On the Importance of African Traditional Religion for Economic Behavior[*]

Lewis Dunia Butinda[†]   Aimable Amani Lameke[‡]   Nathan Nunn[§]

Max Posch[¶]   Raul Sanchez de la Sierra[||]

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Abstract: Within the field of economics, despite being widespread, African traditional religions tend to be perceived as unimportant and ignored when studying economic decision-making. This study tests whether this presumption is correct. Using daily data on business decisions and performance of beer sellers in the eastern Democratic Republic of the Congo, we study the importance of traditional religious beliefs for economic behavior and outcomes. Beer sellers perceive the risk of theft in their shops to be higher than it actually is, causing them to hold lower inventories, more frequent stock-outs, and reduced profits. We facilitate randomly-timed access to commonly-used, but typically prohibitively expensive rituals, which reduce the perceived risk of theft. We find that the rituals partially correct the beliefs about the risk of theft for sellers who report believing in the ritual's efficacy. These sellers purchase more inventory, experience fewer stock-outs, and have larger sales, revenues, and profits. To distinguish the belief in the efficacy of the ritual from other incidental effects of participation, we analyze these outcomes for sellers who do not believe in the ritual. For these individuals, we find none of the observed effects. The findings provide evidence of the importance of African traditional religions, demonstrating that they can influence behavior and outcomes that are important for economic development.

Keywords: Supernatural beliefs; rituals, firm behavior.

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[†]Marakuja Kivu Research, Goma, Democratic Republic of the Congo (email: `duniabutindalewis@gmail.com`)

[‡]Marakuja Kivu Research, Goma, Democratic Republic of the Congo (email: `amanilamek@gmail.com`)

[§]Vancouver School of Economics, University of British Columbia and CIFAR (email: `nathan.nunn@ubc.ca`)

[¶]Department of Economics, University of Exeter (email: `m.posch@exeter.ac.uk`)

[||]Harris School of Public Policy, University of Chicago, NBER and CIFAR (email: `raul@uchicago.edu`)

1. Introduction

The African continent has experienced widespread conversion to Islam and Christianity, but many people continue to hold traditional African religious beliefs. While diverse, the beliefs center around a supreme creator, ancestors, spirits, traditional medicine, magic, and rituals (Pobee and Mends, 1977, Mekoa, 2019). They have largely been overlooked by foreign academics, researchers, policymakers, and NGOs, particularly in the field of economics, where they tend to be dismissed as weakly held or irrelevant to economic behavior. This contrasts with the reality on the ground, where these beliefs are widespread, strongly adhered to, and often significant to human behavior (Chitakure, 2017, Aderibigbe and Falola, 2022).

Surveys asking about belief in "witchcraft" or the "evil eye," which are common Western terms for African traditional religions, reveal the widespread adherence to these beliefs. Religious census data show that the majority of individuals in almost every non-Western country report believing in witchcraft or the evil eye, with many countries reporting rates well above 90% in all regions of the country (Gershman, 2016). African countries are no exception. In virtually every African country, the majority of the population holds traditional religious beliefs.[1]

Although we know that these beliefs are prevalent, we have a limited understanding of their strength and their significance for economically relevant behavior and outcomes. This gap in knowledge has important implications for policy and our understanding of decision-making in Africa. If traditional religious beliefs are relevant to economic outcomes, their exclusion from the economics literature is an important shortcoming.

Our study explores the impact of traditional religious rituals on the economic activities of beer sellers in rural villages of North Kivu, located in the Eastern Democratic Republic of the Congo (DRC). We examine the economic consequences of these rituals among beer sellers from the Bahunde, Bahutu, and Banyianga ethnic groups who reside in the districts of Rutshuru, Masisi, and Walikale. Beer sellers are a natural group to focus on when studying the economic consequences of traditional religious rituals. They are present in nearly all populated areas in Eastern DRC, have similar distribution channels, and sell a limited number of standardized products, which makes their decisions and outcomes especially suitable for comparison. They face significant uncertainty due to the weakness of the state, and their business success is often

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[1]See Appendix Figure A1.

attributed to supernatural forces. Consistent with most of sub-Saharan Africa, there are rituals, amulets, and spells that are believed to reduce the occurrence of bad events. Within the area of our study, these supernatural "medicines" are called *dawa* (medicine in Swahili) or *gri-gri*. The spells generally involve a ritual, the enchantment of an object, and adherence to specific conditions to maintain the continued efficacy of the spell.

Over several years, we collected daily information on the business activities of 44 beer sellers using a smartphone-based reporting and verification system. These data revealed three key findings that informed our experiment. First, sellers frequently worried about the potential theft of their beer stock and tended to overestimate the actual likelihood of theft. Second, they typically kept low beer inventories and frequently experienced stock-outs, which limited their sales potential. Third, beer sellers demonstrated a high willingness to pay for rituals and amulets that protect against inventory theft.

