification**
	Invested in	*Signed proposal*	*Donated toward*
	trust game	*to reciprocate*	*public good*
Mean	27.6%	34.8%	54.7%

I measure **cooperation** with a novel compliance experiment aimed at capturing reciprocal services in the community. In particular, respondents were asked

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[7]Note that the survey team did not communicate to respondents that the project was about missionaries in order to circumvent demand effects.

[8]Specifically, enumerators were instructed to interrupt the survey at a random point during the survey to play the different behavioral games.

if they would be willing to support a proposal the researchers had arranged prior to surveying activities. In the proposal, village or church members (the endorsers were randomly assigned to respondents) proposed to establish a regular meeting to discuss church or community matters. We then asked respondents if they would sign the proposal. The specific script was as follows: "*Some members of [random: this community / your church] want to establish a meeting to discuss [random: community / church] issues. I have here a letter, which supports this idea. Do you want to sign it?*" On average, 35 percent of individuals gave their signature (Table 3.2).

I measure **identification** using a public donation game aimed at measuring whether respondents were willing to anonymously contribute any amount of their final compensation fee toward the church or village community (the recipients were, again, randomly assigned to respondents). The contributions were paid out six months later when my team of research assistants gathered additional qualitative data. On average, 55 percent of respondents donated an amount toward the respective community (Table 3.2).

The three behavioral items are combined to a comprehensive community cohesion index by standardizing the items and averaging across them. It ranges from 0 to 1 (Cronbach's α of 0.64). I employ the index in the empirical analyses in order to avoid multiple comparisons concerns, and to ascertain a robust measurement of community cohesion.

I measure **pro-social preferences** using the non-strategic Random Allocation Game (Purzycki et al., 2016), which provides an unobtrusive measure of whether an individual is willing to betray others. In particular, respondents were given a six-sided die and given two envelopes. One envelope was to be given to the community / church (fully randomized), the other was to remain with the individual.

Respondents were then asked to throw the die ten times and distribute ten 10 PEN coins, which they were given, into the envelopes. In particular, the person was to put a coin into the community / church envelope whenever the die showed 3 or less; and to put a coin into the own envelope when the die showed more than 3. Respondents played the game in private and were instructed to seal the envelopes after the game. In statistical expectation, 5 coins should land in both envelopes. If the personal or community envelope receive more than half the coins, this is evidence of bias towards the respective entity. On average, individuals allocated 5.2 (SE 0.06; see Table 3.3 and Figure 3.3) coins to themselves, indicating a slight tendency to put their own interest first.

Table 3.3: Mechanism measurement

	Pro-social preferences	**Networks**
	Amount of money allocated to oneself	*Found out neighbor's age*
Mean	5.2	61.9%

Figure 3.3: Random allocation game distribution
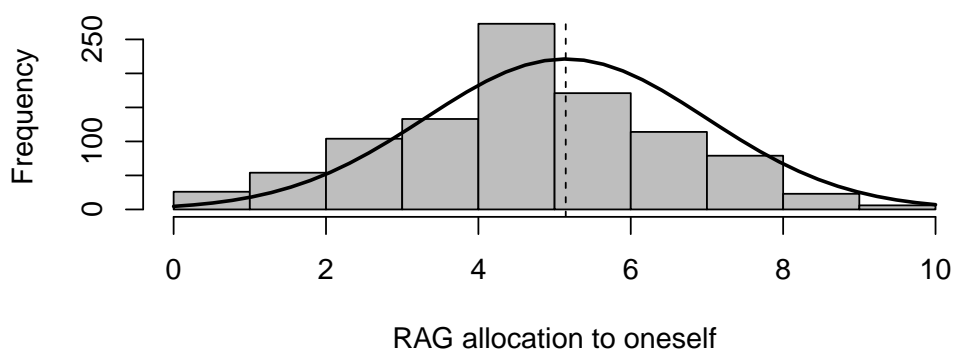
Notes: The figure plots the frequency distribution of the investment in the random allocation game toward the "own" envelope.

Finally, I measure **network density** using a local-level behavioral game intended to measure respondents' ability to obtain relevant social information. In particular, we asked respondents to find out their neighbor's (head of household) age within a time frame of five minutes (respondents where not incentivized this task). Respondents claiming to know the answer—a small minority—were asked to confirm their information. The logic behind this measure is as follows. Given that we implemented the surveys on work days, most surveys were conducted outside of respondents' homes at their places of work (e.g., in the fields or at construction sites). Finding out one's neighbor's exact age thus required respondents to quickly mobilize their network to get the answer within the allotted time. On average, 62 percent of individuals found out their neighbor's age (Table 3.3). Originally, I had intended to check the provided answers. This did not prove feasible, however, as neighbors, too, were frequently not at home.

3.4   Results

To assess the effect of Protestant missions on community cohesion, I analyze the intention-to-treat effect at the village-level. The primary linear model—estimated using OLS—is as follows:

$$Y_{il} = \beta_0 + \beta_1 Adventist_{il} + \beta_2 Mixed_{il} + \beta_3 Peruana_{il} + \beta_4 Maranatha_{il} + \mathbf{X}_{il}\beta + \epsilon_{il}$$

(3.1)

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 signifi-

cant discretion over which variables to include in the models.[9] I therefore focus on variables entirely unaffected by the treatment. At the individual-level (Table 3.5), this includes *age* and *gender*. At the village-level (Table 3.6-3.7), this includes all geographic variables (i.e., road and geodesic distance to Cusco, elevation, longitude and latitude). Additionally, it includes historical pre-treatment 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 3.7. Overall, there are thus well over 100 possible pre-treatment covariates at my disposal. In order to minimize "researchers degrees of freedom" when determining the prognostic value of covariates and to avoid multicollinearity, I therefore report models without covariate adjustment as my preferred benchmark specification. In addition, I report models that include individual-level control variables unaffected by the treatment, namely, age and gender. In the supplementary information, I perform sensitivity analyses, estimating models where covariates are gradually and randomly added to my models. Figure 3.10 in the Supplementary Information presents the average estimated effects and standard errors randomly drawing 1-10 number of covariates, 1,000 times each, which buttresses my 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 assignment is clustered. 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

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[9]The design of the study presented in this chapter was not pre-registered.

bridge this problem, an 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 ten. In this chapter, I therefore apply their method to estimate standard errors.

3.4.1 Community Cohesion

I begin by assessing the primary outcome of interest, community cohesion, which I measure using three behavioral items that capture trust, cooperation and identification. As Figure 3.5 demonstrates, the aggregate community cohesion index is systematically lower in treated villages. The treatment effect is particularly pronounced in Pentecostal villages—i.e., a combination of Maranatha and Peruana— as can be seen in the lower figure. Respondents in Pentecostal villages are roughly 20 percent less likely to sign the village / church proposal, and 22 percent less likely to invest in the trust game. Regarding the four individual missions, I find that the Peruana treatment is associated with a sizable reduction in trust, identification, and cooperation. The effects are both significantly bounded away from zero, and substantively meaningful. Villages exposed to the Maranatha or Mixed treatment witness slightly less pronounced treatment effects, but effects continue to be significantly bounded away from zero. On the other hand, Adventist villages— those exposed to comparatively milder forms of theological preaching—are not systematically different as compared with the control communities. Indeed, most Adventist estimates are remarkably close to zero. For the aggregate community cohesion index, the estimate is even slightly positive (though insignificant).

In a second step, I corroborate these findings using clustered randomization inference, aggregating the treatments at the Evangelical and Pentecostal level. The

analysis in Figure 3.4 shows that Pentecostal missions, in particular, are associated with a decrease in community cohesion. Regarding cooperation, I estimate that villages exposed to Pentecostal missions have a 14 percentage points lower likelihood of signing the village / church proposal (p-value of 0.07). The effect is positive, though insignificant, for the Evangelical missions. Regarding identification, I estimate that Pentecostal villages are 13 percentage points less likely to donate toward a public good (p-value of 0.07). Again, the effect is positive, but insignificant, for Evangelical villages. Finally, I estimate that Pentecostal villages are 10 percentage points less likely to invest in the trust game (p-value of 0.13). The finding, again, is slightly positive among Evangelical villages. In sum, then, I show that Protestant missions reduce community cohesion, particularly so when missionaries label themselves Pentecostal. All results are robust to the inclusion of pre-treatment covariates. Indeed, the pre-treatment covariates do not appreciably change the estimates—as would be expected in a study where the treatment is plausibly exogenous.

Three points merit discussion. First, recall that all three community cohesion outcomes are unobtrusive behavioral measures. They thus capture actual behavior, such as signing a village petition. Second, there is a clear trend that the treatment effects are more pronounced in Pentecostal villages. I should note, however, that the differences between the different estimates are not themselves statistically significant. In addition, the Mixed and Maranatha estimates seem broadly similar, despite the fact that the latter displays a stricter theology. By far the strongest treatment effects are consistently found in Peruana villages. Last, the evidence produced here marks a causally identified counterpoint to several observational studies that have produced a positive correlation between missionaries and community cohesion (or outcomes sufficiently close to community cohesion,

Figure 3.4: Protestant missions and community cohesion (randomization inference)

(a) Cooperation (signed proposal)
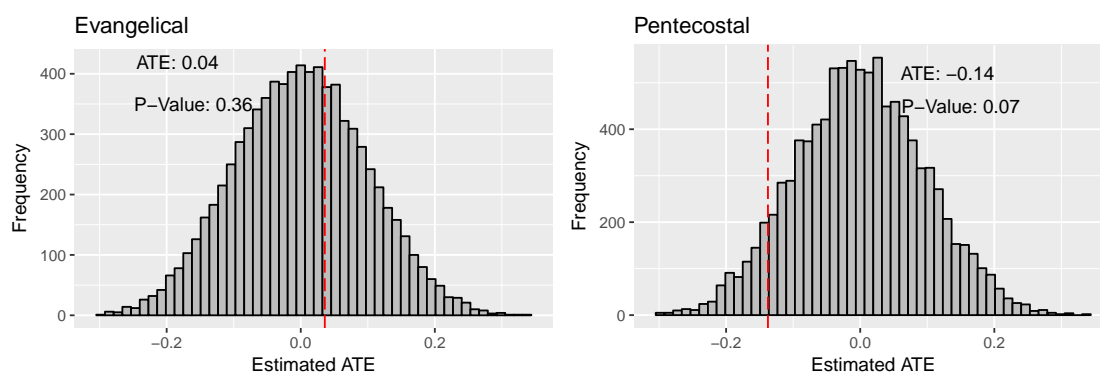
(b) Identification (donated toward public good)
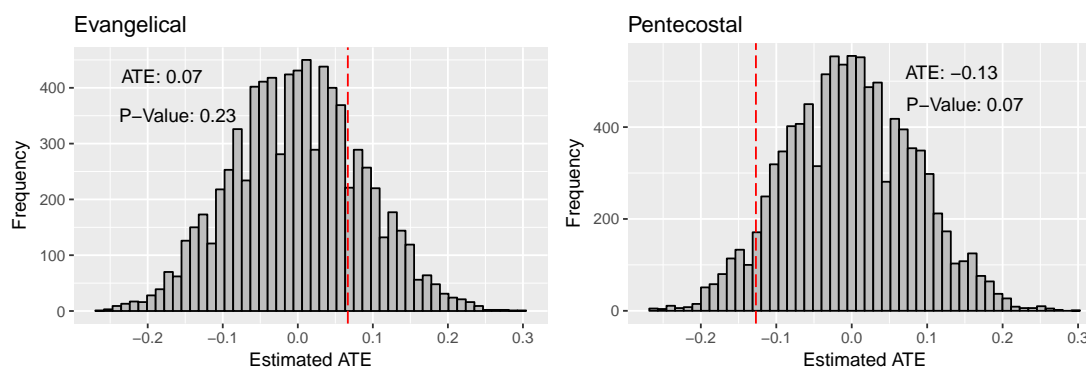
(c) Trust (invested in trust game)
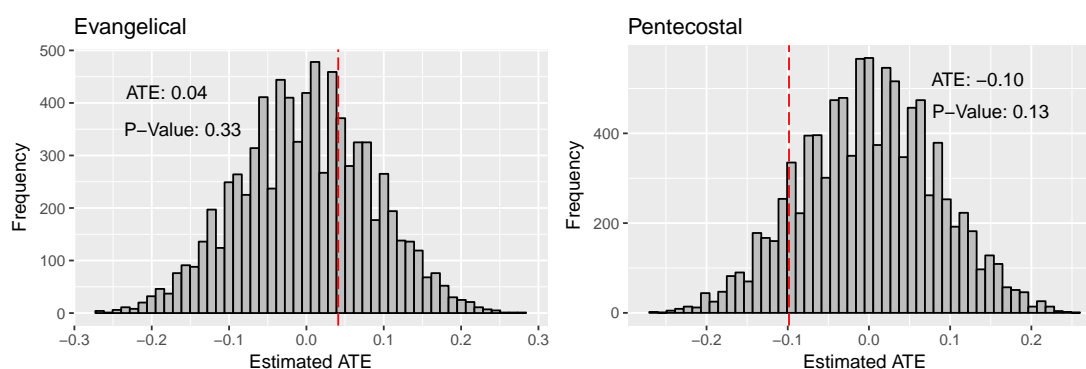
Notes: The Figures plot the estimated average treatment effect (ATE) of the indicated treatment (Evangelical or Pentecostal) on the three outcomes depicting community cohesion using clustered randomization inference under 10,000 simulations. I report the corresponding one-sided p-value.

Figure 3.5: Protestant missions and community cohesion

(a) All treatment conditions
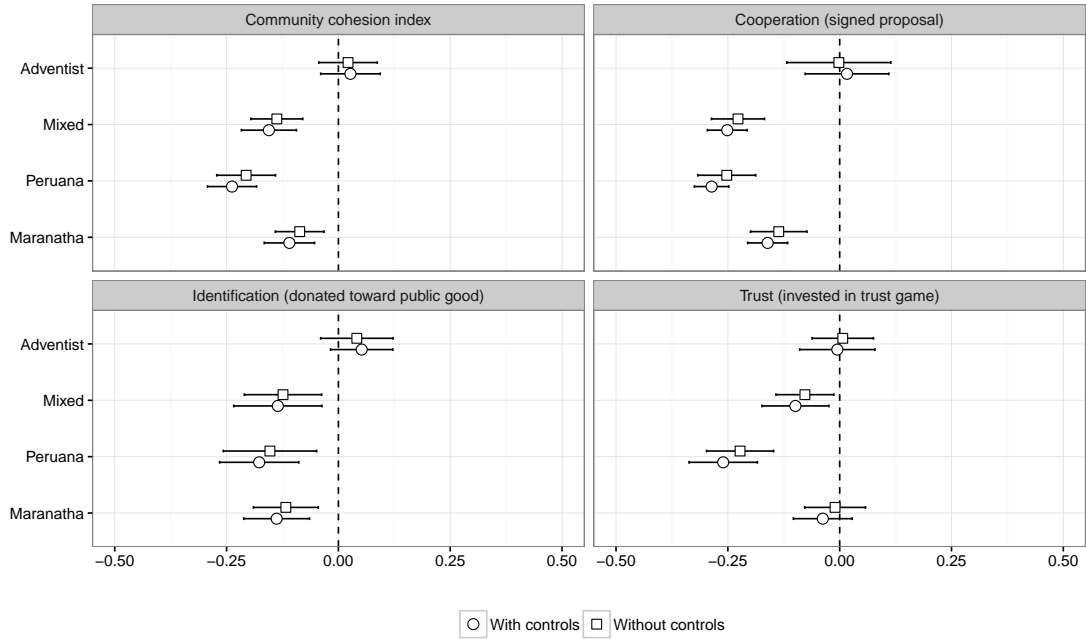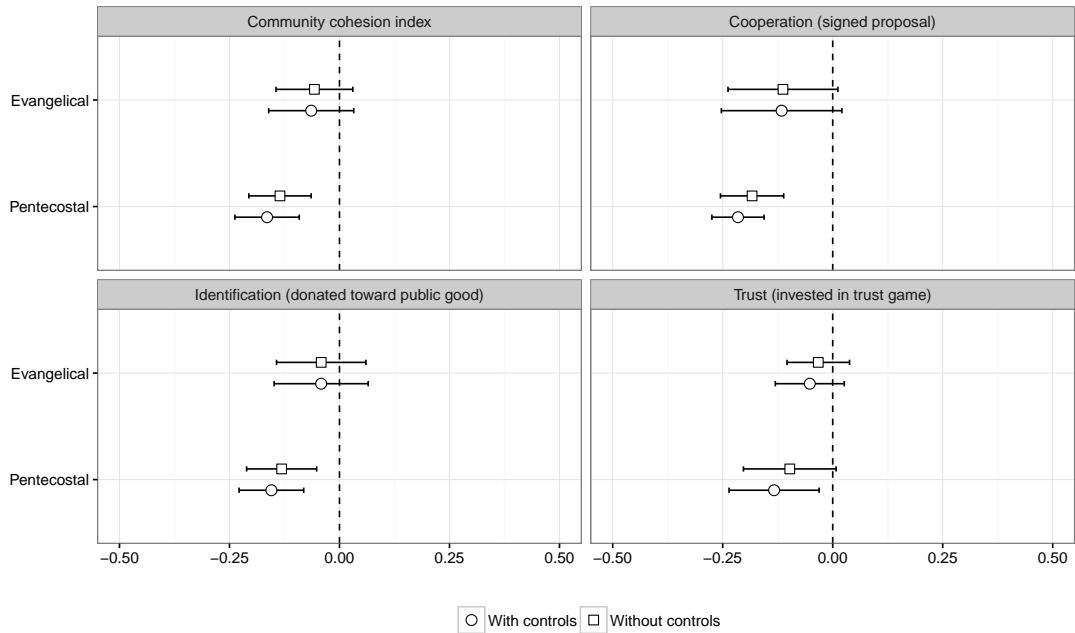
(b) Pentecostal vs. Evangelical

Notes: The figures plot point estimates (dot / square) and 95 percent confidence intervals (lines) of regressions of the four treatments on the indicated outcomes of the community cohesion 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.

including trust). To better understand the drivers behind the reduced form finding, the following section tests the theorized mechanisms.

3.5   Mediation

Next, I turn to the mechanisms that give rise to the observed reduction in community cohesion. I approach mediation in two steps. First, I analyze whether the treatment affects the two mediators. Such an exploratory analysis is useful because causal mediation analysis relies on strong assumptions. Yet, even the critics of causal mediation analysis note that "if certain pathways are unaffected by the treatment, one may begin to argue that they do not explain why (the treatment) works," and that such "exploratory investigation may provide some useful clues to guide further experimental investigation" (Green, Ha and Bullock, 2010, 207). In a second step, I then estimate a causal mediation model, which—under a set of strong assumptions—can help us understand to what degree the observed negative correlation between Protestant missions and community cohesion is mediated through the two hypothesized mechanisms. As in chapter 2, I must caution that these assumptions are unlikely to be met in the present chapter. Thus, the mediation analysis is exploratory in nature and must not be interpreted as causal evidence

First, I regress the preference mechanism on the four treatment indicators. As Figure 3.6a showcases, being exposed to a Protestant mission is associated with a significant drop in pro-social preferences. On average, individuals in treated villages contribute 0.5 fewer coins toward the community (RAG). The effects hold across all four treatment statuses. They are also robust to the inclusion of pre-treatment covariates. The treatment effects do not, however, gain in strength

Figure 3.6: Protestant missions' effects on two mediators

(a) All treatment conditions
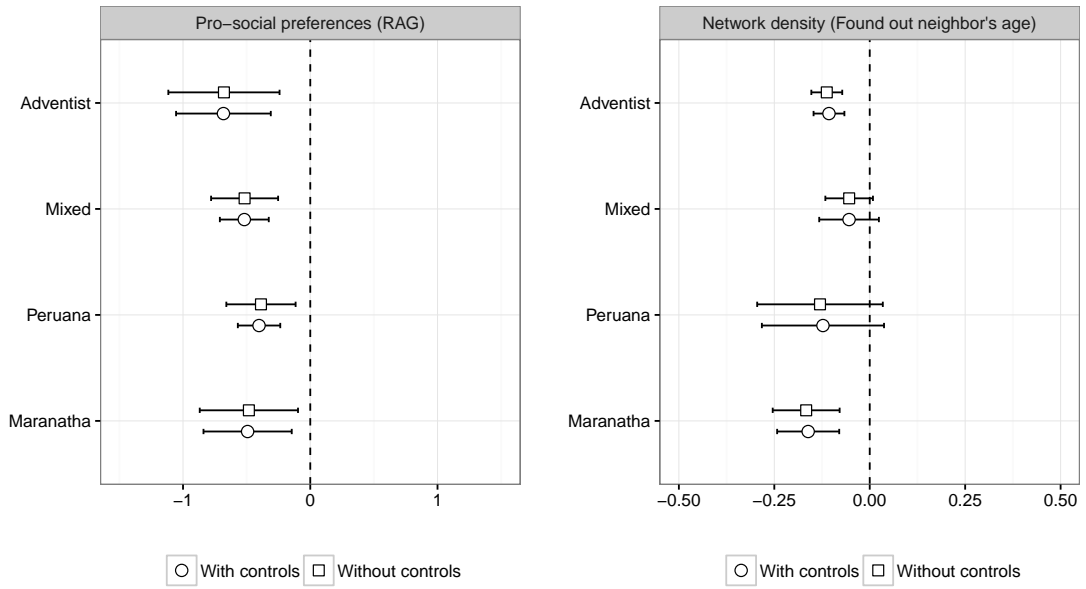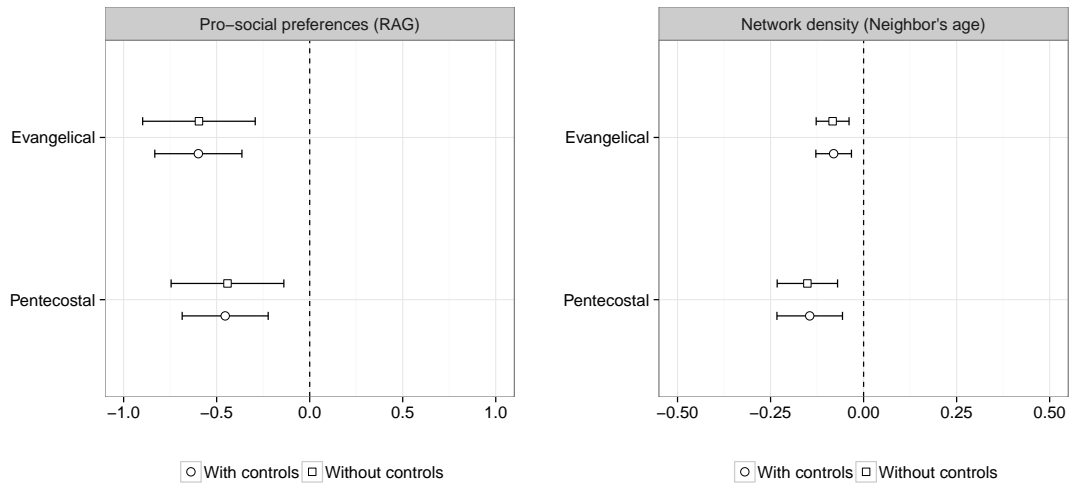
(b) Pentecostal vs. Evangelical

Notes: The figures plot point estimates (dot / square) and 95 percent confidence intervals (lines) of regressions of the four treatments on the indicated outcomes. Dots represent regressions with covariate adjustment, squares represent regressions without covariate adjustment (age and gender). Standard errors estimated using wild cluster bootstrap-t procedure.

as one moves from Adventist to Maranatha missionaries (see also Figure 3.6b). Notwithstanding, the evidence provides support for Hypothesis 1b.

Second, I regress the structure mechanism on the four treatment indicators. As Figure 3.6a demonstrates, network density—measured using respondents' ability to find out their neighbor's age—is consistently lower in evangelized villages. Despite the Adventists adopting the broadest proselytism strategy, the treatment effect is strongest in Maranatha villages. Here, individuals are 18 percent less likely to find out the age of their neighbor. All estimates are robust to the inclusion of pre-treatment covariates. The evidence is thus in with Hypothesis 2b.

3.5.1 Causal Mediation

The analyses thus far have shown that missionaries reduce overall community cohesion, and also negatively affect the two hypothesized mechanisms: pro-social preferences and network density. To explore the role that these mechanisms play, I now move toward the causal mediation framework proposed by Imai, Keele and Yamamoto (2010). To do so, I state my case in the potential outcomes framework. Let mediation be a process whereby a treatment, T, affects an outcome, Y, through a mediator, M. In addition, let $Y_i(t)$ denote the outcome of individual i when treated ($t = 1$) or not treated ($t = 0$). The average causal effect, estimated before, is then $E[Y_i(t = 1) - Y_i(t = 0)]$.

I hypothesized that two variables mediate the relation between the missions, T, and community cohesion, Y. Specifically, let the outcome of these mediators be $M_i(t = 1)$ when assigned to treatment, and $M_i(t = 0)$ when assigned to control. The indirect causal mediation effect is then $\delta_i(t) = Y_i(t, M_i(1)) - Y_i(t, M_i(0))$. It represents the indirect effect of the treatment on the outcome through the mediator (average causal mediation effect). The effect not captured by the two hypothesized

mediators is then given by $\zeta_i(t) = Y_i(1, M_i(t)) - Y_i(0, M_i(t))$. It represents the direct effect of the treatment on the outcome (average direct effect). The total effect—assuming that causal mediation and direct effects do not vary as functions of treatment status (no-interaction assumption; Imai, Keele and Tingley 2010, 312)—is then $\pi_i = \delta_i + \zeta_i$.

In order to causally identify both effects, researchers must invoke two strong assumptions (sequential ignorability). First, one must assume that the treatment is ignorable given pre-treatment covariates. I have defended this assumption in the preceding sections by arguing that the assignment toward treatment and control was plausibly exogenous. In any event, I will adjust for pre-treatment covariates in the following analyses. Second one must assume that the mediators—pro-social preferences and network density—are ignorable given the observed treatment and pre-treatment covariates. That is, one must assume that the mediators, themselves, are exogenous. In other words, the ignorability of the mediator implies that among villages, which share the same treatment status and the same pre-treatment characteristics, the mediator can be regarded as if it were random.

The second assumption is difficult to invoke convincingly in observational as well as experimental studies (Green, Ha and Bullock, 2010) as it cannot be directly tested from the observed data (Imai, Keele and Tingley, 2010, 313). In the present study, however, there is reason to be cautiously optimistic that the missionaries activated the two mediators to varying degrees. In particular, I have argued that the Pentecostal (Maranatha and Peruana) missionaries are more likely to affect the preference mechanism—due to their increased focus on individualism—while the Evangelical (Adventist and Mixed) missionaries are more likely to affect the structure mechanism—given their broader proselytism strategy. This is, of course, not to say that the mediators are exogenous. After all, there may still be

unobserved variables that affect the mediator as well as the outcome. As such, the present mediation analysis is exploratory in nature.

In Figure 3.7, I plot the ACME for the two mediators. As can be seen, despite the fact that the mediators, taken in isolation, are negatively correlated with the treatment, they only marginally mediate the relationship between Protestant missions and community cohesion. When taken at face-value, the network mechanism accounts for roughly 7 percent of the observed variation in the Maranatha condition. The preference mechanism, on the other hand, seems to exhibit no causal role. All estimates are remarkably close to 0 across the four treatment conditions. As such, the estimates imply that the network effects exhibits a more pronounced effect as compared with the preference mechanism, but only in Pentecostal villages. While this finding is in line with my theoretical considerations and qualitative evidence from the field, several words of caution are in order. First, the ACMEs reported here are estimated with significant uncertainty and not significantly bound way from zero. This is predominantly due to the low number of clusters. Second, the ACME relies on the assumption that the mediators are exogenous—an assumption that is ambitious even in the present study where the mediators were plausibly exogenously activated. Third, even if the effects are causal and precisely estimated, they are rather low.

3.6 Testing the Club Argument

Thus far, I have presented evidence that villages exposed to Protestant missions are shaped by lower levels of community cohesion, likely mediated by weaker networks. Before concluding, one widely discussed theoretical question should be addressed. Namely, whether Protestant missions merely *shift* cohesion from the

Figure 3.7: Mediation analysis

(a) All treatment conditions

-O- Controls included -□- Controls not included

(b) Pentecostal vs. Evangelical

-O- Controls included -□- Controls not included
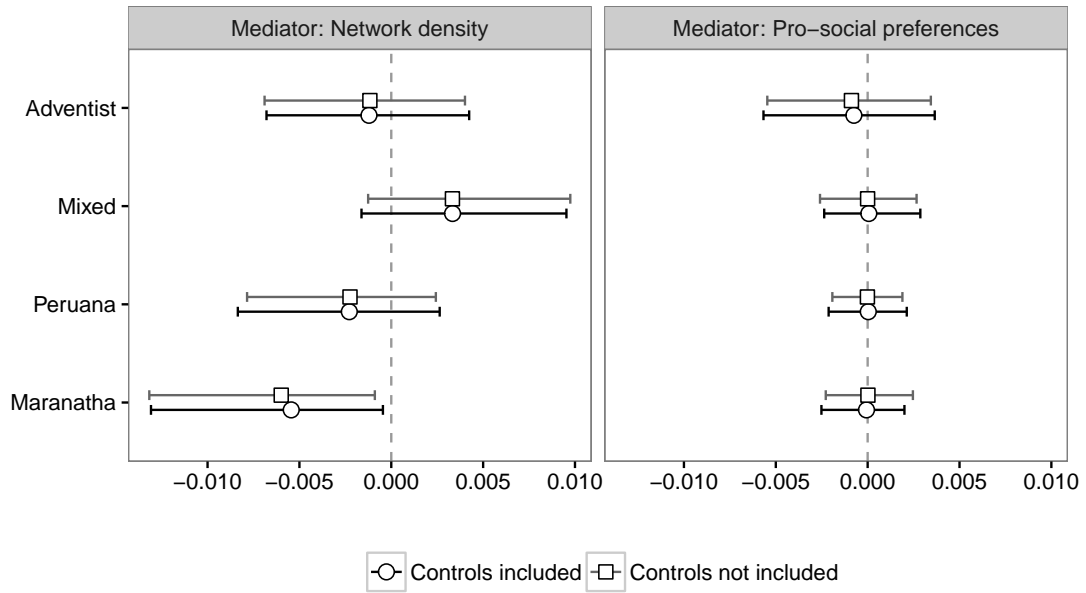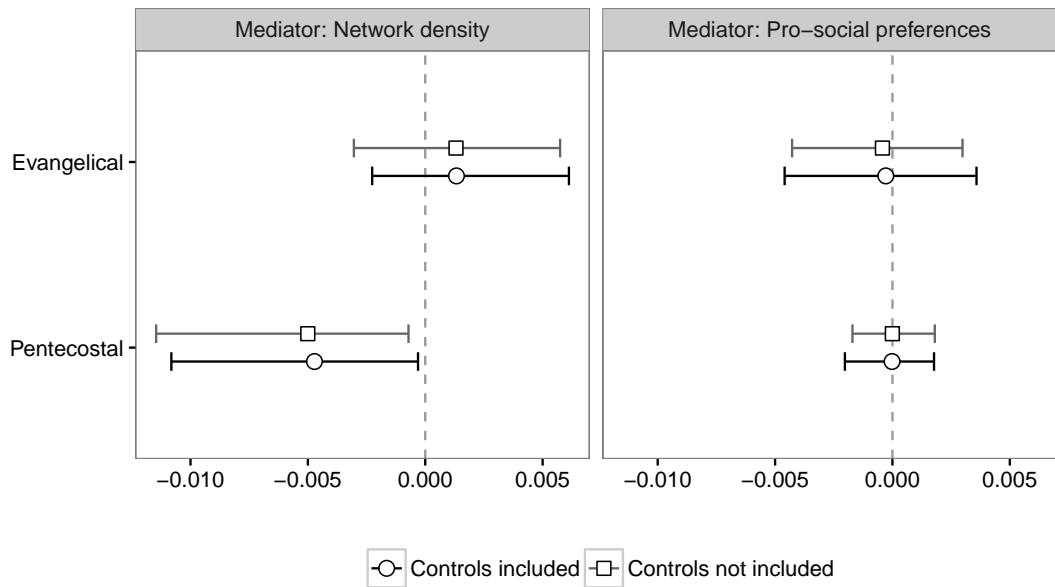
Notes: The figures plot average causal mediation effect estimates (dot / square) and 95 percent Bayesian confidence intervals (lines) of regressions of the four treatments on the indicated mediators. Dots represent regressions with covariate adjustment, squares represent regressions without covariate adjustment.

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greater village to the church community. Following this reading, the communities under study may have lower aggregate community cohesion, but stronger cohesion within the missionary community. If this logic were to hold, one should observe greater trust, cooperation, and identification within the missionary community in treated villages.

To assess this question, I can draw on the fact that two pivotal outcome measures of community cohesion—cooperation and identification—randomized the respective group with which the respondent cooperated or identified. As was described before, respondents were asked whether they wanted to donate money toward the church community or the village community (identification). Similarly, respondents were asked whether they would be willing to sign a church or a village proposal (cooperation). In both instances, the community was fully randomized across respondents, and blocked on gender. The priming experiment thus allows us to test the "club"-hypothesis that missions merely shift community cohesion from the village toward the church. Specifically, I test whether the primes—the church or village community—produce different effect on cooperation and identification levels between evangelized and non-evangelized villages. If Protestant missions merely shift cohesion from the greater village to the church community, we should see a *positive* effect size in treated villages for the church prime.

In Figure 3.8, I plot the cooperation and identification measures, as well as an aggregated index, under the two priming conditions. The figure shows that there are more pronounced negative treatment effects when individuals are primed with the village condition. For instance, while donations are not significantly bound away from zero in the church condition, they are significantly lower when donations go toward the village community (the differences in the estimates are not significant). Differences regarding cooperation are not discernible.

Figure 3.8: Community cohesion within church and village community

(a) Church community
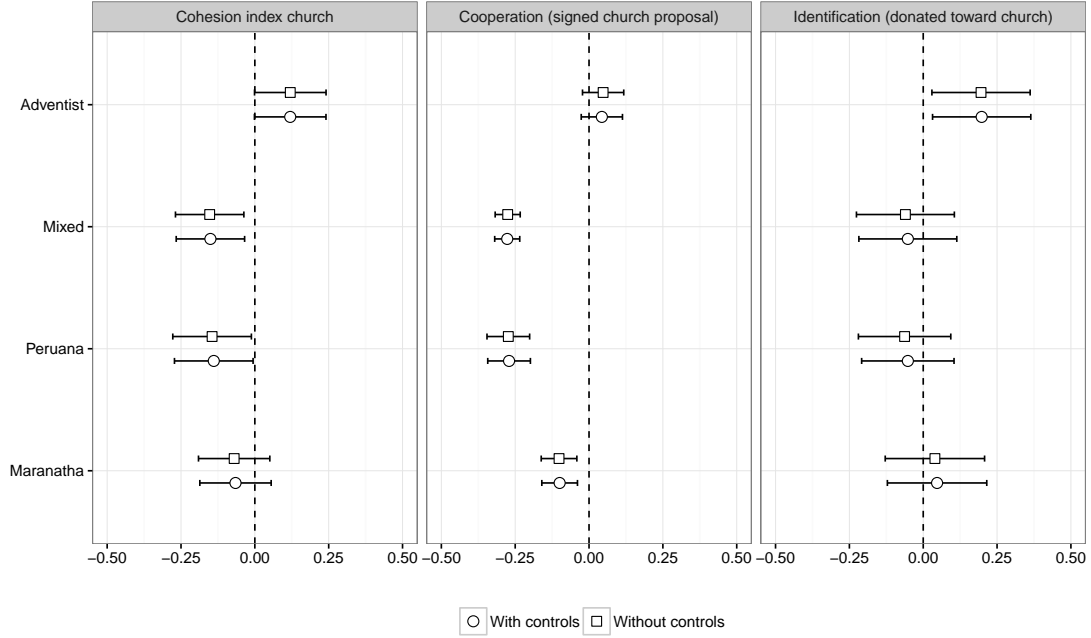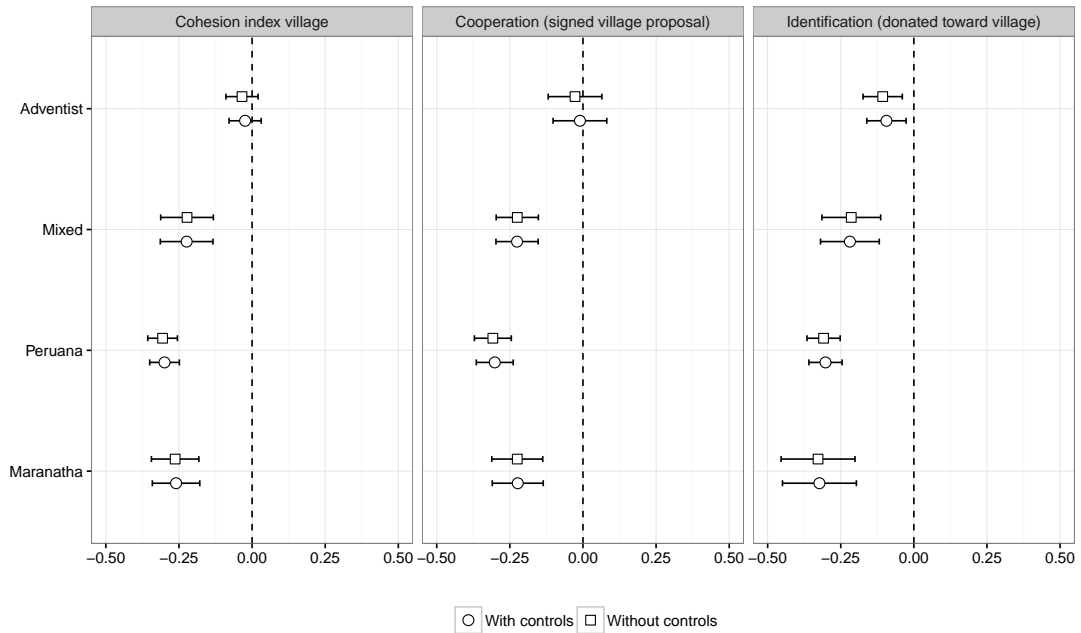
○ With controls □ Without controls

(b) Village community

○ With controls □ Without controls

Notes: The figures plot point estimates (dot / square) and 95 percent confidence intervals (lines) of regressions of the four treatments on the indicated outcomes. Dots represent regressions with covariate adjustment, squares represent regressions without covariate adjustment (age and gender). Standard errors estimated using wild cluster bootstrap-t procedure.

Importantly, however, one should note that the treatment effects, as can be seen when scrutinizing the aggregate church and village community cohesion indexes, are negative even under the church condition. This shows that missions reduce community cohesion not just within the village, but also within the respective church community as compared with the control condition where religion plays a minor role. Therefore, I can rule out the "club"-hypothesis. Both the village and the church community are shaped by lower levels of community cohesion.

3.7   Conclusion

This chapter has revisited a growing number of empirical studies that point to a positive relation between Protestant missions and community cohesion. Focusing on modern-day Protestant missions in southeastern Peru, I presented different logics that can explain a rise and fall in community cohesion as a result of Protestant proselytism. Presenting evidence from an in-depth study of villages in Peru, I found that villages exposed to Evangelical or Pentecostal missions are shaped by lower levels of community cohesion. I found weaker social networks to be a salient mediator of this relation. A reduction in pro-social preferences played no apparent mediating role, though they, too, were lower in treated villages. I also demonstrated that the reduction holds even within the respective church communities, thereby rejecting the well-known "club good" hypothesis.

After collecting the quantitative evidence, I cross-checked the empirical findings by conducting qualitative, semi-structured interviews with all community presidents and one missionary in case a communities was treated. In line with the quantitative findings, the qualitative interviews with village presidents confirmed that Catholic 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 Catholic villages could not think of any significant differences. Regarding community cohesion, two of the three Catholic presidents confirmed that their villages were not separated. The absence of an individualization of preferences, too, was confirmed. None of the Catholic presidents complained about selfish behavior among villagers.

In the proselytized villages, this picture was largely reversed. While Adventist presidents discerned only mild behavioral changes—one president noted no change, the remaining two cited non-descriptive changes—presidents in the Mixed and Pentecostal villages overwhelmingly noted that people worked harder and consumed less alcohol. Regarding overall community cohesion, presidents noted that villagers had become more selfish and displayed stronger religiosity. One Peruana village president complained "they (the residents) changed when the Evangelicals came, we no longer drink alcohol and we are committed to our work and no longer focus on parties." The weakening of networks was noted specifically. One president stated "sometimes, some churches forbid people of different religions to talk to one another." The preference mechanism was also acknowledged by some presidents in treated villages. For instance, one president noted that "within the churches they don't help one another, there is also a lot of egoism within the missions."

While the qualitative interviews largely confirmed the quantitative findings, I want to conclude this chapter by highlighting three potential shortcomings and thus areas of improvement of this study.

First, while I argued that the assignment of missions to villages was plausibly exogenous, the careful case and village selection came at a cost: The number of clusters was low. This is problematic as it increases the risk that effect sizes do not translate to the greater population due to sampling variability. The estimation

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of standard errors is also anything but straightforward. I tried to ameliorate this problem by also gathering qualitative, within-case evidence to corroborate the quantitative findings. In addition, the fact that estimates were largely in line with the theoretical considerations builds trust in the results. Future studies on religious conversion, however, would undoubtedly benefit from increasing the number of units under study.

Second, the declared goal of this study was not only to provide causally identified evidence, but to assess the mechanisms that underlie the observed correlation. In so doing, I explored two specific mechanisms: preferences and networks. The rather low average mediated effect of these mechanisms, however, demonstrates the need to investigate more carefully the full universe of plausible mechanisms. Several mediators come to mind. While education plays a minor role, the fact that missionaries spur abstract spirituality and time devoted to reading scripture may affect to what degree individuals are cognitively willing to engage with others in their community.