For beer retailers with traditional religious beliefs, protection rituals are seen as valuable but expensive investments that are desirable if one can afford them. Among our sample, the average willingness to pay for a ritual was $31.50. Most participants had previously taken part in theft protection rituals. However, since these rituals are costly and only last for a limited time, most sellers remain unprotected most of the time. Historically, the cost of a protection ritual was one goat, but recently, the monetary equivalent (approximately $40–50) is also accepted.

A few months before the baseline data collection ended, Marakuja – the Congolese non-profit organization that was managing the data collection – asked the beer sellers how the local organization could best assist them. Marakuja communicated that they would like to give the sellers a token of gratitude, in addition to their compensation for the sellers' role in the data collection. The sellers requested help in paying for rituals that could enhance business performance.

The anti-theft spells are delivered through one or two-hour in-person rituals, during which ancestors provide protection against beer inventory theft. During the ritual, a traditional religious practitioner, known as *waganga wa tiba asili* in Swahili or *tradipraticien* in French, invokes the community's ancestors, communicates the desire for protection, and performs a ceremony that often includes chants, dancing, and the insertion of various herbs or other items into an object, which becomes enchanted. At the end of the ritual, the individual receives the object, which has protective powers that are expected to work as long as the object is placed in the store, and the individual adheres to the necessary conditions (i.e., actions). These conditions can be difficult to

fulfill, and if they are unknowingly violated, the amulet's efficacy is believed to diminish over time. In general, the protection is expected to last several days or weeks.

To examine whether traditional religious beliefs are relevant to economic outcomes, and to satisfy the requests of the beer sellers, we provided free access to a protection ritual to all beer sellers. The rituals occurred in an urban center of each district, so the sellers were transported to and from the location of the ritual. All costs associated with the rituals were covered by the research team.

To obtain causal estimates of the effects of participating in the rituals, we randomized the timing at which Marakuja provided access to the rituals for the beer sellers. By taking advantage of the random variation in treatment timing and using the data collection system developed over two years prior to the experiment, we conduct two complementary analyses. Both analyses enable us to estimate the causal impact of the rituals on the economic choices, expectations, and beliefs of the sellers.

The first analysis, an event study, examines sellers' outcomes before and after each seller's ritual. We explore patterns in the raw data, a standard two-way fixed effects specification with beer seller and date fixed effects, and the Callaway and Sant'Anna (2021) procedure that estimates cohort-specific time average effects.

The second analysis employs a waitlist design, comparing sellers' outcomes during 'treated' observations (days when protected) to those during 'non-treated' observations (days when not protected). This specification, which includes beer seller fixed effects, is similar to the event study analysis in that it compares average outcomes before and after the ritual, but it accounts for the varying likelihood of treatment over time based on the number of untreated individuals remaining.

In both analyses, we find that the ritual affected the business decisions and outcomes of the sellers. It is associated with increased inventory purchases, which results in fewer stock-outs, more sales, revenues, and higher profits. We find no effect on sales prices. We find that, after treatment, those who believe in the ritual – approximately 66% of the sample – are more likely to feel protected from theft and less likely to believe theft will occur, despite no change in actual theft risk. We find no effect of the ritual on self-reported stress. The event study analyses provide insights into the timing of these effects. They show no change in outcomes prior to the ritual and then a persistent change that begins immediately following the ritual. Thus, there is no evidence

that the estimated effects are driven (or biased) by anticipation effects which influence behavior prior to the ritual. The findings indicate that the protection ritual results in more purchases, increased inventory, reduced stock-outs, higher sales, and greater profits.

Approximately one-third of our sample did not believe in the effectiveness of traditional religious rituals. These sellers report never having participated in such a ritual before the experiment. Even those who did not believe in the rituals still participated in the ritual that was provided. Reassuringly, the effects described earlier are only found among those who reported believing in the protection spell. While belief in the ritual is not randomly assigned, we view this as an important falsification test, offering additional suggestive evidence that the ritual influences beer sellers' revenues by altering their perceived probability of theft. It also provides assurance that aspects of treatment other than the ritual – like being transported to the local city – are not driving our findings.

Given the presence of non-believers in our sample, we implement a third strategy that takes advantage of this distinction. We estimate a difference-in-differences (DD) specification comparing believers and non-believers before and after they were scheduled to participate in a protection ritual. We evaluate three different versions of a DD estimator — two-way fixed effects, waitlist design, and the Callaway and Sant'Anna (2021). All three estimators confirm our previous findings, showing a decrease in the belief that theft will occur and in stock-outs, and an increase in inventory purchases, revenues, and profits.

A crucial aspect of our findings is that the ritual helped create beliefs about the probability of theft that were more accurate. We find that without the ritual, sellers tended to overestimate the likelihood of theft. The ritual partially rectified this by decreasing the perceived probability of theft. By correcting this biased belief, the ritual led to behavior that was closer to optimal, resulting in increased inventories, reduced stock-outs, higher sales, and greater profits.