The analysis of mechanisms, however, remains a daunting task. Any mechanism may be separated into further sub-mechanisms, few of which can be exogenously varied by a researcher. The analyst of missions thus face a trade-off between causally identified evidence and mechanism analysis. Notwithstanding, by measuring a more comprehensive set of mechanisms, researchers could plausibly show which mechanisms are not affected by missionary activity—a finding arguably as interesting as weakly identified positive evidence. Qualitative interviews may then offer further guidance on which mechanisms are at work, and which are not.

Third, the present study has only rudimentarily described the content of the missionary work and preaching. Though I only found weak evidence that preferences mediate the causal relationship between missions and community cohesion,

I did confirm a strong negative correlation between pro-sociality and the presence of missions. This is valuable evidence for the conjecture that religion affects individual-level preferences. One promising pathway to better understand this relation is to scrutinize sermons in a more systematic descriptive way. When missionaries preach to believers in hour-long sermons, they likely have profound effects on preferences of interest to social scientists. To what degree such effects depend on the content of theological preachings and ideas has received all too little empirical scrutiny in the social sciences.

3.8 Supplementary Information

Figure 3.9: Research sites

(a) Peru

(b) Cusco
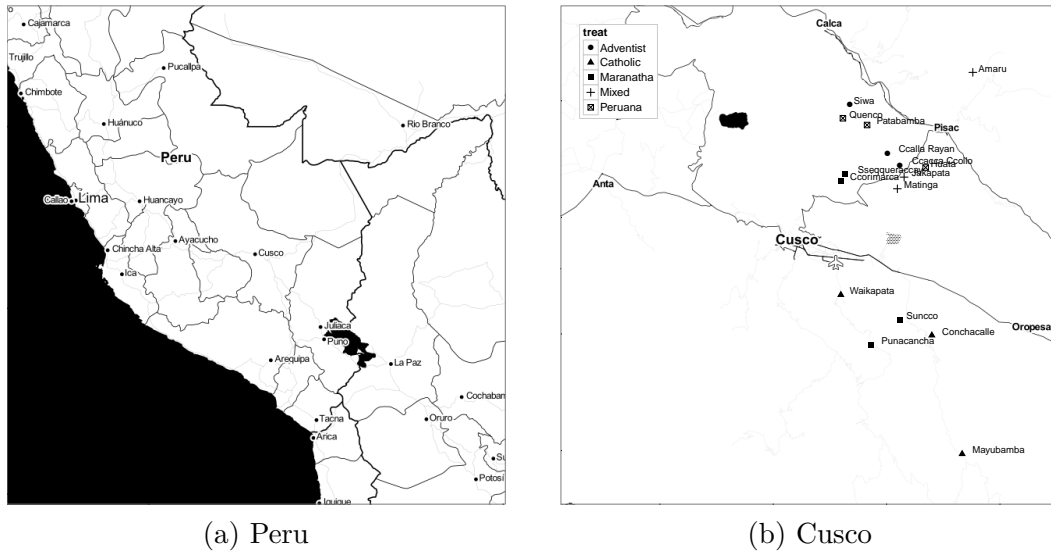
Table 3.4: Treatment and conversion

Treatment	Control	Adventist	Mixed	Peruana	Maranatha	Total
Conversion						
Catholic	184	72	78	73	114	521
%	*95.8*	*37.5*	*40.6*	*38.0*	*44.5*	*50.9*
Adventist	1	98	41	5	3	148
%	*0.5*	*51.0*	*21.4*	*2.6*	*1.2*	*14.5*
Peruana	2	15	34	110	4	165
%	*1.0*	*7.8*	*17.7*	*57.3*	*1.6*	*16.1*
Maranatha	0	0	17	1	127	145
%	*0.0*	*0.0*	*8.9*	*0.5*	*49.6*	*14.2*
Israelita	0	0	18	0	1	19
%	*0.0*	*0.0*	*9.4*	*0.0*	*0.4*	*1.9*
Other	5	7	4	3	7	26
%	*2.6*	*3.7*	*2.1*	*1.6*	*2.7*	*2.5*
Total	**192**	**192**	**192**	**192**	**256**	**1024**

Table 3.5: Individual-level covariates and outcomes

	N	Control		Adventist		Mixed		Peruana		Maranatha	
		Mean	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*	*Mean*	*SD*
Individual-level covariates											
Age (#)	1021	40.9	14.5	39.1	15.1	40.3	15.3	37.2	12.1	38.4	14.4
Male	1024	50.0	50.1	50.0	50.1	50.0	50.1	50.0	50.1	50.0	50.1
Education (1-5)	1023	2.3	0.8	2.2	0.9	2.3	0.8	2.4	0.8	2.5	0.7
Income (PEN)	1012	52.0	95.0	61.6	108.1	44.1	63.0	81.1	131.8	49.4	78.6
No income	1012	28.2	45.1	41.9	49.5	30.9	46.3	31.6	46.6	29.8	45.8
Married	1024	48.4	50.1	62.0	48.7	65.1	47.8	63.5	48.3	63.3	48.3
Relationship	1024	28.1	45.1	10.9	31.3	9.9	29.9	8.3	27.7	12.9	33.6
Single	1024	17.2	37.8	24.0	42.8	18.2	38.7	26.0	44.0	19.9	40.0
Farmer	1024	46.9	50.0	40.6	49.2	52.1	50.1	57.3	49.6	49.2	50.1
Housewife	1024	34.4	47.6	34.4	47.6	29.7	45.8	23.4	42.5	33.6	47.3
Internet access	1010	5.3	22.5	7.8	26.9	2.6	16.0	4.8	21.5	2.4	15.2
Traveled	1020	55.3	49.9	59.4	49.2	57.8	49.5	68.1	46.7	65.5	47.6
Outcome measures											
Investment incidence	1024	35.4	48.0	34.9	47.8	25.5	43.7	9.4	29.2	31.6	46.6
Sign proposal	992	48.6	50.1	50.3	50.1	23.5	42.5	20.0	40.1	32.5	46.9
Signed church proposal	495	48.4	50.2	52.7	50.2	20.7	40.7	21.3	41.1	38.4	48.8
Signed village proposal	497	48.9	50.3	47.9	50.2	26.4	44.3	18.8	39.2	26.6	44.4
Donation incidence	1024	63.0	48.4	68.2	46.7	49.5	50.1	45.3	49.9	49.2	50.1
Donation incidence village	512	72.9	44.7	63.5	48.4	51.0	50.3	42.7	49.7	40.6	49.3
Donation incidence church	512	53.1	50.2	72.9	44.7	47.9	50.2	47.9	50.2	57.8	49.6
RAG themselves	983	5.6	1.8	4.9	2.0	5.0	1.9	5.2	1.8	5.1	1.7
Found out neighbor's age	1017	71.2	45.4	60.5	49.0	65.8	47.6	58.9	49.3	55.1	49.8

Notes: Mean and standard deviations of individual-level covariates and outcome measures gathered in the random
population survey. Variables given in percentages unless stated otherwise.

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Table 3.6: Village-level descriptive statistics from expert surveys

	N	Control		Adventist		Mixed		Peruana		Maranatha	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Geographic data											
Distance Cusco street (km)	16	21.6	14.8	27.5	11.1	27.7	11.6	33.9	6.9	17.0	1.3
Distance Cusco geodesic (km)	16	16.3	9.5	12.5	1.0	15.1	6.8	12.9	0.9	9.9	3.1
Elevation ('000 feet)	16	11.95	0.59	12.34	0.48	12.26	0.30	12.56	0.38	12.04	0.52
Lon (#)	16	-71.9	0.1	-71.9	0.0	-71.9	0.0	-71.9	0.0	-71.9	0.0
Lat (#)	16	-13.6	0.1	-13.4	0.0	-13.4	0.1	-13.4	0.0	-13.5	0.1
Expert interviews – current measures											
Tourism	16	33.3	57.7	33.3	57.7	33.3	57.7	33.3	57.7	25.0	50.0
Families	16	55.0	13.2	81.7	12.6	63.3	20.8	73.3	11.5	67.5	8.7
Paved place	16	33.3	57.7	33.3	57.7	33.3	57.7	33.3	57.7	25.0	50.0
Tenure village president (m)	15	22.0	3.5	16.7	12.7	20.0	6.9	20.0	6.9	22.0	3.5
Village assemblies per year (#)	16	12.0	0.0	12.0	0.0	12.0	0.0	12.0	0.0	12.0	0.0
Relation pres village (1-10)	16	8.3	1.5	4.3	4.2	9.3	1.2	10.0	0.0	6.2	3.6
Relation pres Protestants (1-10)	16	–	–	6.0	4.4	1.0	0.0	1.0	0.0	3.2	4.5
Expert interviews – historical data											
School 1992	16	0.0	0.0	0.0	0.0	0.0	0.0	33.3	57.7	0.0	0.0
Hospital in 1992	16	0.0	0.0	0.0	0.0	0.0	0.0	33.3	57.7	0.0	0.0
Post station in 1992	16	0.0	0.0	0.0	0.0	0.0	0.0	33.3	57.7	0.0	0.0
Football field in 1992	16	33.3	57.7	66.7	57.7	66.7	57.7	100.0	0.0	50.0	57.7
Activity shining path	11	100.0	0.0	100.0	0.0	100.0	0.0	100.0	0.0	100.0	0.0

Notes: Mean and standard deviations of village-level covariates gathered during the expert surveys and using geographic data. Variables given in percentages unless stated otherwise.

Table 3.7: Village-level descriptive statistics from 1993 Census (part 1)

	N	Control		Adventist		Mixed		Peruana		Maranatha	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Total population	16.0	185.3	74.0	449.0	129.3	427.7	298.8	435.0	162.9	225.0	61.2
Male	16.0	49.0	0.5	49.1	2.9	48.8	2.6	49.9	3.4	49.7	1.7
Female	16.0	51.0	0.5	50.9	2.9	51.2	2.6	50.1	3.4	50.3	1.7
Age below 1	16.0	4.0	0.6	2.3	1.7	3.1	0.7	3.8	1.8	3.7	0.6
Age 1-4	16.0	15.8	0.8	13.4	1.9	13.0	1.8	10.4	1.5	13.7	1.7
Age 5-14	16.0	27.0	3.3	28.5	0.6	29.4	1.3	28.3	3.5	27.6	5.4
Age 15-64	16.0	47.5	1.5	51.5	2.5	50.3	1.5	52.2	2.3	49.2	2.9
Age over 64	16.0	5.6	1.8	4.3	1.9	4.2	1.1	5.4	0.6	5.9	3.0
Natives	16.0	98.6	1.1	98.1	1.2	98.8	1.1	98.4	0.7	98.5	0.3
Migrants	16.0	0.6	0.3	0.0	0.0	0.6	0.8	0.4	0.4	0.7	0.5
Foreigners	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Handycap	16.0	1.3	0.7	15.3	24.6	1.3	0.4	10.7	11.7	1.1	1.5
Blind	16.0	0.3	0.5	13.0	22.5	0.2	0.3	0.7	0.8	0.5	0.6
Mental issues	16.0	0.0	0.0	1.1	1.0	0.3	0.3	2.5	1.9	0.3	0.4
Polio	16.0	0.4	0.5	0.5	0.7	0.3	0.5	1.6	2.8	0.0	0.0
Underdeveloped extremities	16.0	0.3	0.3	0.0	0.0	0.3	0.3	1.4	1.6	0.1	0.2
Overdeveloped extremities	16.0	0.0	0.0	0.2	0.2	0.0	0.0	2.1	2.4	0.0	0.0
Other handycaps	16.0	0.3	0.4	0.5	0.4	0.2	0.4	2.3	2.5	0.2	0.4
Analphabets (total)	16.0	30.4	7.5	37.7	16.8	34.1	10.5	34.3	7.0	29.6	8.1
Analphabets (male)	16.0	36.3	0.7	33.2	4.4	33.3	6.6	34.0	5.0	33.8	4.3
Analphabets (female)	16.0	63.7	0.7	66.8	4.4	66.7	6.6	66.0	5.0	66.2	4.3
No education	16.0	22.0	10.4	33.9	19.5	28.9	8.9	28.2	11.5	24.1	4.8
Preschool	16.0	4.4	1.4	2.9	3.1	4.0	0.4	4.1	3.6	2.3	1.4
Primary education	16.0	42.5	3.7	37.9	17.3	43.7	6.3	42.1	3.9	45.0	2.2
Secondary education	16.0	9.2	6.8	6.0	2.8	6.4	2.7	8.3	6.1	9.5	4.8
Higher education	16.0	0.0	0.0	0.1	0.2	0.0	0.0	0.5	0.6	0.3	0.3
Workers 6-14	16.0	0.3	0.5	3.3	5.0	1.2	1.1	2.3	2.9	0.7	0.6
Workers over 14	16.0	24.7	2.5	33.9	13.0	32.4	15.4	39.8	14.0	25.4	7.4
Workers over 14 (employed)	16.0	0.6	0.2	0.2	0.1	0.3	0.2	0.3	0.1	0.5	0.2
Workers over 14 (unemployed)	16.0	2.1	2.1	0.4	0.7	0.5	0.6	0.3	0.5	3.0	3.6
Occupation: Agriculture	16.0	13.9	8.3	24.5	2.6	27.2	10.5	21.5	2.5	18.3	8.7
Occupation: Construction	16.0	1.1	1.4	0.6	0.7	2.7	4.7	1.4	0.5	0.8	0.6
Occupation: Retail	16.0	0.0	0.0	0.1	0.1	0.1	0.1	0.0	0.0	0.2	0.2
Occupation: Hawker	16.0	0.4	0.6	0.0	0.0	0.2	0.3	0.0	0.0	0.3	0.7
Occupation: Other services	16.0	7.8	5.4	5.2	9.0	0.0	0.1	6.1	5.3	3.9	2.2
Occupation: Other	16.0	0.0	0.0	0.0	0.0	0.3	0.5	0.0	0.0	0.7	0.7
Wage earner	16.0	6.6	5.7	0.3	0.3	0.1	0.2	0.5	0.2	2.1	2.4
Independent worker	16.0	13.6	8.4	22.8	0.7	28.1	14.1	21.1	2.1	18.5	10.2
Employer	16.0	0.0	0.0	0.0	0.0	0.0	0.1	0.1	0.1	0.1	0.2
Family Employment	16.0	3.1	1.0	8.8	9.0	3.9	1.5	15.3	10.9	4.0	2.1
Working in household	16.0	0.4	0.5	0.0	0.0	0.0	0.1	0.0	0.0	0.2	0.3
Employed in primary sector	16.0	18.4	9.6	30.0	8.5	27.2	10.5	27.3	6.7	21.8	8.3
Employed in secondary sector	16.0	0.3	0.4	0.1	0.1	1.5	2.6	1.1	0.6	0.9	1.3
Employed in tertiary sector	16.0	1.5	1.5	0.0	0.0	0.6	0.4	0.1	0.1	1.1	1.2
Living together	16.0	26.3	1.4	12.5	1.9	9.5	4.7	10.9	3.0	8.0	3.8
Married	16.0	13.4	3.5	28.4	2.3	26.8	1.3	30.6	1.9	30.2	7.0
Living alone	16.0	14.0	0.5	16.4	4.3	17.7	2.2	15.7	6.1	18.8	1.5
Living in other form	16.0	5.3	1.7	5.4	0.8	7.1	0.3	6.2	2.0	5.2	2.3
Heads of household	16.0	43.3	16.7	108.7	28.3	100.7	74.2	100.3	31.7	49.0	11.7
Head household (male)	16.0	84.8	7.7	83.6	5.1	80.6	3.4	84.4	2.5	87.7	5.7
Head household (female)	16.0	15.2	7.7	16.4	5.1	19.4	3.4	15.6	2.5	12.3	5.7
Average children per mother	16.0	7.3	2.1	5.7	0.6	6.3	0.6	6.3	2.5	6.3	1.7
Women over 4 children	16.0	7.1	3.0	6.0	1.2	7.5	1.4	5.2	3.5	6.6	2.1
Single mothers	16.0	0.3	0.4	0.2	0.2	0.5	0.2	0.2	0.2	0.1	0.2
Single mothers 12 19	16.0	0.0	0.0	0.1	0.1	0.2	0.3	0.0	0.0	0.0	0.0
Single mothers 20-29	16.0	0.1	0.2	0.1	0.2	0.2	0.2	0.2	0.2	0.1	0.2
Single mothers 30-49	16.0	0.1	0.2	0.0	0.0	0.1	0.2	0.0	0.0	0.0	0.0
Young mothers	16.0	0.3	0.3	0.6	0.6	0.4	0.1	0.6	0.2	0.6	0.4
Flats incl. unoccupied homes	16.0	48.3	20.5	108.7	28.3	107.0	84.8	103.3	30.2	52.8	7.8
Households	16.0	43.3	16.7	108.7	28.3	100.7	74.2	100.3	31.7	49.0	11.7
Average household size	16.0	4.3	0.6	4.1	0.1	4.3	0.2	4.3	0.3	4.6	0.6
Independent houses	16.0	0.9	0.2	0.6	0.5	1.0	0.1	1.0	0.1	1.1	0.2
Makeshift	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Other house	16.0	22.7	19.9	4.3	7.4	0.0	0.0	8.3	14.4	4.6	4.9
Ownership: own	16.0	73.9	16.2	66.7	52.1	98.5	1.7	62.4	49.1	71.4	33.0
Ownership: rental	16.0	3.8	4.1	1.1	1.9	1.1	1.1	1.9	2.1	2.6	4.2
Ownership: occupied	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Ownership: other	16.0	21.4	18.4	32.2	53.0	0.4	0.6	35.7	49.2	26.0	30.1
Walls: cement	16.0	0.0	0.0	0.0	0.0	0.2	0.3	0.0	0.0	0.0	0.0
Walls: quincho	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Walls: stone	16.0	0.0	0.0	0.0	0.0	0.2	0.3	2.6	3.2	1.1	2.2
Walls: wood	16.0	1.1	1.9	0.3	0.5	0.2	0.3	1.1	1.9	0.0	0.0
Walls: mat	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Walls: other	16.0	98.0	1.8	99.7	0.5	99.5	0.9	96.3	3.3	98.9	2.2
Roof: concrete	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Roof: corrugated steel	16.0	4.4	7.6	3.2	2.9	8.5	7.8	13.9	18.4	2.3	3.1
Roof: mats	16.0	2.6	4.6	0.3	0.5	0.0	0.0	0.8	1.4	0.7	1.4
Roof: straw	16.0	50.6	35.9	66.1	29.7	58.9	20.9	47.8	28.4	39.5	39.1
Roof: other	16.0	41.5	23.0	30.4	26.6	32.6	13.7	37.5	32.3	57.4	38.2
Water: private	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Water: public well	16.0	39.0	45.8	6.7	7.3	49.8	24.0	43.8	34.2	34.6	28.9
Water: tankwagon	16.0	4.3	7.4	0.8	1.4	0.0	0.0	3.8	6.6	0.0	0.0
Water: other	16.0	55.9	46.2	92.5	8.6	50.2	24.0	52.3	27.9	65.4	28.9

Notes: Mean and standard deviation of covariates. Variables given in percentages unless stated otherwise.

Village-level descriptive statistics from 1993 Census (part 2)

	N	Control		Adventist		Mixed		Peruana		Maranatha	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Sanitary: connected grid	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Sanitary: privy pit	16.0	0.0	0.0	1.9	1.7	10.9	10.7	0.5	0.9	26.1	31.1
Sanitary: other	16.0	0.9	1.5	0.3	0.5	0.0	0.0	0.0	0.0	0.4	0.8
Sanitary: none	16.0	98.2	3.0	97.9	2.0	89.1	10.7	99.5	0.9	73.5	30.6
Flats with electricity	16.0	0.0	0.0	22.7	30.2	47.4	46.9	53.1	46.2	40.2	48.6
Flats no electricity	16.0	99.1	1.5	77.3	30.2	52.6	46.9	46.9	46.2	59.8	48.6
Flats with only 1 room	16.0	51.0	17.0	59.8	4.3	52.8	10.7	51.0	26.2	33.7	16.5
Households only dormitories	16.0	59.6	26.3	59.8	4.3	50.1	14.2	50.2	26.9	25.3	19.1
Households with shared toilet	16.0	0.0	0.0	0.0	0.0	2.6	4.4	0.0	0.0	0.7	1.4
Households with comm rooms	16.0	3.0	3.2	1.2	1.2	2.6	2.5	4.5	4.5	1.9	2.7
No domestic appliances	16.0	19.0	15.4	37.3	20.3	30.3	9.8	24.7	5.8	21.0	9.5
Only radio	16.0	58.4	19.8	59.0	17.3	52.1	11.9	48.7	18.5	51.4	13.3
Radio and TV	16.0	81.0	15.4	62.7	20.3	68.9	9.0	75.3	5.8	79.0	9.5
Sewing machine	16.0	31.5	28.6	5.8	4.6	17.4	7.4	19.7	13.8	29.4	11.3
Fridge	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Tricycle	16.0	0.9	1.5	0.5	0.5	0.0	0.0	0.8	1.4	28.1	31.6
More than 4 appliances	16.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0

Notes: Mean and standard deviation of covariates. Variables given in percentages unless stated otherwise.