The economic benefits of the rituals prompt the question of whether rituals are a profitable investment for our beer sellers. To explore this, we assess the long-term effects of the ritual and discover that they persist for at least three months, which is when our data collection concludes. Based on the estimates and calculations, the typical seller earns an additional $78 in profits during this period. However, this figure conceals heterogeneity depending on the size of the seller. When comparing the distribution of additional profits across beer sellers to the approximate cost of the ritual, we find that for 22% of all sellers (nine in total), the increase in profits are greater than the

cost of the ritual.

In summary, our findings offer evidence that protection rituals, a common aspect of African traditional religions, have significant economic implications. Among our sample of beer sellers, the rituals affect beliefs about theft, inventory purchases, stock-outs, sales, revenues, and profits. An important takeaway from our findings is that African traditional religious beliefs have important effects on standard economic outcomes. These findings contribute to a nascent literature within economics that seeks to understand the consequences of African traditional religions (Leeson, 2014, Gershman, 2015, 2016, Alonso, Houssa and Verpoorten, 2016, Nunn and Sanchez de la Sierra, 2017, Alidou and Verpoorten, 2019, Stoop, Verpoorten and Deconinck, 2019, Stoop and Verpoorten, 2020, Deopa and Rinaldo, 2023, Alesina, Hohmann, Michalopoulos and Papaioannou, 2023, Le Rossignol, Lowes and Nunn, 2023).

Our findings also contribute to a larger literature that seeks to understand the consequences of religion on economic outcomes. Our findings, which provide evidence for African traditional religions, add to existing evidence for Christianity (Becker and Woessmann, 2009, Gallego and Woodberry, 2010, Acemoglu, Gallego and Robinson, 2014a, Nunn, 2014, Benjamin, Choi and Fisher, 2016, Andersen, Bentzen, Dalgaard and Sharp, 2017, Rubin, 2017, Waldinger, 2017, Valencia Caicedo, 2019, Fruehwirth, Iyer and Zhang, 2019, Schulz, Bahrami-Rad, Beauchamps and Henrich, 2019, Squicciarini, 2020, Montero and Yang, 2022, Schulz, 2022, Alesina et al., 2023, Becker and Voth, 2023, Espín-Sánchez, Gil-Guirado and Ryan, 2023), Islam (Clingingsmith, Khwaja and Kremer, 2009, Campante and Yanagizawa-Drott, 2015, Platteau, 2017, Rubin, 2017, Kuran, 2018, Bazzi, Koehler-Derrick and Marx, 2020, Alesina et al., 2023, Mehmood, Seror and Chen, 2023), and Judaism (Abramitzky, 2008, Pascali, 2016, Johnson and Koyama, 2017).

Our focus on traditional indigenous religious rituals connects our study to literature outside of economics that studies the role that superstitions, rituals, and religious beliefs play in helping individuals cope with stress, uncertainty, or trauma. The research tends to either be descriptive case studies (Malinowski, 1948, Sosis and Handwerker, 2011) or based on cross-sectional correlations (Bryant-Davis, 2013). Our findings contribute to this existing knowledge by providing causal estimates of the effect of such rituals in a real-world setting and using randomized controlled field experiments. Our study is related to a growing number of publications that use randomized controlled field experiments to study the causal effect of religion on economic outcomes (Auriol, Lassebie, Panin, Raiber and Seabright, 2020, Auriol, Delissaint, Fourati, Miquel-Florensa and

Seabright, 2021, Bryan, Choi and Karlan, 2021, Le Rossignol et al., 2023).

Lastly, our findings provide a previously overlooked explanation of a fundamental problem studied in development economics: how insecure property rights might disincentivize productive investment (Goldstein and Udry, 2008, Besley and Ghatak, 2010, Acemoglu, Reed and Robinson, 2014b). Our findings add to this literature by showing that factors usually presumed irrelevant for economic analysis may play a central role in solving one of the most important problems of development economics.

The remainder of the paper is structured as follows. Section 2 provides an overview of the beer retail market in the Eastern DRC and the nature of traditional religions in the region, including protection rituals. It also describes the data collection infrastructure. Section 3 describes the experimental design. Our various estimation strategies and our results are reported in Section 4. In Section 5, we offer concluding thoughts.

2. Background to Beer Sellers and Supernatural Protection Rituals in the Eastern DRC

A. *Beer Retail Market*

The beer market in the eastern Democratic Republic of Congo (DRC) is dominated by two companies: Bralima, owned by Heineken, and the French consortium Castel. Bralima produces the most popular beer in the region, called Primus. The beer is shipped from Bralima's brewery in Bukavu to its subsidiary in Goma via a network of intermediaries who use boats to transport the beer daily. From Goma, wholesalers distribute the beer to retailers across rural areas. A similar distribution network is in place for Castel. The rural sellers, who have retail outlets in their local village, typically travel to wholesalers in urban centers to replenish their inventories. A bottle of Primus typically costs 1,500 Congolese francs (about US $1.10). Bottles are reused, and the value of the bottle is about ten times the cost of a beer.

B. *Collection of Two Years of Daily Data*

The origins of the project date back to July 2015, when we implemented a 28 month firm-level data collection system for 81 rural beer sellers the eastern Democratic Republic of Congo (DRC). Our survey team visited a random sample of 81 villages in North Kivu province and invited a beer seller in each village to participate in the study. When there was more than one seller, the

surveyors selected the seller with the largest establishment. The surveyors trained the beer sellers to use a smartphone application to enter daily data about their business. This took upwards of one week since many sellers were unfamiliar with smartphones. We provided each seller with a smartphone used solely for data entry and a solar panel to charge the phone. To encourage participation, we gave sellers a $30 payment and phone credit to transmit the data weekly. Since many villages were located in remote areas without cell phone coverage, we also provided transport money to enable participants to travel to the nearest area with cellular coverage.

Ensuring data quality was a critical aspect of our study, we took several measures to achieve this. Our local team made weekly calls to each of the 81 beer sellers to review the data they had entered and address any issues. To further ensure the accuracy of the data, a project coordinator based in Goma verified the submissions every week and thoroughly inspected each seller's data to detect potential issues. In addition, the smartphone-based survey recorded GPS information and time stamps for each data entry. This allowed us to ensure that data were being entered daily and enabled us to verify the location of each seller. We also informed beer sellers that we would be monitoring the quality of the data and that any detected issues could result in loss of compensation for that month. These steps were effective in obtaining high-quality data with minimal aberrations.

In May 2017, due to ongoing conflict and violence in the region, we had to suspend the data collection in two of the five districts (Beni and Lubero) before the experiment took place (which was in July and August, 2017). Moreover, one beer seller declined to participate in the experiment due to his monotheistic beliefs. Therefore, we ultimately had a sample of 44 sellers from three districts in North Kivu who participated in the experiment.

From July 2015 to November 2017, the following information was collected daily for sixteen beverage brands: (*i*) the price and quantity of bottles sold; (*ii*) the price and quantity of bottles purchased; (*iii*) the number of bottles in stock at the end of each day; and (*iv*) whether the beer seller experienced a theft during the day. From late April 2017, the following questions were added to the daily surveys: (*v*) whether they suffered a stock-out during the day; (*vi*) how much business beer sellers lost due to the stock-out; (*vii*) their perceived risk of theft during the next day, week, and month; (*viii*) self-reported stress level; and (*ix*) whether they feel protected by an amulet carrying supernatural powers.

C. *Facts about Beer Sellers and Their Business*

The data collection from 1 July 2015 to 15 July 2017 (the day before the rituals were rolled out) reveals that, among the 44 beer sellers who participated in the experiment, the average seller sells 16.8 bottles of beverages per day, generating approximately $20 USD in daily revenues. Appendix Figure A2 indicates that 75% of these sales are from beer, with the remaining 25% from other beverages, such as Coca-Cola, Fanta, or water. Among beer brands, Primus generates the highest revenues.[2]

Furthermore, the data collection reveals several interesting facts about the retail beer market in rural North Kivu and the role of supernatural protection for business.

Fact 1: Theft is a common occurrence, but beer sellers have an overly pessimistic perception of the risk. Figure 1a indicates that 1.9% of beer retailers experience theft of their inventory on a given day. When a theft occurs, 15 bottles on average are lost.[3] We also find that sellers tend to overestimate the risk of theft. On approximately 8.9% of days, they report being certain that a theft will occur the next day, higher than the actual frequency. As we discuss below, this is an important fact for understanding the findings of the experiment.[4]

Fact 2: Beer sellers frequently lose business due to stock-outs. In the face of the risk of theft, a key decision for sellers is how much inventory to stock. A smaller stock of inventory reduces the expected loss from theft. However, less inventory makes it more likely that a particular brand of beer, or even all beer, is sold out, resulting in lost business. Figure 1b documents the prevalence of stock-outs, showing that approximately 11.7% of beer sellers report experiencing a stock-out on a given day. Beer sellers suffer a (partial) stock-out about once a week, leading to significant losses. According to their reports, the average stock-out results in approximately $23 of lost revenues.

Fact 3: Beer sellers highly value protection rituals. There are actions that sellers can take to help protect against the possibility of theft. The two most common forms of protection are either

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[2]We find that the 44 beer sellers who participated in the experiment were very similar to the 37 beer sellers who could not participate in the experiment, at least in terms of the total volume and composition of sales before the data collection was interrupted. See Appendix Figure A3.

[3]The insecurity can be attributed to the state's weakness in protecting property rights. At the time of the study, 122 armed groups and numerous bandits were in the region, regularly engaging in theft (Kivu Security Tracker, 2021).