Figure 3.10: Robustness to covariate adjustment
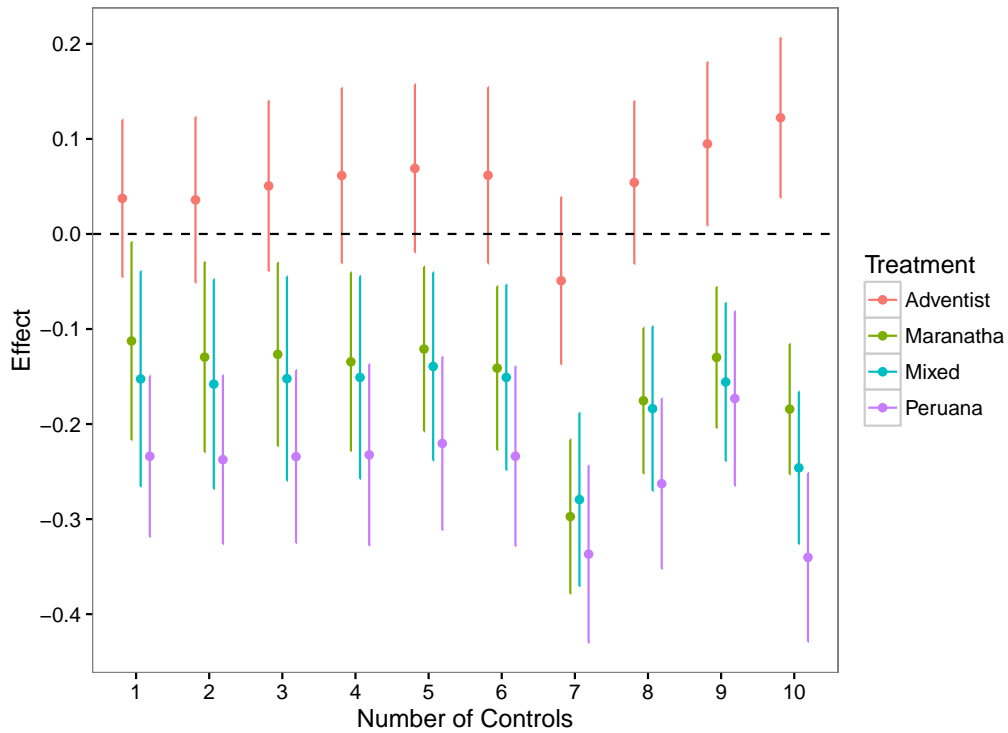
Notes: The figure plots average point estimates (dots) and cluster robust standard errors (lines) of regressions of the cohesion index on the four missionary treatments including x number of randomly drawn covariates running 1,000 iterations. I use cluster robust standard errors, rather than wild cluster bootstrap-t standard errors, to ease computation.

4

Missionaries and Social Capital

4.1   Introduction

A variety of scholars have hypothesized that religious organizations increase social capital (notably, Putnam, 2000). The reasons are manifold. Above all, religious communities function as civic associations and may thus increase associational activity (Curtis, Baer and Grabb, 2001). Moreover, religious doctrines affect how believers relate to other members of society, perhaps spurring pro-social behavior. Indeed, in the U.S. context religious groups are asserted to "have more potential to contribute to America's social capital than any other institution in American society" (Smidt, 2003, 2). Protestant missionaries are particularly highlighted as key catalysts of social capital. As Woodberry (2012, 252), focusing on Great Britain and the United States, notes: "CPs [conversionary protestants] disproportionately mobilized and signed petitions [...], and they organized and led virtually all the organizations and movements that formalized these tactics in the early 1800s."

Despite a rich historical and theoretical literature, causal evidence regarding the role of Protestant missionaries in social capital formation is scarce. This par-

ticularly holds true for the missionaries who are active in Sub-Saharan Africa today. The few studies that exist rely on observational historical data and discuss foreign missionaries. This is problematic for a number of reasons. Above all, Protestant missionaries carefully select communities rather than converting people at random. For instance, Nunn (2010, 147), discussing missionaries in Sub-Saharan Africa, writes: "[a] number of factors played a role in determining a mission's location. Among the most important were access to a clean water supply, the ability to import supplies from Europe, and an abundance of fertile soil that could be used to grow crops." Besides such selection, historical data sources may fall victim to reporting biases and can only crudely proxy modern conceptualizations of social capital. Moreover, given a staggering rise of Protestantism across Sub-Saharan Africa (e.g., Marshall, 2009), we lack evidence about the effects of today's evangelizers, particularly those who are not foreign imports. This is surprising. After all, notes Killingray (2011, 93), "African Christians, not foreign missionaries, have been largely responsible for the spread of the Christian Gospel across the continent."

To advance this debate, this chapter presents evidence from a randomized field experiment implemented in Eastern South Sudan. I partner with a local Protestant missionary group to scientifically evaluate the consequences of their evangelism. The group adopts a strategy whereby villages are evangelized for two to four weeks in the hopes that individuals will then regularly join the group's main church compound in a nearby town. Evangelism takes the form of intensive preaching sessions twice a week. Evangelists touch on core Christian topics and make explicit recommendations about behavior.

Using a cluster-randomized design, I randomly assigned 60[1] geographically

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[1]The experiment is currently still ongoing and will conclude in July 2017. Therefore, the empirical part only includes 38 villages.

matched village pairs to a first and a second treatment phase to be subsequently visited by the missionary group (*waitlist design*). To control for time trends, the village pairs were also put in random order. Two weeks after the evangelism in a given village had concluded, a team of local enumerators conducted a complete population-level survey in the respective treatment and control village pair. The survey included attitudinal items as well as lab-in-the-field experiments to measure social capital. Specifically, it focused on networks, trust, cooperation, information, cohesion, and empowerment (Woolcock, 1998). Additionally, the enumerators inquired about general attitudes toward the local Toposa tribe's beliefs and traditions, thus measuring broader cultural effects. To minimize the potential for "fishing" (Humphreys, de la Sierra and Van der Windt, 2013), I pre-registered the design, hypotheses and estimation strategy at *Evidence in Governance and Politics (EGAP)*, and made all data publicly available.

Comparing outcomes across treated and untreated villages, I first document that missionaries have profound effects on local traditions and beliefs. A comprehensive tradition index scores 0.2 standard deviations lower in treated villages. Protestant missionaries led individuals to wear fewer witchcraft items, to pray more regularly, and to become more skeptical of long-held cultural practices such as polygamy and dowries. Traditional animism, such as believing in minor spirits and frequenting witch doctors, is also shown to be significantly lower. Moreover, the evangelists have a positive effect on knowledge about Christianity. A Christian knowledge index scores 0.2 standard deviations higher in evangelized villages.

With this strong effect on local traditions, beliefs and knowledge in mind, I assess the pre-registered battery of social capital indicators. Surprisingly, I find that the aggregate social capital index is unaffected by the missionary intervention. However, upon closer inspection—drawing on post-hoc "researchers' degrees

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of freedom" (Simmons, Nelson and Simonsohn, 2011)—I uncover two significant counteracting forces in the index. All *group-level* social capital outcomes are negatively affected by the missionaries. This includes the village network size, safety, petitioning and generalized trust. By contrast, all *individual-level* social capital outcomes are positively affected. For example, residents in evangelized villages invest greater amounts in a trust game, and need less time to obtain information from other village members compared to villages not evangelized.

I argue that this result portrays evangelism as a network interruption treatment. Missionaries undermine deep seated cultural beliefs and practices, which reduces overall social capital. Yet, at the individual-level, their preachings of charity and forgiveness work to foster pro-social behavior. The effect of missionaries on social capital is hence ambiguous. While further research spanning different missionary groups, time frames and locales is necessary to elaborate on this effect, the estimates show why the debate on protestant missionaries and social capital continues to be contested.

This chapter adds to the literature on social capital and religious organizations in two important ways. First, I present field experimental evidence assessing the consequences of Protestant proselytism. Given the pronounced historical effects of missionaries, which has been highlighted by other scholars (e.g., Lankina and Getachew, 2011; Woodberry, 2012; Lankina and Getachew, 2013; Caicedo, 2014; Meier zu Selhausen, 2014; Sanneh, 2015), I add to the literature by providing causally precise estimates. Second, I contribute to an ever-growing literature on the determinants of social capital (e.g., Knack and Keefer, 1997; Putnam, 2000; Glaeser, Laibson and Sacerdote, 2002; Kaasa and Parts, 2008), by pointing out the role of Protestant missionaries in spurring and hindering social capital formation.

4.2 Missionaries and Social Capital

4.2.1 Definition

Popularized by Bourdieu (1984) and Putnam, Leonardi and Nanetti (1993), social capital is a core interest of a wide array of social scientists. It has been linked to numerous salient social outcomes such as economic growth (Beugelsdijk and Van Schaik, 2005) and democratization (Paxton, 2002, 254). Across several disciplines, Protestant missionaries have been pointed out as one determinant of outcomes pertaining to social capital (e.g. Neill, 1966; Dietrich, 1992; Macdonald, 1993; Peel, 1995; Steinmetz, 2008; Porter, 2004). After all, notes historian Makdisi (1997), missionaries are not merely "purveyors of modern medicine and print technology." Rather, proselytism represents a "fragile process of staking out claims of cultural and historical belonging," which affects longstanding societal modes of conduct.

Precisely what is meant by "social capital," however, continues to be a source of controversy. As Portes (2000) notes, sociologists have traditionally conceptualized social capital as benefits accruing to individuals. By contrast, political scientists, beginning with Putnam, Leonardi and Nanetti (1993), have traditionally transposed the concept onto larger social units, viewing social capital as an attribute of the community or nation state that facilitates collective action.

In this chapter, I adopt a definition of social capital that has found its way into many empirical studies. Specifically, I rely on the influential World Bank guide to social capital (Woolcock and Narayan, 2000). It defines social capital along six dimensions: "Groups and networks; trust and solidarity; collective action and cooperation; information and communication; social cohesion and inclusion; empowerment and political action" (Grootaert et al., 2000, vii).

For the purpose of this study, I collapse these dimensions into networks, trust, cooperation, information, cohesion and empowerment. In so doing, I note that this definition closely maps onto other influential definitions. For example, Karlan (2005, 1689) writes "trust and trustworthiness are two critical traits encompassed by individual social capital." Paldam (2000, 629) argues that social capital relies on the fact that "people build trust in and networks to others and come to cooperate with them" (see also, Coleman, 1988; Sobel, 2002; Lin, 2002).

4.2.2 Hypotheses

Why might Protestant missionaries affect social capital? In the pre-registration document, I laid out six hypotheses about the effects of Protestant missionaries based on qualitative interviews in South Sudan's Eastern Equatoria region—this chapter's empirical focus. Since the end of the second South Sudanese civil war in 2005, the region has seen a growing influx of missionary groups (more below in the Setting section). It thus allowed me to form expectations about the effects of Protestant missionaries on social capital. I will discuss the resulting pre-registered hypotheses in turn.

First, I expected ***networks*** to be weakened as a result of Protestant evangelism. Partly drawing on my own research (chapter 3), I expected Protestant missionaries to cut through existing social networks by providing a new religious identity, which some follow fervently, while others remain skeptical. The expectation was also motivated by a burgeoning literature conceptualizing religious groups as "clubs." Such theories highlight that religious groups, like families, produce and consume religious goods, which generates collective benefits. Problematically, this makes religious groups—including missionary groups—vulnerable to free-riding. As a result, religious groups, writes Iannaccone (1994, 740), use "gratuitous costs

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('sacrifice and stigma) [...] to mitigate free rider problems" (Douglas, 1966; Ruffle and Sosis, 2007). Indeed, the missionary groups under study in this chapter use public acts—such as the public burning of witchcraft items—to screen true believers from not-so-true believers (more below). As a consequence, village networks may become polarized and likely weakened due to the missionary activity.

Second and partly as a result, I expected ***trust*** within the community to decrease. Legal scholars and economists highlight that exchanges between individuals depend on the identity of one's partner, particularly when legal frameworks are not well developed (Carr and Landa, 1983, 135-136). If the availability of a new religious identity creates two new groups, aggregate trust will likely be reduced. This is particularly likely when new converts enter the equation. Carr and Landa continue: "[c]onverts [...] are generally treated with suspicion. Converts are not trusted to the same extent that bona fide long-standing coreligionists are" (1983, 154). At the same time, Protestant missionaries are frequently portrayed as strong proponents of pro-social trusting behavior(McCracken, 2008). Based on my qualitative interviews and evidence from Peru (see chapter 3), however, I pre-registered a hypothesis that expects trust to decrease as a result of the missionary activity.

Third, I expected village-level ***cooperation*** to decrease. Religious groups, including the missionaries studied in this chapter, require converts to direct mental and physical resources toward the missionary community. This includes making offerings during services as well as providing material support for those in need. The fact that religious groups provide "insurance" to its members has been highlighted by theoretical as well as empirical papers (Clark and Lelkes, 2006; Scheve and Stasavage, 2006; Dercon et al., 2006; Stegmueller, 2013). Scheve, Stasavage et al. (2006), for instance, note that religion and state spending are substitute mechanisms to insure members against adverse events. As such, missionary groups

may crowd out support for and cooperation with the greater village (see also, Huber and Stanig, 2011). While there are a variety of studies that show cooperation to be greater *within* religious groups (e.g., Sosis and Ruffle, 2003; Bulbulia and Sosis, 2011), this may come precisely at the cost of lowering cooperation *between* new converts and those not converted. In addition, missionaries in the region are not entirely welcoming of local traditions and beliefs, which may further weaken established modes of cooperation.[2]

Fourth and closely related, I expected both ***information*** and ***cohesion*** to be reduced by missionary groups. If networks are sparser, trust less pronounced and cooperation reduced, one would, almost by definition, expect cohesion and information flows to be lower, too. The degree to which religious groups affect cohesion, however, is determined by a variety of factors. As Wald, Owen and Hill Jr. (1990, 197) note about political cohesion in U.S. congregations: "'Strong' churches, defined by a combination of theology, social practices, and demographic characteristics, apparently possess the necessary resources to promote attitudinal conformity." At its worst, writes Gellner (2000, 153), "[r]eligion employs an idiom of unequal patronage," while the broader political sphere "speaks a language of participatory brotherly or cousinly equality." While the missionaries studied in this chapter can hardly be labeled as speaking the idiom of unequal patronage, they may still undermine general cohesion and information-flow within the greater village network, owing to the substitution effect laid about above.

Finally, I hypothesized that ***empowerment*** within evangelized villages would increase. In line with Duflo (2012, 1053), I define empowerment as "the ability to [...] access the constituents of development—in particular [...] political par-

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[2]See Block (1994) for a careful treatment of the effect of missionaries on cooperation and the broader culture of indigenous tribes in the upper Amazon.

ticipation."[3] This hypothesis was based on my field work in the region. When observing church services and local evangelism, converts expressed joy about their newly found belief. Stories of "being saved" and "liberated" were common. Just as important, non-converts went on with their daily life. There was thus no crowding out effect. Moreover, I based the hypothesis on the missionary self-description as being "liberating powers through the gospel" (Danfulani, 2001, 7). Local missionaries portrayed their work as "saving individuals" and "bringing a story of love and redemption." Similar evidence about the "liberating" aspect of the Protestant theology is provided by authors from other contexts (e.g., Marshall, 2009). Psychological studies, too, have found a link between religiosity and empowerment (Maton and Wells, 1995; Brodsky, 2000; Park, 2005).

ne definition is given by McClendon and Riedl (2015, 1047) who note that "Pentecostal churches in Sub-Saharan Africa [...] [have] an emphasis on individual self-worth and empowerment—that is, a stressing of the God-given potential for earthly achievement present in each individual listener." Salt and Light, too, stresses the potential of the Christian belief to lead to earthly achievements, including the overcoming of hunger and poverty. Still, since the Toposa tribe does not in large parts identify as Christian, most of the missionary work focuses on teaching core Christian values that arguably most Christian denominations adhere to.

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[3]I note that this definition is in line with other influential accounts. For instance, Rowlands (1995, 102) defines empowerment as "bringing people who are outside the decision-making process into [...] political structures". Similarly, Blattman et al. (2014, 35) measure empowerment, i.a., as "self-reported social and community engagement".

4.3 Design

4.3.1 Setting

To explore the effects of Protestant missionaries on social capital, I draw on empirical evidence from South Sudan's Eastern Equatoria state. The state shares international borders with Kenya in the east, Uganda in the south, and Ethiopia in the north-east (see Figure 4.1a). The precise borders between South Sudan and Kenya, however, remain disputed. The capital of the state is Torit. The present study focuses on the area around the town of Kapoeta (see Figure 4.1b). The area is predominantly inhabited by the Toposa tribe, a Teso-Turkana subgroup of Eastern Nilotic peoples. The overall population of Toposa is estimated to be around 50,000-100,000 individuals, though precise numbers are not available.

The Toposa are commonly portrayed to hold animist beliefs (Longokwo, 1981; Roth and Kurup, 1990). This is despite the fact the tribe has not been spared from missionary activity. A history of evangelism in the Toposaland is hard to come by. Few scholarly accounts have systematically traced the evolution of Christianity in the region and in South Sudan more broadly. One rare account is provided by Sanderson (1976). The author studies the missionary history of the Sudan Interior Mission (see also, Kumm, 1918; Eshete, 1999). The Sudan Interior Mission, founded in the late 1890s in Canada, was the largest Protestant interdenominational mission in Africa at least until 1942 (Faught, 1988, 121). Sanderson points out the "very tight administrative control of missions which prevailed in the Sudan" to explain the relative ineffectiveness of missionaries (1976, 14). The Sudan Interior Mission made explicit attempts to proselytize "in the 'Open Sphere' in Equatoria." But, continues Sanderson (1976, 18), "the Government preferred more work to be done in the more 'backward' areas of Upper Nile province." The tight

restrictions also undermined missionary efforts by other groups. Sanderson writes:

> "*The Sudan Interior missionaries were to find the Sudan a particularly difficult country for missionary enterprise. So had the Church Missionary Society and the Roman Catholic Verona Fathers from the early years of Condominium rule. [...] They were not given freedom to go wherever they chose in the Sudan.*"
>
> Sanderson (1976, 13)

In my own interviews, I learned that the first missionaries came to the Toposaland in 1935, and remained there until 1962/3.[4] These missionaries were Italian Catholics of the *Comboni Mission.* Indeed, the area was officially designated to Catholicism (Sanderson, 1976, 17).[5] This came as a result of a ruling from the British authorities at the time. As Griswold (2010, 102) writes: "Shortly after forbidding missionaries from evangelizing among Muslims in the north [...], the British authorities divided the territory south [...] into three main spheres of Christian influence—Catholic, Anglican and American (Presbyterian)."

The Catholics in the Toposaland, by many accounts, took a mild approach to proselytism. As one interviewee relayed to me: "the Catholics baptized some people and built schools, but did not otherwise actively evangelize." In 1962, the Sudanese military junta expelled the Catholics along with all other foreign missionaries. As Tier (1982) documents "the government in 1962 expelled, at 48 hours' notice, about 335 foreign missionaries consisting of 272 Roman Catholic Verona and Mill Fathers, and 63 Protestants of the American Church Inland Mission and

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[4]It should be noted, however, that the Sudan, including Eastern Equatoria, had sporadically been visited by Catholic missionaries before (see, for example Gray, 1967).

[5]I confirmed this in a personal interview, where one missionary stated: "The Catholic church was the first one to be here. Somewhere back in the day [...] Southern Sudan was divided up into regions. And each different region got their own denomination. So the Toposa got Catholic, Roman Catholicism."

the Sudan Interior Mission."

In 1977, another group of Catholics—part of the Irish St Patricks Missionary Society—started a new mission in Eastern Equatoria. Yet, as the Catholics themselves write on their website "the Church here is still at its cradle" (Ihunnia, 2017). Longokwo (1981, 185), in one of the few careful anthropological studies of the Toposa, confirms that the impact of missionaries was very limited: "the whole missionary preoccupation was more numerical than qualitative" and attributes this to "the semi-nomadic and continuous movement of the Toposa in search for food and water for people and animals."