[4]We also find that the 14 beer sellers who do not believe in supernatural protection have lower perceptions of the likelihood of theft compared to the 30 beer sellers who believe in supernatural protection (see Appendix Figure A4).

Figure 1: Perceived risk of theft, actual theft, and the frequency of stock-outs

Share of beer sellers, per day (in %)

Share of beer sellers, per day (in %)
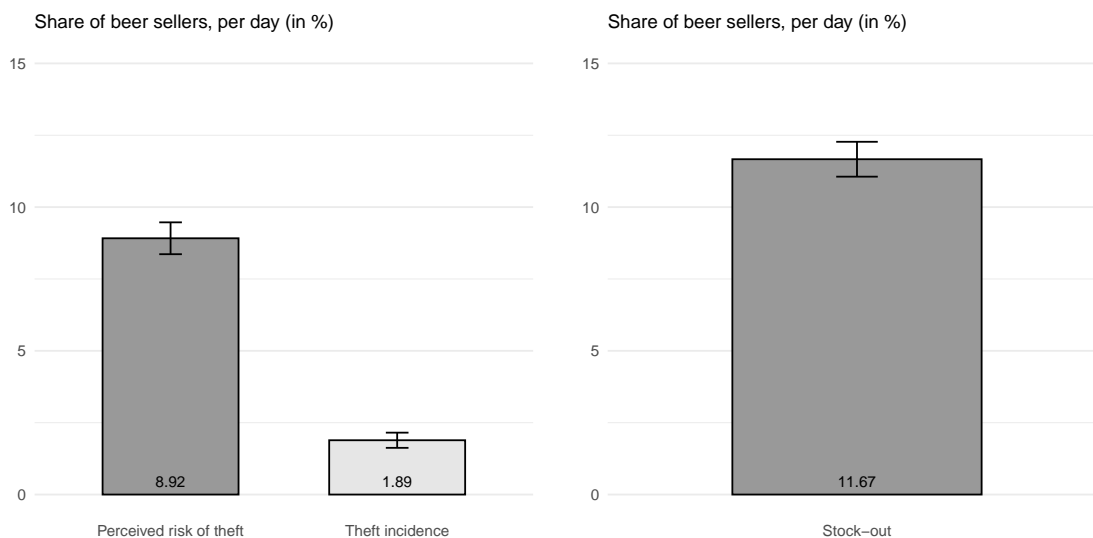
(a) Average perceived vs. actual risk of theft

(b) Average frequency of stock-outs

Notes: Top Left: This figure compares the perceived risk of theft, computed as the proportion of sellers who believe they will be the victim of theft the next day, with the actual risk of theft, estimated as the proportion of sellers who were victims of theft on a given day. Top right: The figure shows the frequency of stock-outs, computed as the proportion of sellers who report experiencing a stock-out during the day. These two figures summarize data collected from May 1st to July 15th, 2017. The error bars represent standard errors.

hiring a security guard to watch the business at night, or participating in a protection ritual. As noted, the latter is typically viewed as being a safer investment since one cannot be certain that someone whom you hire to watch the store will not themselves steal from you. To quantify the demand for supernatural protection, in June 2017, we asked beer sellers to indicate their maximum willingness to pay for an anti-theft ritual. Specifically, we asked *"Up to what amount would you be willing to pay for the anti-theft gri-gri?"*. In total, 41 of the 44 beer sellers reported a willingness to pay. On average, the sellers were willing to pay $31.50, indicating that they viewed supernatural protection rituals as an effective method of providing protecting against theft.

D. *Demand for Supernatural Protection and Protection Rituals*

In 2017, as the data collection was coming to an end, Marakuja consulted with the beer sellers to identify ways to thank them for providing their business data. While direct monetary support was the preferred option for some, many requested support in purchasing an amulet to help them run their business.

The demand for protection rituals, along with the three facts outlined above, motivated an

experiment in which the timing of access to supernatural protection rituals would be randomized. As noted above, the survey data show that demand for protection rituals is high. This was also confirmed by qualitative interviews and pilot studies conducted in March 2015 and June 2016, where we found that traditional rituals that protect against theft or increase sales are in high demand, common among beer sellers, and are purchased when they can be afforded.

While the specifics of protection rituals can vary greatly from one village or provider to another, they maintain certain commonalities. The rituals require the performance by a person possessing the requisite knowledge and mystical power to effectively implement them. They must be conducted in private, and they require certain criteria during the ceremony to be followed. An object – i.e., an amulet – is typically bestowed with supernatural powers by ancestors during the ceremony. The beneficiary must follow a series of conditions in order for the protection to remain effective, with the most common condition being that they must keep the amulet in their possession.

To better understand the properties of the anti-theft spells in our specific context, in July 2017, we interviewed individuals familiar with the reputations of local traditional practitioners. These individuals included local chiefs, village secretaries, and lineage elders. Through these interviews, we were able to compile a list of respected traditional practitioners across all rural areas of North Kivu, the province our study focuses on. As a result, we collected comprehensive information on the effectiveness of 40 traditional practitioners, which included specific details about the spells they offered. We present a summary of the fundamental attributes of these practitioners and their protective rituals in our study area.

a. Traditional Practitioners and Their Skills

We focused on 40 traditional practitioners who lived across various rural areas in North Kivu, spanning the districts of Walikale, Masisi, and Rutshuru.[5] These practitioners were often acting as traditional doctors, providing healing and a variety of other services. Out of the 40 practitioners we studied, six had official certificates while many belonged to an association of traditional practitioners.

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[5]These areas were Wanyanga, Kishengo, Osso, Bwisha, Bakano, Batangi, Bahunde, Bwito, and Bamate. 20 practioners resided in Walikale, 11 in Masisi, and 9 in Rutshuru. Walikale comprises half of the area of the province of North Kivu. The number of practitioners is approximately proportional to the areas of these three districts. Overall, this covers 2/3 of the area of North Kivu. We were forced to exclude Beni and Lubero, two districts commonly referred to as the "Grand Nord Region," from the survey due to safety and insecurity concerns.

We found that 70% of these practitioners possessed the skill to perform the anti-theft ritual. Additionally, 97.4% could provide luck spells, with a smaller percentage offering general protection spells (23.1%) and anti-bullet spells (15.4%).[6]

Our team conducted structured interviews with all practitioners to assess their efficacy. During these interviews, our team members asked practitioners to demonstrate their skills by performing a ceremony. The team also held more informal interviews with local villagers to survey each practitioner's reputation in the village. They then rated each practitioner on a scale of 1 (lowest) to 5 (highest). These ratings revealed large variation. The average rating was 2.9, with standard deviation 1.2, minimum 1 and maximum 5.

b. Ceremony Procedures

A typical ceremony lasts a few hours and involves the traditional practitioner enchanting an object through a series of culturally prescribed steps, thereby imbuing it with anti-theft properties. The steps are generally predetermined by established custom, allowing customers to determine whether the practitioner is authentic or fake. In many cases, there are also demonstrations of powers aimed to convince the customer that the practitioner masters supernatural powers. The beneficiary is typically present in the ceremony, although this does not seem to be necessary for the efficacy of the amulet in all cases.

During the ceremony, the practitioner communicates with ancestors, known as *Bakoko* in local languages. The ceremony consists in him talking to the ancestors through various channels, including possession of a third person who acts as a messenger with the voice of the ancestors, and songs and music intended to attract the ancestors to the scene. The practitioner then asks the ancestors to give their powers for a specific purpose while the practitioner follows a set of steps marked by custom.[7]

If the ancestors are satisfied with the ceremonial steps, their power is infused into an object, which serves as an amulet. The beneficiary is then required to keep the amulet for protection. The amulet's interior is filled with a mix of plants, roots, seeds, and/or oil.

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[6]The ability to provide such spells was sometimes restricted. For example, two practitioners reported that if they lived outside their village for more than one month (which was required for their involvement in the study), they would not have the requisite powers.

[7]In some cases, practitioners must present a gift to the ancestors to secure their favor, often in the form of an animal sacrifice. For example, it was common for practitioners to behead a chicken during the ceremony. We did not consider practitioners as candidates if they required an animal sacrifice for their ritual.

c. Behavioral Prescriptions

The rituals' efficacy depends on the beneficiary adhering to certain requirements. These vary by practitioner and spell, but common conditions include avoiding sexual intercourse with a menstruating woman, hiding the amulet under a cabinet or wardrobe, abstaining from alcohol, not saluting anyone on the way back from the ritual to the shop, refrain from drinking or showering with rainwater, not stealing from or fighting with others, and sweeping the shop before installing the amulet.

In general, there tends to be uncertainty with whether the prescriptions are followed perfectly. For example, whether the shop was swept to the full extent necessary. There is also uncertainty in whether the conditions begin to break down over time. For example, whether the hidden amulet is found by someone. Thus, it is generally believed that the power of the ritual isn't perfect and that its efficacy will tend to diminish over time unless eventually reactivated through a follow-up ritual.

d. Cost of the Rituals

Despite differences among ethnic groups, the high cost of the rituals, typically between $40 to $50, is prohibitive for many individuals. This is especially true considering the average annual per-capita income in the DRC is approximately $250. Given that the actual cost of performing the rituals is often limited to a few plants and a doll, these high prices suggest that practitioners might be charging substantially more than their direct costs.

3. Experimental Design

Given the background described in the previous section, we decided to study the economic effects of protection rituals. We now describe in detail the experimental design.[8]

Selection of Traditional Practitioners

The selection of traditional practitioners for our study was rigorous, to ensure the legitimacy and efficacy of the rituals performed. We used the detailed information collected on the 40 most reputed practitioners in the province. We sifted through comments and reviews about each

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practitioner, discarding those who were perceived as fraudulent, less powerful, or illegitimate. Using this meticulous approach, we selected one practitioner from each of the three study districts. Each of these chosen practitioners was well-known and respected within their district. The broader local population was not informed about our selection process, rather they were just informed that a local practitioner would be providing the spells.