Attempts by Protestant missionaries in the region were limited as well. One group, part of the missionary organization *SIL International* (formerly known as the Summer Institute of Linguistics), came to a town north of Kapoeta and stayed for three years in the 1980s, before the ongoing civil war put an end to the missionary work. In additional interviews, interlocutors mentioned another group of Protestant missionaries, likely coming from Australia, that took root in the same area for a number of years in the early 1980s. I did not, however, come across documented evidence. And, local elders provided conflicting accounts on the activities of the group.[6]

Taken together, then, missionary activities among the Toposa have been relatively limited—particularly, when compared to other nations in Sub-Saharan Africa (see Gallego and Woodberry, 2010). Thus, concludes Griswold (2010, 102), "Christians never caught on as a cohesive identity in the south." Besides the aforementioned government interventions, there are two other reasons that help explain the relatively low activity of missionaries in the region. First, there were

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[6]I confirmed the scarce remnants of Protestantism pre-2005 in several interviews. For instance, one local missionary stated: "They [the Protestant missionaries] didn't leave a legacy behind."

two long civil wars between separatists in the South and the government in Khartoum (1955 to 1972; and 1983 to 2005), which made the area an unlikely target for missionaries. Second, writes Griswold (2010, 102), "the greatest impediment to Christianity's spread in the south was that the so-called border pagans were hostile to outsiders and their beliefs," which the author attributes to local tribes being accustomed to fending off slave traders.
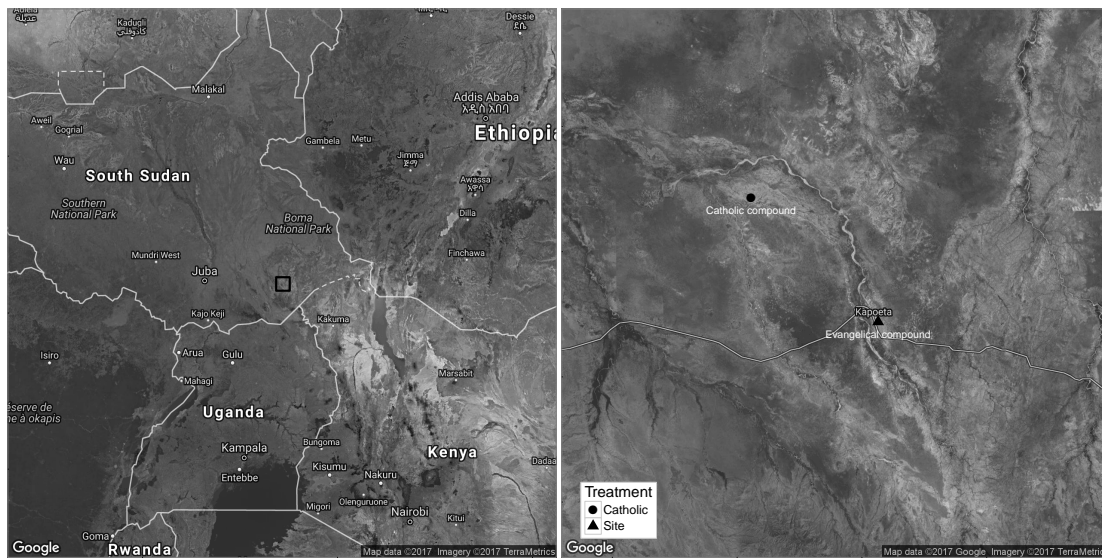
(a) Map of South Sudan and its bordering states   (b) Map of evangelism area around Kapoeta

Figure 4.1: Map of research area

Since the end of the second civil war in 2005, however, there has been a growing influx of missionary groups into the Equatoria region. Therefore, the Toposa, like many neighboring tribes, have been exposed to increasing attempts by Protestant missionaries to convert individuals to the Christian belief system. This makes the area a unique case study for the effects of missionaries on the social fabric of local villages.

In early 2013, I approached several local missionary groups about the prospects of conducting a research project on Christian proselytism in the area. I chose the area for two key reasons. First, as already stated, the area has witnessed

concerted efforts by several missionary groups to convert the local Toposa tribe to Christianity. While traditional foreign missionary work in Sub-Saharan Africa has been in decline ever since decolonialization went into full force (Killingray, 2011, 96), Eastern Equatoria continues to see a surprisingly active involvement of Christian proselytizers.

Second, while the rest of South Sudan descended into civil war in December 2013—a time when the research project was well under way—the Toposaland has, to this day, remained peaceful, though economic consequences can be felt. The reasons for this stability are manifold. In personal interviews, local residents pointed to the ethnic homogeneity of the region. Interlocutors also argued that the current civil war is, in essence, an ethnic conflict between the Dinka and Nuer tribes. Indeed, the civil war broke out when South Sudan's President Salva Kiir, a member of the Dinka tribe, accused his deputy Riek Machar, a member of the Nuer tribe, of attempting a coup d'état. The Toposa leadership has, time and again, stated its intention to abstain from the civil war. Finally, local residents highlighted that geographic factors make it unlikely for the Toposaland to be affected by the civil war. The area is tucked away in the southeastern corner of the country and protected by an impenetrable swamp area with no road access to the north.

4.3.2 Sample

During my first research visit to the region in the summer of 2014, I reached out to three missionary groups that are active in the region: the already-mentioned Roman Catholic St. Patricks Mission hailing from Ireland, a Southern Baptist missionary group from the United States, and a local Pentecostal missionary group

called *Salt and Light Outreach Church/Ministries* (henceforth, Salt and Light).[7] All groups signaled interest in my study. They subsequently agreed to share the geographic coordinates of the areas where they had proselytized.

Salt and Light's missionary efforts were particularly pronounced. I therefore scheduled several meetings with the church leadership to explore the potential of obtaining a list of active evangelism areas—Salt and Light calls them "preaching sections"—which they were planning on visiting in the next years. The group agreed to do so. They subsequently shared a list of 30 geographic sites with numerous villages. The sites cover a balanced area south, east, west and north of the town Kapoeta. I then inquired whether they were willing to allow me to conduct an independent study about the consequences of the evangelism. In particular, I asked whether they would be open to the idea of having 'a coin flip decide which villages to visit first, and which to visit thereafter.' Salt and Light's head minister appreciated the idea and agreed to the research project.

Based on the list of preaching areas, Salt and Light identified 60 villages that were to be evangelized, two per preaching area. The term "village" requires some explanation. The Toposa are semi-nomadic. During a typical day, young men take the cattle out into the vast open area of Eastern Equatoria. Older men typically sit under so-called shade trees and discuss important matters of the community (Müller-Dempf, 2009, 190). Women predominantly engage in agriculture, prepare food and take care of young children. Most families, however, have stable settlements. These settlements or "villages," typically consist of 20-30 huts. Several villages will then cover one "area." Given that there are no paved roads in the

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[7]To my knowledge, the three groups are the only ones actively involved in proselytism in the area. There are other churches active in the main town, Kapoeta. This includes a Baptist church founded by a Kenyan missionary as well as a Pentecostal church founded by a U.S. citizen. Both, according to my interviews, have roughly been active since 2010. These churches, however, do not evangelize in the Toposaland.

area, I was only able to visit 18 villages personally.[8]

4.3.3 Randomization

The 30 village pairs were combined in one comprehensive spreadsheet. Thereafter, as was laid out in the pre-registration document, the village pairs were put in random order. I then randomly assigned one village per pair to phase one, and the remaining village to phase two. This randomization scheme—known as a "waitlist design"—has the advantage of being relatively simple. It also ensures that exposure to the treatment is not withheld from any individual or village. Put differently, the empirical design merely changes the temporal sequence of the evangelism—a decision that I took for ethical reasons (more below). Moreover, by randomizing within blocks (pairs) of highly similar villages, I further ensure that unobserved difference between villages are minimized. Finally, by randomizing the order of the village pairs, I ensure that potential time trends are of minimal concern. This is important given that the evangelism took place over a 30-week span, and surveying took place two weeks after a village had been visited by the evangelists. Taken together, the empirical strategy thus relies on a mix of matching and random assignment in order to ascertain comparable treatment and control groups.[9]

4.3.4 Ethical Considerations

At this point, it is imperative to reflect on the ethics of conducting a randomized field experiment with a missionary group. A long literature has critically evaluated Christian missionaries and their effects on local communities and nation

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[8]An area photograph of the Toposaland and exemplary villages is given in Figure 4.7 in the Supplementary Information.

[9]The code used for this exercise is provided in the pre-analysis plan.

states. Scholars have found that missionaries of different denominations affect literacy (Graff, 1982), education (Lankina and Getachew, 2013), pro-social behavior (Caicedo, 2014) and democratization (Woodberry, 2012). Leaving aside whether these effects are desirable, they represent sufficiently stark social changes that researchers, such as myself, may not want to be involved in them. At the same time, if Protestant evangelism has such profound effects, the research and policy community arguably has an interest in obtaining reliable and up-to-date local-level evidence. Indeed, to my knowledge, all published articles discussing the consequences of proselytism have thus far relied on observational, historical data, most often spanning countries or even continents. This is problematic given that observational studies can fall victim to unobserved confounding. Moreover, we currently lack recent evidence about the consequences of today's evangelism. For this reason, I deemed the present project to be of sufficiently high scholarly relevance to justify cooperating with a missionary group.

I took three steps to reinforce my scholarly independence. First, I consistently communicated with the missionary group that the research project would adopt a critical scientific method and might produce results that the group might find undesirable. Second, as stated, I opted for a waitlist design, which merely changes the temporal sequence of the evangelism. This ensures that the research does not affect *whether* individuals are exposed to evangelism, but only *when* they are exposed. Third, I minimized my interaction with the treatment villages in order to stay out of the missionary work. Taken together, I thus believe the present study to be sufficiently critical and non-invasive in order to provide reliable estimates about the consequences of Protestant evangelism.

4.3.5 Evangelism

The explanatory variable of interest is the evangelism by Salt and Light in Eastern Equatoria. Salt and Light was officially registered with the Republic of South Sudan's Bureau of Religious Affairs in 2012 (see Figure 4.8 in the supplementary information). The group, however, has been active in the region since 2006. Salt and Light has a large church building in Kapoeta (see Figure 4.1b). The missionaries actively evangelize across numerous Toposa settlements in Eastern Equatoria. Evangelism for Salt and Light takes the form of preaching in local villages twice a week for a period of two to four weeks in the hopes that individuals will then actively participate in the church compound in Kapoeta and establish local Christian communities within the Toposaland.

For the purpose of this study, Salt and Light suggested to visit each community twice for two weeks, for an overall exposure of four visits. To minimize researcher interference with Salt and Light's activities, the research team did not accompany the missionary group. However, the evangelists did record several sermons in the local Toposa language. In personal interviews with the author, they also stated that the evangelism follows a rather standard procedure—though deviations are undoubtedly regular. According to these interviews, and corroborated by analyzing the recordings (available upon request from the author), Salt and Light's evangelism typically unfolds as follows.

In a first step, the evangelists—usually two South Sudanese trained theologians from the church's main compound in Kapoeta—visit a village and ask the elders for permission to tell stories from the Bible. If granted permission, the evangelists introduce themselves and schedule a time when to get the village together. Typically, this happens after dinner around 6 p.m. The evangelists then convene the village and tell key stories from the Bible. This includes the birth

of Jesus, miracles of Jesus, salient ethical preachings (e.g., the mountain prayer) as well as Jesus' crucifixion. Resources permitting, the evangelists show a Jesus film that was translated into the Toposa language. The evangelism also touches upon culturally sensitive topics such as polygamy and witchcraft. Indeed, an important element of the proselytism is the destruction of so-called witchcraft items (bracelets and necklaces). The evangelists also preach that believers should not drink alcohol and smoke tobacco. Baptisms are also frequently performed. Other elements of the evangelism include communal prayers as well as singing Christian hymns.

Using qualitative interviews with Salt and Light as well as the Southern Baptist missionary group, I delved into the logistics and content of the evangelism in greater detail. The strategy of both missions is to teach young evangelists who "take the word of God to the villages." These trainings have been going on for several years. As a results, both missions have a large group of evangelists. The following quote from the head minister of the Southern Baptist church illuminates this:

> "*We meet at our place [...] on Sundays for church. And how that's really come about is many churches from the villages will end up coming together on Sunday. So, they have church, they're meeting together at night in their areas. And on Sunday morning they're all kind of coming together, the ones that are fairly close. So, after worship time, I pull in all the leaders and I have about an hour of teaching. [...] We're about to the fourth or fifth generation of leaders training leaders. [...] I probably have like 75 people or so that are trained, that could go out and start groups. They don't all do it, but they have been trained.*"
> Head missionary of the Southern Baptist mission

Salt and Light has a distinct emphasis on biblical scripture. They also explicitly focus on changing the local culture toward Christianity. One evangelist, for instance, stated:

> "*There is all of these things that they [the Toposa] have been taught that are not Biblical. And so when I came here my big thing was 'I'm not here to change the culture' [...]. But God's word will eventually change the culture. And you have to make a decision one day to either to follow his word or continue with your culture, because what happens: the two collide. So you can't say 'I am a believer in Jesus,' and still kill people and raid cattle.*"

Broadly speaking, Salt and Light adheres to a literal form of Protestantism and self-identifies as Pentecostal. The scholarly discussion of Pentecostalism abounds (e.g., Miller and Yamamori, 2007). One definition is given by McClendon and Riedl (2015, 1047) who note that "Pentecostal churches in Sub-Saharan Africa [...] [have] an emphasis on individual self-worth and empowerment—that is, a stressing of the God-given potential for earthly achievement present in each individual listener." Salt and Light, too, stresses the potential of the Christian belief to lead to earthly achievements, including the overcoming of hunger and poverty. Still, since the Toposa tribe does not in large parts identify as Christian, most of the missionary work focuses on teaching core Christian values that arguably most Christian denominations adhere to.

4.3.6 Sampling

To assess the short-term effects of Salt and Light's evangelism on attitudes and behavior relating to social capital, I organized a village-level survey two weeks

after the final evangelism in a given village had taken place. The survey was then implemented in both the treatment and control village in a given village pair. I decided on two weeks for two key reasons. First, local residents migrate frequently in order to attend cattle and to do business, which poses a spillover risk. I therefore wanted to record outcomes sufficiently quickly in order to parse out treatment effects. At the same time, I wanted to rule out priming effects, which may have been present had enumerators visited village pairs too soon. Second, given the volatility the country was experiencing coupled with the logistical and financial challenges of conducting a survey in the region,[10] I had to keep to a swift schedule. Conducting the survey several months later would have posed additional logistical challenges. For instance, the enumerators relied on guidance by the missionaries about the exact whereabouts of the villages. Given the absence of official roads, the recollection of missionaries about the best way to get to villages would thus have been less reliable had surveying taking place at a significantly later point. The detailed schedule of the treatment and surveying activities is provided in the Supplementary Information.

The survey was implemented by a trained team of four local enumerators who were covered under Columbia University's Institutional Review Board. The enumerators underwent a rigorous training in Kapoeta about the proper conduct of human subjects research, including the administration of oral consent. They also administered a pre-test of the survey items among 70 respondents. This pre-test was pivotal as many items had to be adapted to the cultural context. For example, experimental games such as the trust game typically ask respondents to invest cash. The Toposa mostly barter goods and services. This made it necessary

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[10]It should be pointed out that the area under study has no electricity, scarce supply of oil and gas and very limited cellphone reception. Therefore, conducting surveys was both time-consuming and expensive. Moreover, standard techniques (such as collecting GPS coordinates) were impossible to implement due to budgetary restrictions.

to adapt the dictator game such that the investment amount was sugar, not cash.

At any given day, the enumerators were split into two teams and randomly assigned to the 30 villages. The enumerators were not told about the treatment status of the villages. Moreover, while the enumerators knew the project was largely about Christianity, I did not communicate specific hypotheses so as to avoid enumerator effects. As such I am reasonably confident that the measurement was accurate.[11] In each village, the enumerators first approached the elders and asked to perform a social scientific survey. They then surveyed all individuals above 18 years of age that were present and willing to participate. Roughly 20 percent of residents could not be reached. In any given village, roughly one in every 20 individuals did not want to participate in the survey. Surveys were conducted on hand-held phones. The data was pushed unto a password-protected cloud server using satellite internet.

4.3.7   Descriptive Statistics

The descriptive statistics of the sample are given in Table 4.1. The average age (*Age*) is 31 years and 53 percent of the sample are men (*Male*). 80 percent of individuals are married (*Married*) and individuals have an average of six children (*Number of children*). 53 percent of the sample are predominantly engaged in cattle herding (*Job: Cattle herder*), while 36 percent identify as farmers (*Job: Farmer*). The vast majority of respondents have no formal education (94 percent; *Edu: None*) and their ability to write, scored on a Likert-scale from 1 (very poorly) to 5 (very well), is, on average, 1.7 (*Ability to write*). Well over 80 percent answered

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[11]I should note, however, that I did not deceive subjects about the content of the survey. That is, it was communicated that the survey is, i.a., about Christianity and social capital. It was thus not a "seemingly unrelated" survey. This could mean that subjects connected the survey to the recent evangelism. I have no anecdotal evidence, however, that would corroborate this concern.

two algebraic questions correctly, which were administered in order to proxy for education (*Math ability 1* and *Math ability 2*). The average number of cows owned—the key indicator of status and wealth in Toposa society—is 43 (*Cows owned*).

4.3.8 Balance

Thanks to the random assignment of villages to the treatment phases, potential outcomes of respondents across treatment and control villages should, in statistical expectation, be identical. To test this assumption, Figure 4.2 plots t-values from t-tests assessing differences-in-means for the indicated variables across the treated and untreated villages, aggregating individual-level data at the village-level. As can be seen, no t-value exceeds the critical threshold of 1.96 (dotted vertical lines). Balance is thus solid across treatment and control areas. The same picture emerges when comparing the individual-level variables' means and standard deviations across the treatment and control samples in Table 4.1. The *F*-statistic of a regression of the treatment on all covariates is also insignificant (F-statistic: 1.01). The balance allows me to attribute any observed differences across the villages to the evangelism of the mission.

4.3.9 Measurement: Cultural Beliefs and Practices

The first outcome of interest is whether the missionaries do, indeed, affect local traditions and beliefs. I included ten survey items that measure core cultural traditions and beliefs. They are reported in Table 4.2. In the Supplementary Information, I report the same table aggregating variables at the village-level (Table 4.10). First, the enumerators unobtrusively counted the number of witchcraft items respondents were wearing (*Witchcraft items*). The outcome is, thus, be-

Table 4.1: Descriptive statistics of sample

		Overall sample		Treatment group		Control group	
	N	Mean	SD	Mean	SD	Mean	SD
Age (#)	641	31.4	8.9	31.2	8.7	31.5	9.0
Male	641	52.6	50.0	51.0	50.1	54.1	49.9
Single	641	9.2	28.9	9.4	29.2	9.1	28.8
Relationship	641	4.2	20.1	6.1	24.0	2.4	15.4
Married	641	79.9	40.1	77.7	41.7	81.9	38.6
Widowed	641	6.4	24.5	6.8	25.2	6.0	23.9
Number of children (#)	641	5.6	3.9	5.5	3.9	5.6	4.0
Job: Cattle herder	641	52.9	50.0	53.9	49.9	52.0	50.0
Job: Farmer	641	36.3	48.1	33.9	47.4	38.7	48.8
Job: Stays home	641	5.0	21.8	5.2	22.2	4.8	21.5
Unemployed	641	5.1	22.1	6.5	24.6	3.9	19.5
Edu: None	641	94.1	23.6	93.2	25.2	94.9	22.1
Edu: Elementary	641	5.5	22.7	6.5	24.6	4.5	20.8
Edu: Secondary	641	0.5	6.8	0.3	5.7	0.6	7.8
Edu: High school	641	0.0	0.0	0.0	0.0	0.0	0.0
Edu: University	641	0.0	0.0	0.0	0.0	0.0	0.0
Math ability 1	641	87.2	33.4	88.1	32.5	86.4	34.3
Math ability 2	641	84.9	35.9	84.5	36.2	85.2	35.6
Ability to write (1-5)	641	1.7	0.6	1.7	0.6	1.7	0.6
Cows owned (#)	641	42.9	29.4	42.5	29.3	43.3	29.5

Notes: Mean (Mean) and standard deviations (SD) of all outcome measures across the overall sample as well as treatment and control groups.

Figure 4.2: Pre-treatment balance
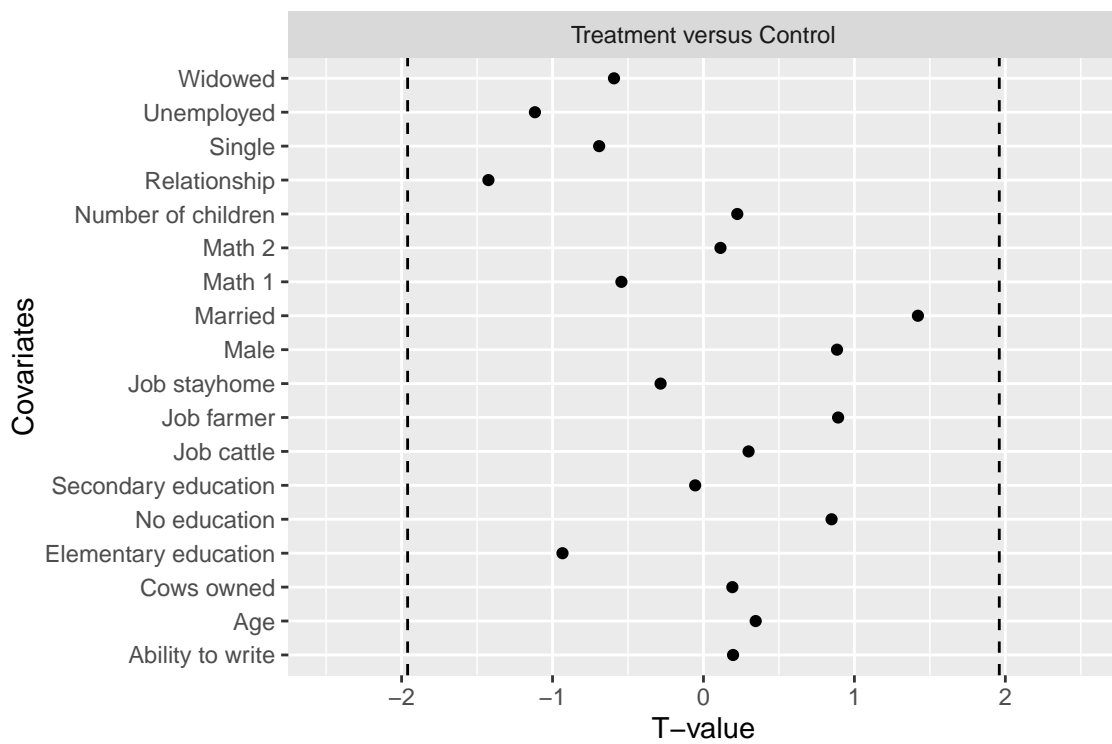
Notes: The figure plots t-values from t-tests assessing differences in means across treated and control villages for the indicated variables. Data aggregated at the village-level to reflect clustered treatment assignment.

havioral and as such unaffected by social desirability bias. Witchcraft is openly opposed by Salt and Light and other missionary groups in the area. Second, respondents were asked how often they consumed tobacco (*Tobacco*), assessed on a 4-point scale ranging from never to often. Salt and Light encourages believers to not consume tobacco. Third, respondents were asked about their alcohol consumption assessed on the the same 4-point scale (*Alcohol*). Alcohol, too, is opposed by the missionary group. Fourth, respondents were asked how often they had visited the witch doctor in the last year (*Witch doctor*). Again, Salt and Light preaches that individuals not consult the witch doctor—a common practice among the Toposa. Fifth, respondents were asked how often they pray per week (*Praying times*). Sixth, respondents were asked whether they believed in so-called minor spirits (*Minor spirits*). These spirits are an important part of the Toposa religion. While the Toposa believe in an abstract superior being, they also hold ancestral spirits in high regard. These so-called minor spirits are not part of the Christian preaching and thus opposed by missionary groups in the area, including Salt and Light. Seventh, respondents were asked to what extent they agreed that young Toposa men and women should participate in the traditional tooth extraction (*Tooth extraction*), scored on a 5-point scale (strongly disagree to strongly agree). The tooth extraction tradition is part of a rite of passage into adulthood among the Toposa and many neighboring tribes (Willis, Harris and Hergenrader, 2008). Eighth, respondents were asked to what extent they agreed that men and women should be allowed to have sex before marriage (*Sex before marriage*), scored on the same 5-point scale. Sex before marriage is strongly opposed by the missionary group. Ninth, respondents were asked to what extent they agreed that the practice of dowry should be abolished (*Dowry*), scored on the same 5-point scale. The practice of dowry among the Toposa merits discussion. Müller-Dempf (2009,

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194) writes that among the Toposa "old men often, instead of allocating animals to their sons as a dowry so that they can marry, prefer to use these animals for their own convenience, i.e. to marry another young wife." In order to establish stable and monogamous marriages, the missionaries therefore argue against the practice of dowry. Tenth, respondents were asked whether men should only have one wife (*Polygamy*), scored on the same 5-point scale. Polygamy, too, is opposed by most missionary groups, including Salt and Light (Longokwo, 1981, 186). I combine the ten variables to a comprehensive *Tradition Index* by standardizing all variables and averaging across them. Variables are coded such that greater values reflect greater agreement with traditional Toposa traditions and beliefs.[12] I use this index in order to address multiple comparisons concerns. The intercorrelations of the index are shown in Figure 4.9 in the Supplementary Information.

4.3.10   Measurement: Knowledge about Christianity

The second outcome of interest is whether the missionaries succeed in educating people about Christianity. I included three survey items that measure individuals' knowledge about Christianity. They are reported in Table 4.2. In the Supplementary Information I report the same table aggregating variables at the village-level (Table 4.10). First, the enumerators asked individuals to state what the holy trinity meant and recorded the answer by hand (*Trinity knowledge*). Knowledge for this item was very low (1.7 percent). Second, the respondents were asked about Pentecost, giving them four answer choices ("Jesus went up from the grave"; "The disciples received the holy spirit"; "Jesus walked on water"; "The last supper"). On average, 76 percent of individuals knew the correct answer choice (number 2). Third, respondents were asked how many disciples Jesus had had, again recording

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[12]Specifically, the variable *Praying times*, *Dowry*, and *Monogamy* were coded in reverse.

the answer open-ended. On average, 71 percent of individuals knew the correct answer (12). I combine the three variables to a comprehensive *Christianity Index* by standardizing all variables and averaging across them. Variables are coded such that greater values reflect greater knowledge of Christianity. The intercorrelations of the index are shown in Figure 4.10 in the Supplementary Information.

4.3.11   Measurement: Social Capital

My prime outcome of interest is social capital within the evangelized and to-be-evangelized villages. Based on Woolcock (1998) and Coleman (1988), I conceptualize social capital along six dimensions: networks, trust, cooperation, information, cohesion and empowerment. Using a widely accepted and frequently adopted definition of social capital has the key advantage that results can be cross-checked against existing studies. Again, I combine these six dimensions of social capital to a comprehensive index by standardizing the variables and averaging across them. All six dimensions were measured using one behavioral and one attitudinal outcome.

I measure *networks* as follows. First, we asked respondents to write down all of their friends and then counted the number of names (*Total friends*). Second, we asked respondents how many close friends they have (*Close friends*). I measure *trust*, again, using one behavioral and one attitudinal item. First, the enumerators administered the well-established trust game (Berg, Dickhaut and McCabe, 1995). Here, respondents were given 200g of sugar and asked whether they would be willing to send any amount to the next respondent. The respondent was informed that whatever amount s/he sends will be doubled by the experimenter and that the next respondent would be asked how much money to send back. The variable *Trust amount* depicts the amount invested (in approximate grams). Second, we

Table 4.2: Outcome measurement

	Min	Max	Overall sample			Treatment group			Control group		
			N	*Mean*	*SD*	*N*	*Mean*	*SD*	*N*	*Mean*	*SD*
Cultural outcomes											
Witchcraft items (#)	0	12	638	2.4	2.2	307	2.0	2.1	331	2.8	2.2
Tobacco	1	4	641	2.8	1.3	310	2.8	1.4	331	2.9	1.3
Alcohol	1	4	640	1.9	0.9	309	1.7	0.8	331	2.0	0.9
Witch doctor (#)	0	100	641	12.2	17.3	310	9.8	16.7	331	14.3	17.6
Praying times (#)	0	30	641	3.6	4.2	310	4.0	4.5	331	3.1	3.8
Minor spirits (%)	0	1	641	93.0	25.6	310	90.6	29.2	331	95.2	21.5
Tooth extraction	1	5	641	3.3	1.1	310	3.3	1.0	331	3.3	1.1
Sex before marriage	1	5	641	2.5	1.3	310	2.4	1.2	331	2.6	1.3
Dowry	1	5	641	1.2	0.7	310	1.3	0.7	331	1.2	0.6
Polygamy	1	5	641	1.7	1.0	310	1.9	1.0	331	1.5	0.8
Tradition Index	-1	1	637	0.0	0.4	306	-0.1	0.4	331	0.1	0.4
Christian knowledge outcomes											
Trinity knowledge	0	1	641	1.7	13.0	310	1.9	13.8	331	1.5	12.2
Pentecost knowledge	0	1	641	75.7	42.9	310	81.9	38.5	331	69.8	46.0
Disciples knowledge	0	1	641	71.3	45.3	310	77.7	41.7	331	65.3	47.7
Christianity index	-1	3	641	0.0	70.8	310	10.2	65.5	331	-9.5	74.2
Social capital outcomes											
Total friends (#)	1	11	641	4.6	2.2	310	4.5	2.1	331	4.7	2.2
Close friends (#)	0	30	641	7.8	7.4	310	7.2	7.2	331	8.4	7.5
Trust amount (g)	0	200	641	91.2	34.5	310	94.4	29.8	331	88.3	38.3
General trust	1	5	641	2.1	0.9	310	2.1	0.9	331	2.2	0.9
Cooperation signature	1	5	641	3.5	0.5	310	3.6	0.5	331	3.5	0.5
Cooperation likelihood	1	5	641	4.7	0.6	310	4.8	0.5	331	4.7	0.7
Information time (10s)	1	220	638	12.5	18.1	307	10.2	9.9	331	14.6	23.1
Information strangers (#)	0	20	641	11.2	6.9	310	11.1	7.0	331	11.3	6.8
Finding opponent (%)	0	1	641	69.1	46.2	310	74.5	43.6	331	64.0	48.1
Safety perception	1	5	641	4.7	0.6	310	4.6	0.7	331	4.7	0.6
Change proposal (10s)	0	1248	638	13.0	84.1	307	18.0	114.0	331	8.3	39.4
Change petitioning	1	4	641	3.0	1.0	310	2.9	1.0	331	3.0	1.0
Social Capital Index	-1	2	638	0.0	0.4	307	0.0	0.4	331	-0.0	0.4

Notes: Mean (*Mean*) and standard deviations (*SD*) of all outcomes across the overall sample as well as treatment and control groups. *Min* and *Max* depicts minimum and maximum values in the overall sample.

asked respondents to what extent they agreed with the statement "most people can be trusted," scored on the already-mentioned 5-point agreement scale (*General trust*). I measure *cooperation* as follows. First, individuals were asked whether they would be willing to sign a petition that calls on the Toposa to establish a regular meeting in Kapoeta to discuss matters relating to security (*Cooperation signature*). Since literacy is very low among the Toposa and to streamline the surveying, enumerators were instructed to judge respondents' perceived willingness to sign the petition. We scored this on a scale from 1 (definitely not) to 5 (definitely yes).[13] Second, we asked respondents how likely it would be for the community to come together and cooperate in case a child got sick. Here, respondents were given a 5-point scale ranging from extremely unlikely to extremely likely (*Cooperation likelihood*). I measure *information* as follows. First, individuals were asked to find an individual that knew how many individuals lived in the given village. When respondents already knew the answer, we asked them to nonetheless confirm the information with a reliable source. Using the survey software's built-in tool, we recorded the amount of time it took respondents to obtain this information (*Information time*). Second, we asked respondents how often, in the past month, they had talked to strangers to obtain information (*Information strangers*). I measure *cohesion* as follows. First, respondents were, toward the end of the survey, asked to find a person that they are frequently in disagreement with. Again, individuals were timed how long it took them (*Finding opponent*). Second, we asked respondents how safe they felt in their villages. The answer choices, on a 5-point scale, ranged from unsafe to safe (*Safety perception*). Finally, I measure *empowerment* as

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[13]For instance, respondents asking for a pen were given a 5, while those giving answer such as "in principle, yes" were given a 4. During the pre-test, I tested this procedure by having respondents sign a petition (in the town of Kapoeta, where literacy is higher) and asking enumerators to still judge ex ante whether a person would, indeed, sign. Since enumerators did an excellent job at guessing whether respondents would sign the petition, I opted to drop the physical signature part form the survey.

follows. First, respondents were asked to state a list of changes they felt needed to be implemented. Enumerators recorded the amount of time individuals discussed these matters with the enumerator (*Change proposal*). Second, respondents were asked how often, in the last 12 months, people had come together to petition government officials for the benefit of the community (*Change petitioning*). Again, to circumvent over-testing, I combine all 12 items to a *Social Capital Index* by standardizing individual items and averaging across them. The intercorrelations of the index are shown in Figure 4.11 in the Supplementary Information.

4.4 Results

To assess whether Protestant missionaries affect social capital, I estimate the following model:

$$Y_{il} = \beta_0 + \beta_1 Mission_{il} + \mathbf{X}_{il}\beta + \epsilon_{il} \tag{4.1}$$

Where Y_{il} represents the outcome of individual i in village l, *Mission* is a dummy taking the value one if the village was assigned to be evangelized[14] and zero otherwise, and $\mathbf{X}_{il}$ represents a vector of individual-level control variables unaffected by the treatment, which are added in select robustness checks. Specifically, I include all variables listed in Table 4.1 save those with no variation (i.e., edu: secondary, edu: high school, and edu: university). In line with Cameron, Gelbach and Miller (2008), I estimate standard errors using the wild cluster bootstrap-t procedure, which takes into consideration the clustered treatment assignment. Additionally, I conduct clustered randomization inference as a robustness check for they key outcomes, namely, the social capital and tradition indexes.

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[14]It should be noted that the experiment had full compliance.

Table 4.3: Treatment effects on cultural outcomes (no covariates)

	Witchcraft	Tobacco	Alcohol	Witch doctor	Prayer	Minor spirit	Teeth trad.	Sex	Dowry	Mono-gamy	**Index**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Mission	−0.780	−0.161	−0.303	−4.509	0.902	−0.045	−0.019	−0.153	0.103	0.360	−0.164
	(0.311)	(0.177)	(0.097)	(2.014)	(0.465)	(0.031)	(0.137)	(0.156)	(0.064)	(0.159)	(0.045)
Range	cont.	1-4	1-4	cont.	cont.	binary	1-5	1-5	1-5	1-5	-1-1
R^2	0.032	0.004	0.028	0.017	0.011	0.008	0.0001	0.004	0.006	0.036	0.049
N	638	641	640	641	641	641	641	641	641	641	637

Notes: OLS. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages). Individual-level covariates not included.

Table 4.4: Treatment effects on cultural outcomes (covariate adjusted)

	Witchcraft	Tobacco	Alcohol	Witch doctor	Prayer	Minor spirit	Teeth trad.	Sex	Dowry	Mono-gamy	**Index**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Mission	−0.787	−0.140	−0.303	−4.647	0.872	−0.039	0.004	−0.140	0.077	0.329	−0.152
	(0.253)	(0.127)	(0.084)	(1.857)	(0.414)	(0.024)	(0.093)	(0.105)	(0.061)	(0.131)	(0.034)
Range	cont.	1-4	1-4	cont.	cont.	binary	1-5	1-5	1-5	1-5	-1-1
R^2	0.231	0.245	0.131	0.159	0.186	0.124	0.224	0.232	0.124	0.199	0.269
N	638	641	641	641	641	641	641	641	641	641	637

Notes: OLS. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages). Individual-level covariates included (notably, all variables in 4.1 except Edu: Secondary, Edu: High school, and Edu: University).

4.4.1 Cultural Beliefs and Practices

Before turning to this chapter's main outcome of interest, social capital, I first analyze whether the missionaries affected attitudes toward cultural outcomes that they are primarily concerned with. Table 4.3 reports results from a linear regression of the indicated outcomes on the treatment dummy. The tradition index (Model 11) experiences a sizable reduction as a result of the missionary evangelism. Specifically, the index is 0.2 points lower in treated villages. The reduction thus demonstrates that missionaries profoundly affect cultural beliefs and practices.

When looking at the subcomponents of the index (models 1-10), all indicated estimates point in the expected direction. Most estimates are sizable and statistically significant. Above all, enumerators count 0.8 fewer witchcraft items among respondents in evangelized villages (model 1). While alcohol and tobacco are only mildly affected (models 2 and 3, respectively), weekly praying times increase by 0.9 (model 5). Traditional cultural practices such as tooth extraction, dowry and polygamy, too, are less likely to be accepted in evangelized villages (models 6, 9 and 10, respectively). The highly consistent effects and plausible effect sizes thus underline the profound consequences of the evangelism. All results are robust to the inclusion of individual-level covariates (Table 4.4). I also confirm the results when using clustered randomization inference (Figure 4.3). Here, I obtain a one-sided p-value of 0.01.

4.4.2 Christian knowledge

In a next step, I assess whether missionaries succeed in bringing people "the good news." In Table 4.5,I report results from a linear regression using OLS of the indicated outcomes on the treatment indicator. Overall, the Christianity index (Model 4) sees a strong reduction owing to the missionary activity. The index

Table 4.5: Treatment effects on Christian knowledge (no covariates)

	Trinity	Pentecost	Disciples	**Index**
	(1)	(2)	(3)	(4)
Mission	0.004	0.121	0.125	0.197
	0.010	0.043	0.047	0.065
Range	binary	binary	binary	-1-1
R^2	0.0003	0.020	0.019	0.019
N	641	641	641	641

Notes: OLS. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages). Covariates not included.

Table 4.6: Treatment effects on Christian knowledge (covariate adjusted)

	Trinity	Pentecost	Disciples	**Index**
	(1)	(2)	(3)	(4)
Mission	0.005	0.123	0.128	0.203
	0.009	0.036	0.034	0.052
Range	binary	binary	binary	-1-1
R^2	0.033	0.219	0.219	0.221
N	641	641	641	641

Notes: OLS. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages). Covariates included.

is 0.2 points higher in treated villages. This demonstrates that the evangelists succeeded in fostering Christian knowledge within their area of proselytism.

The subcomponents of the index largely confirm this picture (models 1-3). All estimates are positive, i.e., knowledge increased after the missionary intervention. The estimates are particularly sizable for knowledge about Pentecost as well as Jesus' disciples. The increase is small, though positive, for the Trinity question. On average, individuals are 12 percentage points more likely to know what happened on Pentecost in treated villages. Moreover, individuals in treated villages are also 12 percentage points more likely to know the number of disciples Jesus had. All results are robust to the inclusion of individual-level covariates (Table 4.6). I further corroborate the results by using clustered randomization inference (Figure 4.3). Here, I obtain a one-sided p-value of 0.07.

Figure 4.3: Effect on tradition index (randomization inference)
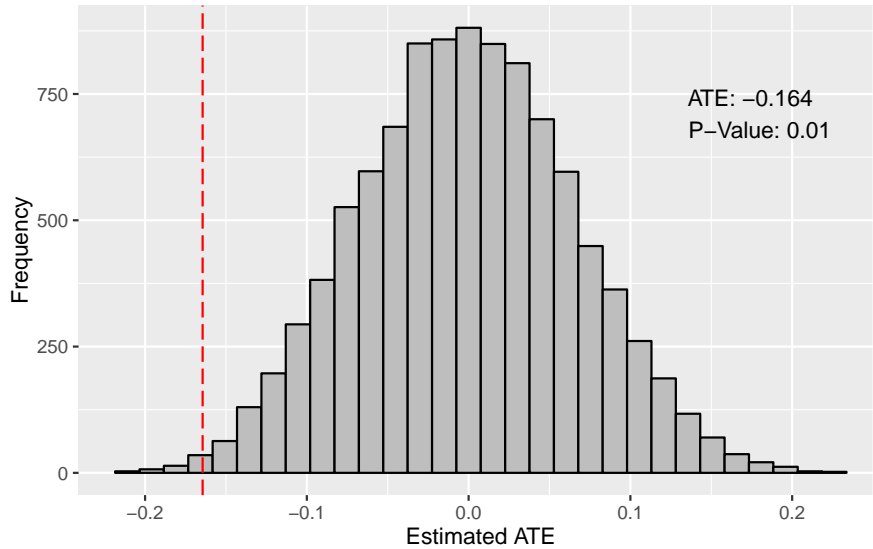
Notes: Estimated average treatment effect (ATE) of the evangelism intervention on the tradition index and corresponding one-sided p-value using clustered randomization inference under 10,000 simulations.

Figure 4.4: Effect on Christianity index (randomization inference)
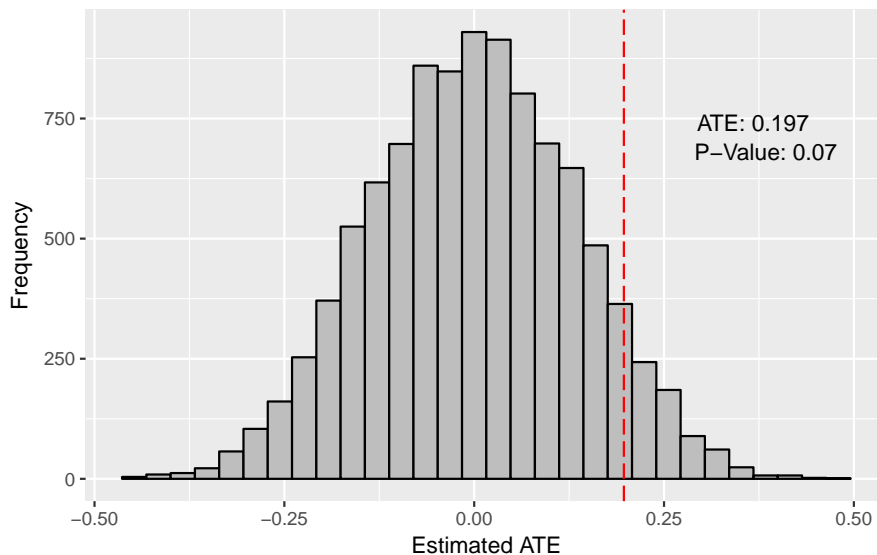
Notes: Estimated average treatment effect (ATE) of the evangelism intervention on the Christianity index and corresponding one-sided p-value using clustered randomization inference under 10,000 simulations.