The Treatment: Traditional Protection Rituals

Each selected practitioner was contracted to provide protective spells to the beer sellers, aiming to decrease the likelihood of theft at their establishments, conditional on adherence to specific behavioral prescriptions.[9]

To receive the protective spell, each seller participated in an ancestral ritual conducted by one of the traditional practitioners. These rituals, which lasted a few hours, involved chants and movements aimed at invoking deceased ancestors. The practitioner, acting as a messenger, sought permission from the ancestors to infuse objects with supernatural powers, following the customs of the local ethnic group. To ensure privacy, the rituals were conducted outside the village, in the nearest urban center. Each seller was transported to the ritual location a day before the ceremony, participated in the ritual, and then returned to their village either on the same day or the day after. Given the chosen locations and the discreet transportation arrangements, it is unlikely that other villagers knew that the sellers underwent the rituals.

The conditions for the practitioners that we selected for the experiment were: to sweep the shop before installing the gri-gri and to visit the practitioner for a re-installment if it fails to work; to not steal from or fight with anyone; to place the amulet in the shop under a secured cupboard and to not tell anyone about the hidden amulet; and to not sleep with a woman who is menstruating during the days of installation of the amulet.

Assignment to the Treatment

Each seller attended a ritual, but the order in which the rituals occurred was randomized. The sellers were treated over approximately thirty days from July 16th to August 14th, 2017. Treatment

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[9]In all three districts, the object protected against theft, while in Walikale, the object also provided good luck, which could result in increased sales. The exact nature of the benefits was determined by the abilities of the traditional practitioners and the bundle of benefits that were provided by their ancestors.

Figure 2: Share of sellers that had attended the ritual by day.

Share of beer sellers
who attended the ritual (in %)
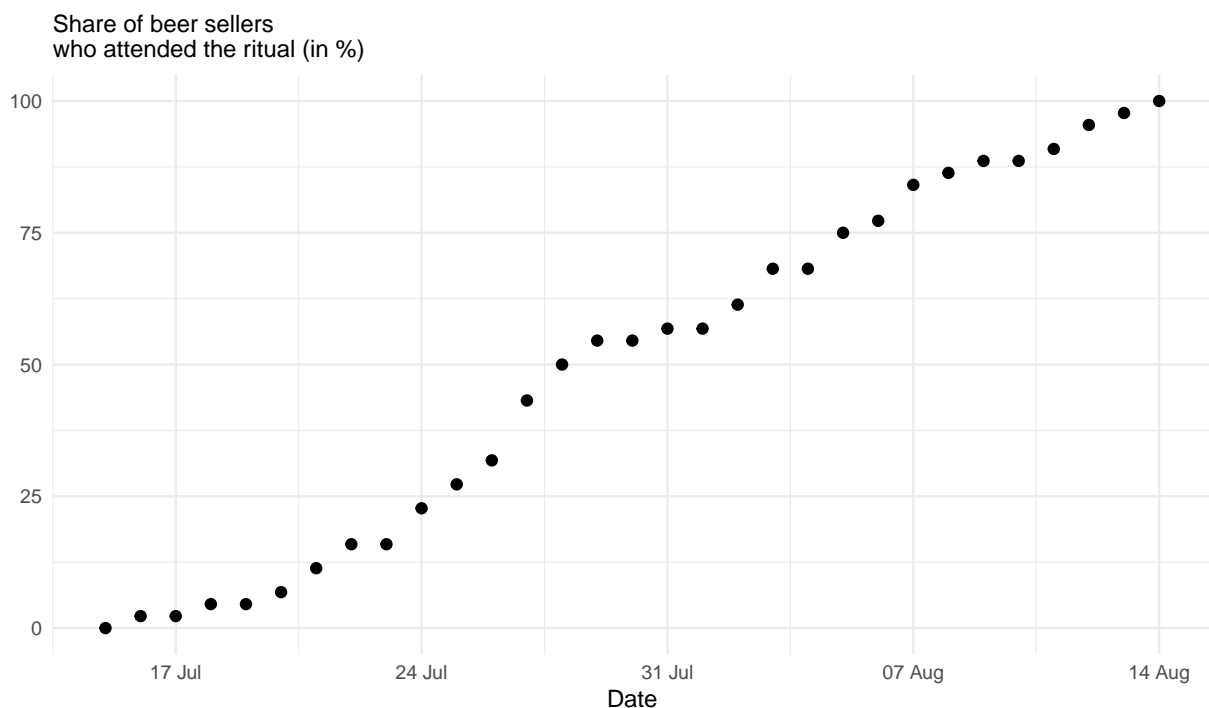
Notes: The graph shows that the roll out of the treatment occurred smoothly during one month.

occurred in nearby cities in each of the three regions.[10] On each day, one seller was assigned to receive treatment from the traditional practitioner.