4.4.3 Social Capital

Do missionaries affect social capital after a relatively short intervention of two weeks? Table 4.7 reports results from a linear regression of the indicated social capital outcomes on the treatment dummy. As can be seen, the evidence is largely inconclusive. The comprehensive social capital index is increased by 0.02 points as a result of the missionary intervention, but the effect size is small and not significant. The individual sub-components paint a similarly inconsistent picture. Specifically, I estimate a reduction in networks (both total friends and close friends; models 1 and 2, respectively) that is in line with my first hypothesis. But, both estimates are noisy. All other measures yield inconsistent findings that do not confirm the pre-registered hypotheses that were based on my qualitative interviews. While evangelized villages invest significantly more in the trust game (6.1 grams of sugar; model 3), they express lower levels of generalized trust (insignificant; model 4). Cooperation is slightly higher in evangelized villages—both statements about cooperation likelihood as well as perceived willingness to sign a cooperation petition (models 5 and 6, respectively). Again, however, estimates are not significant. Information yields an ambivalent picture as well. In evangelized villages, individuals take significantly less time to obtain information from strangers (model 7). Yet, they also report that they, in general, speak to strangers less frequently (model 8). Similarly, respondents in evangelized villages find opponents more quickly (model 9), but express less trust in the safety of the villages (model 10). Both estimates, however, are small and insignificant. Finally, respondents in evangelized villages speak longer about possible changes for the village (model 11), but report less petitioning on behalf of the village (model 12). I confirm the overall insignificant treatment effect on the aggregate social capital index in Figure 4.5 using clustered randomization inference.

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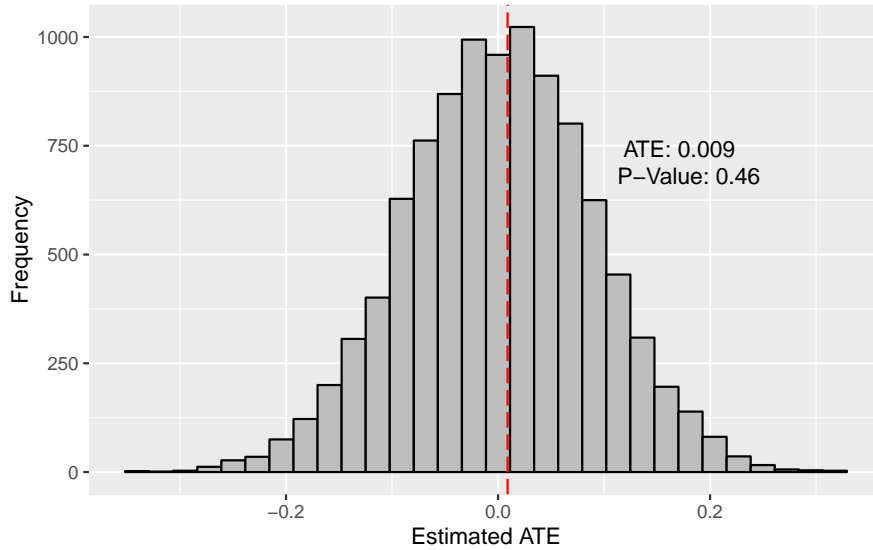Figure 4.5: Effect on social capital index (randomization inference)

Notes: Estimated average treatment effect (ATE) of the evangelism intervention on the social capital index and corresponding one-sided p-value using clustered randomization inference under 10,000 simulations.

In sum, then, I document inconsistent evidence regarding the effects of social capital. Several reasons for this theoretically inconsistent and noisy result come to mind. First, the intervention was mild (two weeks) and may have done little to change the social fabric of villages. This explanation, however, is not entirely convincing. After all, I find a sizable treatment effect on cultural beliefs and traditions—outcomes that are arguably as difficult to move. Second and related, it could be the case that outcomes were collected too soon after the intervention, leaving too little time for social changes to unfold. The fact that evangelized villages report significantly fewer friends speaks against this explanation, though it is largely untestable. Third, it could be the case that the experiment is under-powered. Here, it should be noted that the present chapter reports on the current status quo of the experiment. 22 villages remain to be treated. Still, however, since villages were put in random order, there is little reason to believe that effect

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sizes should change, though uncertainty may be reduced thanks to a larger number of clusters.

Table 4.7: Treatment effects on social capital (no covariates)

	Networks		Trust		Cooperation		Information		Cohesion		Empowerment		Index
	Total friends	Close friends	Trust amount	General trust	Cooperation signature	Cooperation likelihood	Information time	Information strangers	Finding opponent	Safety perception	Change proposal	Petitioning	Social Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Mission	−0.190	−1.146	6.125	−0.051	0.075	0.098	−4.427	−0.259	0.105	−0.090	9.658	−0.067	0.009
	(0.359)	(1.025)	(3.870)	(0.082)	(0.075)	(0.092)	(2.357)	(1.181)	(0.070)	(0.102)	(8.534)	(0.114)	(0.062)
Range	cont.	cont.	cont.	1-5	1-5	1-5	cont.	cont.	binary	1-5	cont.	1-4	1-2
R²	0.002	0.006	0.008	0.001	0.005	0.007	0.015	0.0004	0.013	0.005	0.003	0.001	0.0001
N	641	641	641	641	641	641	638	641	641	641	638	641	638

Notes: OLS. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages). Individual-level covariates not included.

Table 4.8: Treatment effects on social capital (covariate adjusted)

	Networks		Trust		Cooperation		Information		Cohesion		Empowerment		Index
	Total friends	Close friends	Trust amount	General trust	Cooperation signature	Cooperation likelihood	Information time	Information strangers	Finding opponent	Safety perception	Change proposal	Petitioning	Social Capital
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Mission	−0.132	−1.153	7.000	−0.057	0.072	0.121	−4.229	−0.111	0.095	−0.066	9.841	−0.038	0.026
	0.224	0.682	3.245	0.077	0.046	0.067	2.141	0.791	0.064	0.066	8.137	0.082	0.040
Range	cont.	cont.	cont.	1-5	1-5	1-5	cont.	cont.	binary	1-5	cont.	1-4	1-2
R²	0.345	0.302	0.062	0.135	0.206	0.175	0.093	0.273	0.125	0.283	0.023	0.260	0.259
N	641	641	641	641	641	641	638	641	641	641	638	641	638

Notes: OLS. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages). Individual-level covariates included (notably, all variables in 4.1 except Edu: Secondary, Edu: High school, and Edu: University).

4.4.4 Ex Post Analysis

An alternative explanation is that the missionaries affected social capital in ways I had not hypothesized. Indeed, upon closer analysis, the results are in line with a theory of Protestant missionaries and social capital that sees group-level social capital weakened, while individual-level pro-social behavior is strengthened. To see this, note that there are negative treatment effects for all variables that depict village-level social capital outcomes (i.e., models 1, 2, 4, 6, 8, 10 and 12). Specifically, individuals describe a sparser network (models 1 and 2), lower generalized trust (model 4), report a lower incidence of seeking out to strangers (model 8), perceive the village as less safe (model 10) and witness less petitioning on behalf of the village (model 12). All estimates are negative, though they remain noisy. And, only one measure describing village-level social capital is positive, namely, cooperation likelihood (model 6). By contrast, I find positive effect sizes for all individual-level social capital measures (notably, models 3, 5, 7, 9 and 11), particularly those that measure behavior. Again, these estimates are noisy, but are consistently positive. I confirm this finding again using clustered randomization inference (see Figure 4.6).

Table 4.9: Treatment effects on individual- and group-level social capital

	Group-level social capital index	Individual-level social capital index
	(1)	(2)
Mission	−0.054	0.094
	(0.039)	(0.043)
N	641	638

Notes: OLS regressions. Wild cluster bootstrap-t SEs in parentheses (adjusted for 38 villages).

When combining the variables to an individual-level social capital index and

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a group-level social capital index, I detect sizable and significant effect sizes (see Table 4.9). Specifically, the individual-level index is increased by 0.1 points, while the group-level index is reduced by 0.1 points. It is important to point out, however, that this analysis represents a stereotypical case of what Gelman and Loken (2013) term "forking paths." That is, since this analysis was not pre-registered, the uncertainty around the estimates may not accurately reflect the "researchers degrees of freedom," which this analysis draws on.

Still, the analysis points to two forces that are at work when missionaries evangelize, which are laid out in Chapter 3: preferences and networks.

On one hand, evangelists may reduce aggregate social capital by intruding in local networks. The fact that I find significant effects on traditions, beliefs, and Christian knowledge showcases the profound impact of the evangelists on the social fabric of the villages. Research in the social sciences more broadly has confirmed the sustained effects of religious organizations on local networks (e.g. Galbraith, Rodriguez and Stiles, 2007). A popular view links religious and other ethnic groups to "clubs," which redistribute public goods within clearly defined borders (Huber and Stanig, 2011; Scheve, Stasavage et al., 2006). Notably, as outlined in Chapter 3, Berman (2000) argues that religiosity is a signaling device to a religious community, which provides much-needed insurance to its members. In Sub-Saharan Africa, McCauley (2012) argues that missionaries strive to establish new patronage-like relationships. These, by necessity, cut through existing social networks and power structures.

The Toposa tribe is governed by generation sets (Müller-Dempf, 2009), not one chief (Baldwin, 2015). Yet, the message of "Christian brotherhood" and "equality among all men" still presents a challenge to the elites in charge. The missionaries therefore attempt to convert elders and to persuade them to join the church at

an early stage. But, anecdotal evidence from my own field work suggests that the missionaries are most successful among younger individuals. Indeed, certain elders depicted the evangelization as "story telling for children." This depiction may be overly critical. But it shows that elders view the missionaries with skepticism. They fear that their influence might be undermined, and worry that the carefully balanced networks and power structures are put at risk. As a result, it is thus not surprising, and in line with my hypotheses, that the evangelists lead to seemingly sparser networks (Models 1 and 2 in Tables 4.7 and 4.8), lower safety perceptions (Model 10), less pronounced village-wide petitioning (Model 12), and lower *generalized* trust (Model 4). Individuals in evangelized villages also seemingly have a harder time to obtain information (Models 4 and 8).

These effects are surprising inasmuch as the intervention was rather short. It should be noted, however, that I rely on survey evidence, which may be influenced by perceptions of individuals. And perceptions were undoubtedly affected by the evangelizers. As much is detectible from the pronounced effects on cultural beliefs and practices as well as Christian knowledge. Thus, the intervention may have first changed perceptions (e.g., who is and who is not a friend), which explain changes in outcomes that one would typically assume to take longer than four weeks.

On the other hand, the preaching of the evangelists is distinctly pro-tolerance. While I had hypothesized that the evangelism has a detrimental effect on overall trust, I noted that the Toposa evangelists, in line with other Protestant missionaries, are strong proponents of social justice. After all, in the words of Little (2005, 207-208), the "Protestant missionary ethos originally focused on [...] charity." Cross-country studies also confirm a positive association between Protestantism and trust (Bjørnskov, 2007; Delhey and Newton, 2005). Moreover, missionaries,

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including the Salt and Light evangelizers, preach the value of cooperation, particularly between rivaling tribes. This is particularly timely among the Toposa tribe, which has had a long-standing rivalry with the neighboring Turkana tribe. This rivalry plays itself out in frequent and deadly cattle raids (Hendrickson, Mearns and Armon, 1996). The Salt and Light evangelists thus emphasize the shared human bond between the two tribes. They specifically encourage intra- and inter-tribal cooperation. And, they strive to reconcile tribal differences by facilitating meetings between the two groups. Taken together, the missionary emphasis on cooperation, trust and reconciliation may thus help explain the positive finding regarding trusting behavior (Model 3 in Tables 4.7 and 4.8), the propensity to cooperate (Model 5) and the willingness to find an on opponent (Model 9) as well as to propose change (Model 11).

With this finding in hand, it may also be worthwhile to reflect on the theology of the Salt and Light missionaries. In Chapter 3, I found that, on average, Protestant missions reduce community cohesion. Importantly, however, I found the result was particularly strong among Pentecostal groups. By contrast, the Adventist mission—a rather all-encompassing religious group with little focus on individualism—showed positive effects on pro-social preferences. While Salt and Light considers itself Pentecostal, my own time in the field displayed them as a rather out-going, universalist group. This may be owing to the fact that they need to adopt their theology to the local conditions. More importantly, however, it means that the individualistic element of Protestantism, which is typically hypothesized to be particularly salient among Pentecostals, may not matter as decisively among Salt and Light's evangelizers. Interpreted in this light, the positive effects on trust, cooperation and reconciliation is thus in line with Chapter 3. Namely, that a reduction in pro-social preferences is noticeable among strict Pentecostal

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groups, while the opposite can be said about more universalist missions. Above all, then, the result showcases that there is significant variation in the relationship between Protestant missions, community cohesion and social capital.

Figure 4.6: Effects on individual- and group-level social capital

(a) Village-level index

(b) Individual-level index
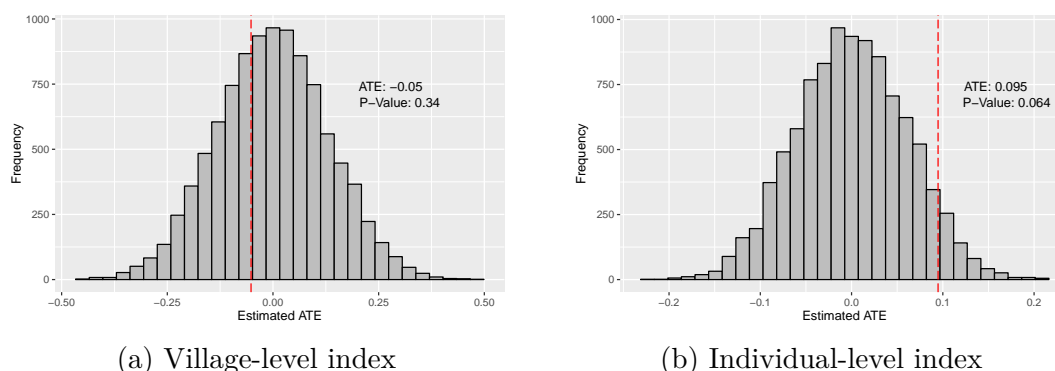
Notes: Estimated average treatment effect (ATE) of the evangelism intervention on indicated social capital sub-indexes and corresponding one-sided p-value using clustered randomization inference under 10,000 simulations.

4.5 Conclusion

This chapter has added to a growing literature on the effects of Protestant missionaries on social capital. Using a field experiment implemented in cooperation with a missionary group in South Sudan, I found that Protestant evangelism has a stark effect on attitudes and toward local cultural practices. Effects on social capital were not found when scrutinizing an aggregate, pre-registered social capital index. When assessing the components of the index, however, I documented two noteworthy effects. Group-level indicators of social capital were found to be lower, while individual-level indicators of social capital were shown to be higher in treated villages compared to control villages. I interpreted the findings to imply that missionaries work akin to a network interruption. They undermine structural

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determinants of social capital, but increase individual-level pro-social behavior.

By providing, for the first time, causal estimates about the effects of Protestant missionaries the present chapter is a significant addition to the heretofore observational literature. Still, four key caveats must not go unnoticed. First of all, one should be cautious to extrapolate these findings to other contexts. The Toposaland, as was discussed, has a unique history of low levels of Christian evangelism. This is, above all, owing to several civil wars that have plagued the country. To make general inferences about Protestant missionaries or, worse, "religion" is thus misguided. What the present chapter can provide, however, are causal estimates about the consequences of Protestant evangelism in a sample of 38 Toposa settlements.

A second concern is the relatively short intervention. In line with the strategy of the local missionary group, villages were exposed to four evangelism sessions across a two-week period. Outcomes were measured two weeks later. Undoubtedly, this design is more reliable than, say, a short priming experiment. Yet, the findings are difficult to marry with the common perspective adopted by historians of religion. They typically discuss decades or even centuries of missionary activities, not months or weeks. Such short missionary interventions are not uncommon among the Toposa (and, likely, other areas in South Sudan). But the findings are difficult to square with the missionary agenda of the large missionary societies active during colonial times.

Third and related, social capital is an outcome unlikely to shift significantly in the short term. As such, the fact that I documented changes (albeit small) is surprising. What the evidence cannot do, however, is speak to long-term effects of Protestant missionaries on social capital. While I am inclined to rule out mere priming effects, future research could help parse out long-term effects of mis-

sionary activity on social capital in greater detail. With all this in mind, I will caution, however, that field experiments of this size are logistically challenging and financially costly, particularly in remote areas such as Eastern Equatoria in South Sudan.

In conclusion, I hope that the evidence presented in this chapter motivates researchers to apply modern empirical methods to the study of religious organizations, including missionaries. The documented treatment effects—particularly those of pertaining to cultural phenomena—offer a rich repository of questions for follow-up studies. This would allow the scientific community to better understand cultural changes in developing countries. For instance, if Protestant missionaries do, indeed, change attitudes toward dowry or polygamy this likely has consequences for greater societal structures. The Toposa are organized around age and generation sets (Müller-Dempf, 2009), which would change irrevocably if dowry or polygamy practices were to be altered. Another plausible venue for scientific inquiry are missionary effects on local-level participation and democratization—arguably one of the key debates in the study religion and politics.

4.6 Supplementary Information

Table 4.10: Descriptive statistics of outcomes aggregated at village-level

	Min	Max	**Overall sample**			**Treatment group**			**Control group**		
	Min	*Max*	*N*	*Mean*	*SD*	*N*	*Mean*	*SD*	*N*	*Mean*	*SD*
Cultural outcomes											
Witchcraft items (#)	0	5	36	2.5	1.1	18.0	2.1	1.0	18.0	2.8	1.0
Tobacco	1	4	36	2.9	0.6	18.0	2.8	0.7	18.0	2.9	0.6
Alcohol	1	3	36	1.9	0.4	18.0	1.8	0.3	18.0	2.0	0.4
Witch doctor (#)	0	41	36	10.9	8.8	18.0	9.5	9.7	18.0	12.4	7.7
Praying times (#)	1	17	36	4.2	3.4	18.0	4.6	3.9	18.0	3.7	2.9
Minor spirits (%)	1	1	36	93.2	9.9	18.0	91.5	12.3	18.0	94.9	6.5
Tooth removal	2	4	36	3.2	0.5	18.0	3.2	0.5	18.0	3.2	0.5
Sex before marriage	1	4	36	2.4	0.7	18.0	2.3	0.7	18.0	2.5	0.8
Dowry	1	2	36	1.3	0.4	18.0	1.3	0.4	18.0	1.2	0.3
Polygamy	1	3	36	1.8	0.6	18.0	2.0	0.7	18.0	1.5	0.4
Tradition Index	0	0	36	-0.0	0.2	18.0	-0.1	0.1	18.0	0.1	0.2
Christian knowledge outcomes											
Trinity knowledge	0	0	36	1.8	4.6	18.0	2.1	5.5	18.0	1.5	3.7
Pentecost knowledge	0	1	36	71.2	26.7	18.0	78.8	18.4	18.0	63.5	31.7
Disciples knowledge	0	1	36	68.5	25.8	18.0	76.8	22.8	18.0	60.1	26.5
Christianity index	-1	1	36	-5.4	43.2	18.0	7.5	34.8	18.0	-18.2	47.7
Social capital outcomes											
Total friends (#)	2	7	36	4.1	1.9	18.0	4.0	1.9	18.0	4.2	1.9
Close friends (#)	2	22	36	8.3	5.0	18.0	7.8	5.6	18.0	8.9	4.5
Trust amount (g)	59	133	36	89.6	16.9	18.0	91.8	15.9	18.0	87.4	18.0
General trust	1	3	36	2.1	0.5	18.0	2.1	0.5	18.0	2.1	0.4
Cooperation signature	3	4	36	3.5	0.3	18.0	3.6	0.3	18.0	3.5	0.3
Cooperation likelihood	4	5	36	4.7	0.4	18.0	4.7	0.4	18.0	4.6	0.4
Information time (10s)	2	45	36	11.2	8.3	18.0	9.2	4.5	18.0	13.2	10.6
Information strangers (#)	2	19	36	10.5	4.2	18.0	10.3	4.7	18.0	10.8	3.8
Finding opponent (%)	0	1	36	70.6	28.4	18.0	74.1	28.9	18.0	67.0	28.2
Safety perception	3	5	36	4.6	0.5	18.0	4.5	0.6	18.0	4.7	0.4
Change proposal (10s)	1	131	36	11.2	25.9	18.0	14.3	33.5	18.0	8.2	15.4
Change petitioning	2	4	36	2.8	0.7	18.0	2.7	0.7	18.0	2.8	0.8
Social Capital Index	-1	0	36	-0.1	0.3	18.0	-0.1	0.3	18.0	-0.0	0.3

Notes: Mean (*Mean*) and standard deviations (*SD*) of all outcome measures across the overall sample as well as treatment and control groups. *Min* and *Max* depicts minimum and maximum values in the overall sample.

Figure 4.7: Picture of the Toposaland and village pair

Figure 4.8: Certificate of registration for Salt and Light

Figure 4.9: Correlation matrix of tradition index
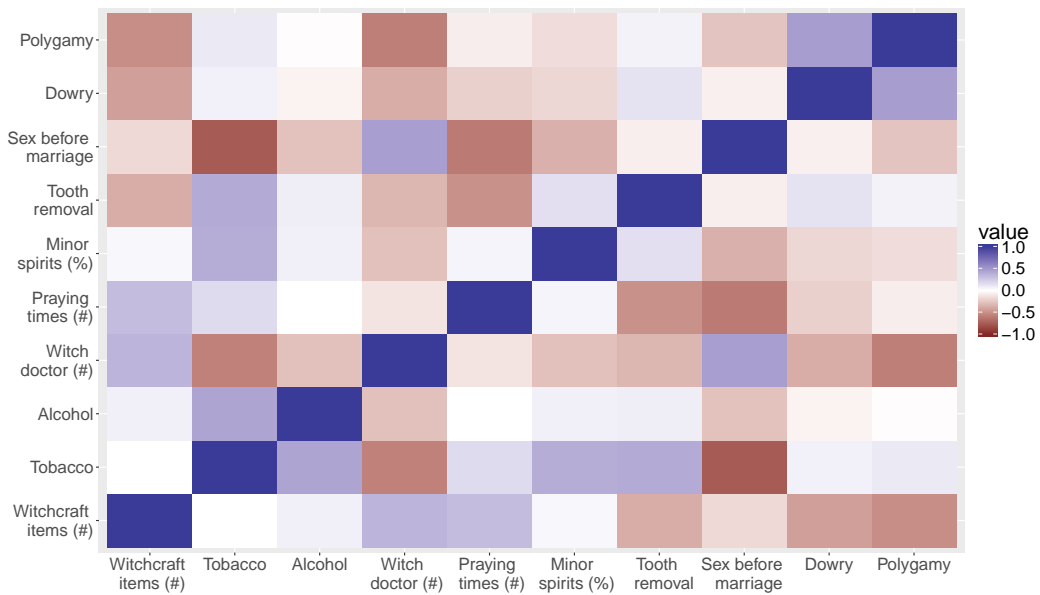
Figure 4.10: Correlation matrix of Christianity index
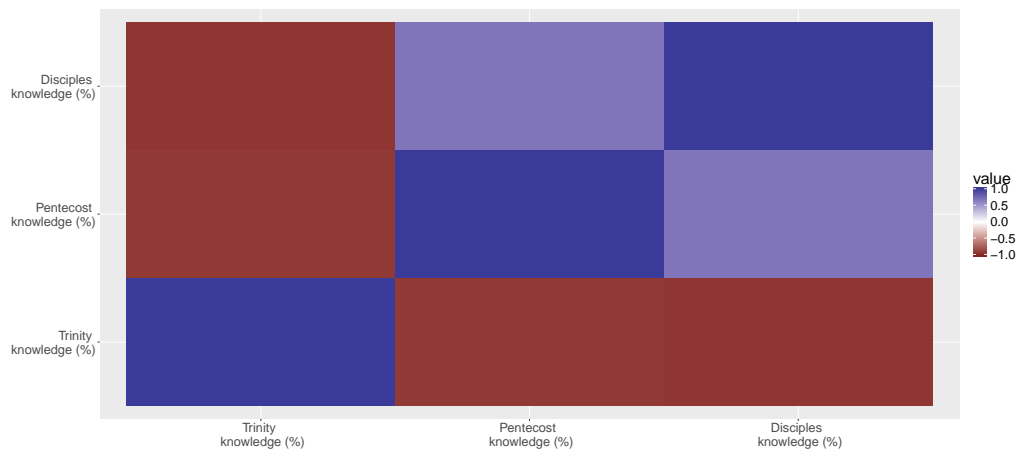
Figure 4.11: Correlation matrix of social capital Index
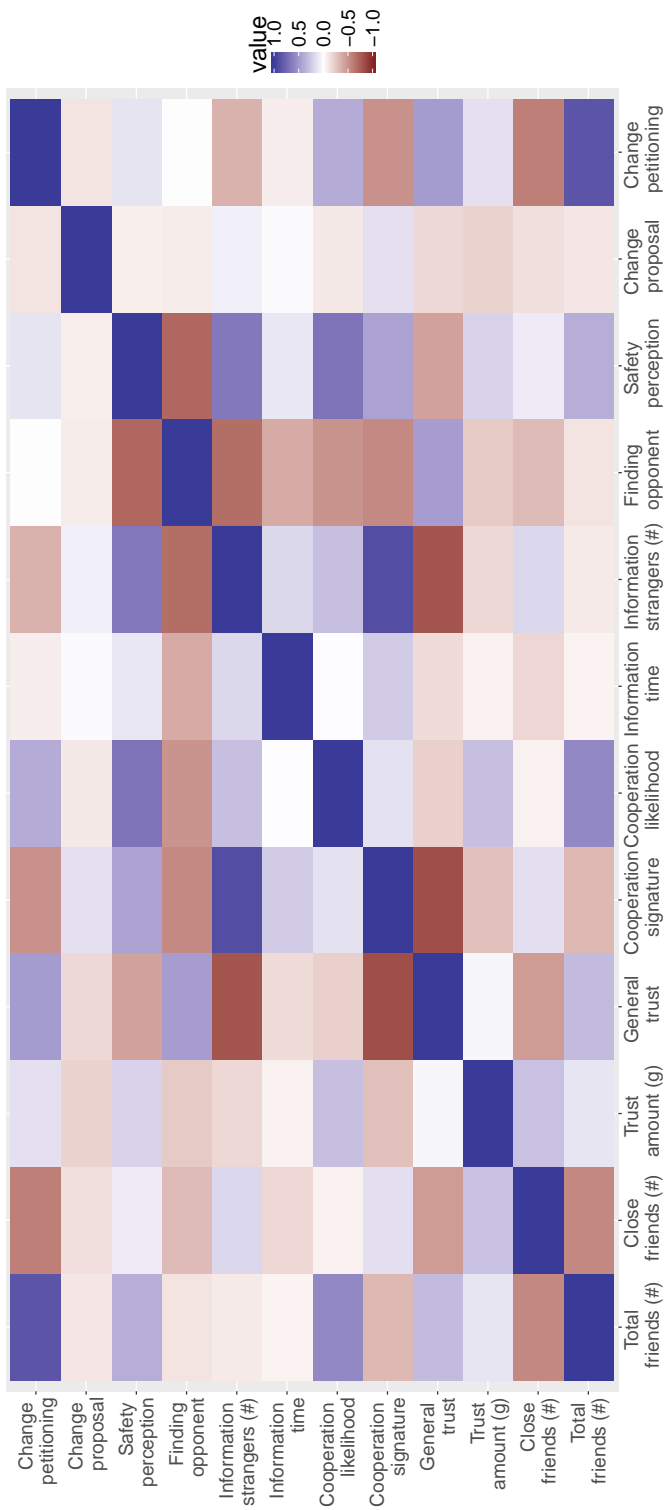
4.6.1   Schedule of Treatment and Surveying

- **Week 1** (September 19, 2016 – September 25, 2016)

  - Evangelism in Kide Emoru II

  - Evangelism in Lotakawa I

  - Survey in Lotakawa II

  - Survey in Kide Emoru I

- **Week 2** (September 26, 2016 – October 2, 2016)

  - Evangelism in Kide Emoru II

  - Evangelism in Lotakawa I

  - No Survey

  - No Survey

- **Week 3** (October 3, 2016 – October 9, 2016)

  - Evangelism in Kapelnyanga I

  - Evangelism in Lokorumoru I

  - Survey in Kapelnyanga II

  - Survey in Lokorumoru II

- **Week 4** (October 10, 2016 – October 16, 2016)

  - Evangelism in Kapelnyanga I

  - Evangelism in Lokorumoru I

  - Survey in Lotakawa I

  - Survey in Kide Emoru II

- **Week 5** (October 17, 2016 – October 23, 2016)

    - Evangelism in Toemoru I

    - Evangelism in Lolepan proper I

    - Survey in Toemoru II

    - Survey in Lolepan proper II

- **Week 6** (October 24, 2016 – October 30, 2016)

    - Evangelism in Toemoru I

    - Evangelism in Lolepan proper I

    - Survey in Kapelnyanga I

    - Survey in Lokorumoru I

- **Week 7** (October 31, 2016 – November 6, 2016)

    - Evangelism in Nakoringoomo I

    - Evangelism in Kide marino II

    - Survey in Nakoringoomo II

    - Survey in Kide marino I

- **Week 8** (November 7, 2016 – November 13, 2016)

    - Evangelism in Nakoringoomo I

    - Evangelism in Kide marino II

    - Survey in Lolepan proper I

    - Survey in Toemoru I

- **Week 9** (November 14, 2016 – November 20, 2016)

- – Evangelism in Nagolotome I

- – Evangelism in Marino villange I

- – Survey in Marino villange II

- – Survey in Nagolotome II

- **Week 10** (November 21, 2016 – November 27, 2016)

  - – Evangelism in Nagolotome I

  - – Survey in Nakoringoomo I

  - – Evangelism in Marino villange I

  - – Survey in Kide marino II

- **Week 11** (November 28, 2016 – December 4, 2016)

  - – Evangelism in Longeleya I

  - – Evangelism in Tolkopo marino I

  - – Survey in Longeleya II

  - – Survey in Tolkopo marino II

- **Week 12** (December 5, 2016 – December 11, 2016)

  - – Evangelism in Longeleya I

  - – Evangelism in Tolkopo marino I

  - – Survey in Nagolotome I

  - – Survey in Nagolotome I

- **Week 13** (December 12, 2016 – December 18, 2016)

  - – Evangelism in Korea II

- – Evangelism in Nalemsokani I

- – Survey in Korea I

- – Survey in Nalemsokani II

- **Week 14** (December 19, 2016 – December 25, 2016)

  - – Evangelism in Korea II

  - – Evangelism in Nalemsokani I

  - – Survey in Longeleya I

  - – Survey in Tolkopo marino I

- **Week 15** (December 26, 2016 – January 1, 2017)

  - – Evangelism in Lotiir II

  - – Evangelism in Loriwo I

  - – Survey in Lotiir I

  - – Survey in Loriwo II

- **Week 16** (January 2, 2017 – January 8, 2017)

  - – Evangelism in Lotiir II

  - – Survey in Korea II

  - – Evangelism in Loriwo I

  - – Survey in Nalemsokani I

- **Week 17** (January 9, 2017 – January 15, 2017)

  - – Evangelism in Nakordula I

  - – Evangelism in Natiiryae II

- Survey in Natiiryae I

- Survey in Nakordula II

- **Week 18** (January 16, 2017 – January 22, 2017)

  - Evangelism in Nakordula I

  - Evangelism in Natiiryae II

  - Survey in Lotiir II

  - Survey in Loriwo I

- **Week 19** (January 23, 2017 – January 29, 2017)

  - Evangelism in Lokorikitoe II

  - Survey in Lokorikitoe I

  - Evangelism in Lowoyapuru I

  - Survey in Lowoyapuru II

- **Week 20** (January 30, 2017 – February 5, 2017)

  - Evangelism in Lokorikitoe II

  - Evangelism in Lowoyapuru I

  - Survey in Nakordula I

  - Survey in Natiiryae II

- **Week 21** (February 6, 2017 – February 12, 2017)

  - Evangelism in Napelet I

  - Evangelism in Nadomeyit I

  - Survey in Napelet II

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– Survey in Nadomeyit II

- **Week 22** (February 13, 2017 – February 19, 2017)

  – Evangelism in Napelet I

  – Evangelism in Nadomeyit I

  – Survey in Lokorikitoe II

  – Survey in Lowoyapuru I

- **Week 23** (February 20, 2017 – February 26, 2017)

  – Evangelism in Kadapangolol I

  – Evangelism in Napelet I

  – Survey in Napelet II

  – Survey in Kadapangolol II

- **Week 24** (February 27, 2017 – March 5, 2017)

  – Evangelism in Kadapangolol I

  – Evangelism in Napelet I

  – Survey in Nadomeyit I

  – Survey in Napelet I

- **Week 25** (March 6, 2017 – March 12, 2017)

  – Evangelism in Moruarengani II

  – Survey in Moruarengani I

  – Evangelism in Najiye II

  – Survey in Najiye I

- **Week 26** (March 13, 2017 – March 19, 2017)

    - Evangelism in Moruarengani II

    - Evangelism in Najiye II

    - Survey in Napelet I

    - Survey in Kadapangolol I

- **Week 27** (March 20, 2017 – March 26, 2017)

    - Evangelism in Lopala proper I

    - Evangelism in Natiir II

    - Survey in Lopala proper II

    - Survey in Natiir I

- **Week 28** (March 27, 2017 – April 2, 2017)

    - Evangelism in Lopala proper I

    - Survey in Moruarengani II

    - Evangelism in Natiir II

    - Survey in Najiye II

- **Week 29** (April 3, 2017 – April 9, 2017)

    - Evangelism in Nakwaatal I

    - Survey in Nakwaatal II

    - Evangelism in Napeicole I

    - Survey in Napeicole II

- **Week 30** (April 10, 2017 – April 16, 2017)

- – Evangelism in Nakwaatal I

- – Evangelism in Napeicole I

- – Survey in Natiir II

- – Survey in Lopala proper I

- **Week 31** (April 17, 2017 – April 23, 2017)

  - – Nothing

- **Week 32** (April 24, 2017 – April 30, 2017)

  - – Survey in Nakwaatal I

  - – Survey in Napeicole I

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Conclusion

This final chapter serves as a point of reflection. The three substantive chapters of my dissertation—though they analyze different settings, independent variables and outcomes—share related outcomes of interest: social inequality and cohesion. They also share four challenges, which deserve a concluding remark: measurement, mechanisms, uncertainty and external validity, and possible normative consequences.

Measurement. The three substantive chapters have broadly assessed outcomes pertaining to social cohesion and inequality. Both social cohesion and social inequality, however, are latent constructs. They cannot be directly observed and must be inferred from a variety of empirical measures. This presents a measurement problem, which must not be taken lightly. To the extent possible, the chapters on Peru and South Sudan have relied on behavioral measures. Such measures are sensible given that they address social desirability concerns. Behavioral games, however, are not without their problems. Above all, they lack in realism. Trust games, for instance, place individuals in a rather contrived situation of having to send money to a random person. This is not to say that the game does

not predict trusting behavior. But just how high is this correlation? I, therefore, conclude this dissertation with a scientific urge to find behavioral outcomes that are more realistic. One such outcome was the wearing of witchcraft items in South Sudan. It was independently assessed by the enumerators. It could therefore not have been biased by demand effects. Whether such behavioral outcomes exist for measures pertaining to social capital is a question of significant scientific importance.

But, measurement challenges were also present in Chapter two. Here, my key measures for social inequality—the share of women in local councils as well as the share of aristocrats in Rotary clubs—were administrative. Yet, it remains to be seen whether they are reliable measures of social inequality that the scientific community interprets as such. The mere fact that there exist only few studies that explicitly measure social inequality suggests that more work needs to be done in this area. Possible paths forward in the measurement of social inequality are the distribution of incomes and high-status jobs among social minorities. It is my intention to fill this gap in future studies, perhaps by, again, accessing tax records, which offer a rich repository for nuanced administrative outcomes.

Mechanisms. Chapters two and three have gone beyond estimating causal effects of a treatment on an outcome. Specifically, in Chapter two I argued that the relation between inheritance customs and inequality may be mediated by two intermediary outcomes: wealth equality and pro-egalitarian preferences. The problems of mediation are well-rehearsed (Green, Ha and Bullock, 2010). Above all, mediators are typically endogenous phenomena. And, even if they were to be experimentally manipulated by a researcher, it is exceedingly difficult to claim that only that very mediator (and not some other mediator) was manipulated. For that reason, I refrained from conducting a mediation analysis in Chapter 2. In

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Chapter 3, however, I made the case that the Evanglical and Pentecostal missions plausibly activated the two hypothesized mechanisms—pro-social preferences and networks—to differing degrees. Even in this case, however, the analyses continues to be observational. After all, the mediators were not randomly assigned but observed. There can thus be no doubt that such analyses do not reliably recover causal effects. As such, my dissertation has instilled in me a desire to reflect more deeply on how such observational findings can motivate future research. Are observational mediation analyses futile? Or can they inform where mediation may occur? The dilemma gets at a fundamental problem, namely, to what degree one is willing to view correlation as *indicative* of causation.

Aside from the question about causality, chapter two, to me, has highlighted that social scientists conceptualize mediators quite differently. In particular, they disagree on the extent to which mediation has a temporal component. Take the classic "Vitamin C" example for mediation. As Green, Ha and Bullock (2010, 207) write: "the introduction of limes into the diet of seafarers in the eighteenth century dramatically reduced the incidence of scurvy, and eventually twentieth-century scientists figured out that the key mediating ingredient was vitamin C." In this reading, Vitamin C is but one dimension of the treatment that causes a reduction in scurvy. This definition is what I had in mind when presenting wealth equality as a mechanism—a mechanism that is part of the treatment, i.e., equitable inheritance. Yet, the second mechanism, pro-egalitarian preferences, are not part of the treatment. Rather, they are an intermediate outcome, which may, in turn, affect social inequality. The two mechanisms thus are conceptualized quite differently. For that reason, I hope to further explore the between mediators as a distinct *part of the treatment* and mediators as an *intermediary outcome* in future research.

Uncertainty and External Validity. The three chapters all are shaped by a desire to recover causal effects. Undoubtedly, chapter four is the most likely candidate to have done so. By randomly assigning villages to a short evangelism intervention, the Chapter convincingly shows that missionaries affect local cultural traditions and beliefs as well as knowledge about Christianity. The effect on social capital, however, was mixed. An increase in pro-social behavior was juxtaposed with a reduction in village-level social capital. This is not a problem. After all, pre-registration does not prevent researchers from documenting such results. But, the finding works as a careful reminder about just how much uncertainty there is regarding the determinants of broader social outcomes such as social capital. Indeed, the project has left me wondering whether broad social outcomes may simply escape clear static laws.

Relatedly, I found the strongest effect sizes in Peru—where I studied a sample of 16 villages. By contrast, effect sizes were smallest in Chapter two, where I analyzed 8,500 West German municipalities. The difference in effect sizes is noteworthy. It reminds one that clear treatment effects (or theoretical predictions) are most likely to be achieved in homogeneous settings where researchers have larger control and theoretical oversight. This is interesting inasmuch as there is often a perceived desire among scholars to generate "externally valid" results. Yet, external validity is undoubtedly an unreachable ideal as separating signals from noise in large, complex social settings is close to impossible. As such, large observational studies face two distinct challenges: they seldom afford causal identification and are sufficiently complex to make it difficult to separate signal from noise. Thus, while chapter two did demonstrate notable effect sizes—maintaining a careful identification strategy—I have become more skeptical about the promises of studies stretching broad social arenas.

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Normative consequences. My dissertation has tackled two broad outcomes, inequality and social cohesion. My goal was to highlight possible determinants of these rather broad social constructs. What was beyond the scope of work, however, is to reflect on potential normative consequences of my findings. Are social equality and cohesiveness necessarily desirable outcomes? This question is far from trivial. After all, writes Abbott "when we say 'inequality' we usually mean 'injustice' " (2016, 234).

Two issues come to mind. First, if inequality and social cohesion are also normative constructs, this raises an issue of measurability. Abbot argues "that some version of immeasurability may in fact be the identifying mark of the normative" (2016, 234). My dissertation grappled precisely with this problem. In Chapter 2, my outcome was the unequal treatment of women, minorities and aristocrats in councils and elite clubs, respectively. While these three groups are traditionally perceived as socially relevant strata (by Tocqueville and others), the demarcation remains arbitrary. Put differently, other relevant markers of identity could have been analyzed, making the measurement of equality somewhat dauting.

Second, the very norm underlying equality and cohesiveness warrants reflection. Scholars of development typically perceive greater levels of social capital and cohesiveness to be a good thing. My experience in the field, however, also pointed to negative repercussions of tightly knit communities. In Peru, for instance, one village president lamented that "social control" made it difficult to have truthful political competition. Such concerns were not raised by presidents in more "individualistic" villages. Here, the market of ideas and goods seemed to thrive. Such arguments are not new. Max Weber famously linked the ethos of Protestantism to the entrepreneurial, individualistic spirit of capitalism (Weber, 2002). At a minimum, my experience in Peru thus shows that the normative dimensions of

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concepts such as "cohesiveness" are undoubtedly context-dependent.

With these unoriginal and rather broad considerations I would like to conclude my dissertation. I hope that the evidence and design choices will help inform policy and academic debates. And I am hopeful that the coming years will allow researchers (including myself!) to expand on my projects in ways that will allow us to draw firmer conclusions about the determinants of societal cohesion.

6

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