Treatment assignment was generally implemented as planned, with a few exceptions. For example, due to security considerations, for some sellers, the treatment had to be delayed by a day, or multiple rituals had to be performed in one day. All violations of the randomization protocol are documented in Appendix B. Figure 2 reports the fraction of all sellers that were treated each day. The assignment to treatment occurred smoothly over one month. Summary statistics for the sample are reported in Appendix Table A1.

Multiple Hypotheses Testing

Given that our analysis tests four hypotheses – (i) theft expectation and stress, (ii) inventory management, (iii) sales, and (iv) pricing – it is possible that any observed significant estimates could be attributed to chance rather than the actual treatment effect. Given this, when reporting our estimates, we report significance levels that are adjusted using a Bonferroni correction.[11]

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[10]The cities were Masisi Centre Kibabi, and Rubaya Centre in Masisi; Vitchumbi, Kanyabayonga, Rubare, Kibirzi, and Kiwanja in Rutshuru; and Itebero, Makana, Ndjingala, and Walikale Centre in Walikale.

[11]The method adjusts the significance level by dividing it by the number of tests, four in our case.

4. Results

This section reports our findings on the causal impact of ritual participation on sellers' beliefs in their supernatural protection from theft, perceived stress levels, associated economic choices, and their business performance. A variety of strategies can be used to analyze the experimental design with randomly staggered treatment. To ensure robustness and accuracy of our results, we employ five distinct econometric strategies, each shedding light on different facets of the data.

A. *Event Study Estimates*

We begin the analysis with an event study design that examines the patterns in the raw data before and after each seller's ritual. Our interest is in whether participation in the rituals affects beer sellers' beliefs in supernatural protection and their expectations regarding the probability of future theft, which, in turn, may impact the quantity of bottles purchased and held as inventory. Since these decisions are not typically made daily, but weekly (on average), we aggregate the daily measure to the week (i.e., seven-day) level in the event-study analysis. The event study is also instrumental in examining the possibility of anticipation effects. Beer sellers were notified about the experiment around 22 June 2017, 24 days before the first ritual was performed. If anticipation effects play a significant role, this should be evident in the event study.

Let i index beer sellers, t index the periods comprised of six seven-day periods (42 days) prior to the ritual and six seven-day periods (42 days) following the ritual. We then estimate the following equation:

$$Y_{it} = \sum_{j=-6}^{6} \beta_j^{es} I_t^j + \varepsilon_{it}, \tag{1}$$

where Y_{it} denotes an outcome of interest for beer seller i during period t. The twelve coefficients β_j^{es} are the estimates of interest, which capture the seven-day means of the respective outcome before and after participating in the ritual. We use seller-clustered bootstrap standard errors based on 10,000 iterations and the Bonferroni correction based on four hypotheses to compute 95% confidence intervals.[12]

In specifications, where the dependent variables of interest are either revenues, bottles sold, bottles purchased, bottles in stock, or sale prices of bottles sold, the outcomes are also vary at

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[12]Some beer sellers hold systematically more inventory and have more revenues than others. Therefore, we also estimate event-study regressions where we include beer seller fixed effects that filter out constant differences across the sellers. We report these results in Appendix Figures A6, A7 and A8.

the brand level. Since some brands can be more or less expensive than others, in these cases, we estimate a variant of equation (1) where an observation is a seller i, a brand b, and a day t. We also apply the inverse hyperbolic sine (IHS) transformation to revenues, sales, and inventories, since these are highly right skewed otherwise. As we discuss below, none of our findings depend on this decision.

The data reveal that out of the 44 sellers in our sample, 14 (31.8%) did not have an active amulet in the 75 days prior to the experiment, while 30 (68.2%) reported having an active amulet for at least one day before the experiment. These percentages align with the survey evidence detailing the prevalence of such beliefs, as shown in Appendix Figure A1. Therefore, we divide our sample into two groups: believers and non-believers of the efficacy of protection rituals. We then estimate equation (1) separately for each group. This approach allows us to better understand whether any effects that we find of the rituals on outcomes is due to the perceived protection of the rituals rather than the experience and logistics involved in participating in the ritual – e.g., traveling to the nearby city, being away from their shop, etc. While all participants in our study experience the logistical aspects of the treatment, only those who believe in the rituals' efficacy beforehand are likely to be affected by the perceived protection offered by the ritual.

While the presence or absence of an active amulet before the experiment was not randomized, our analysis suggests minimal systematic differences between the two groups concerning our outcome variables collected before the rituals were rolled out. As reported in Table 1, eight of the nine variables are balanced. The only exception is theft expectation, which is higher among believers compared to non-believers.

Perceived Protection from Theft

The first outcome we examine is people's beliefs about whether they feel protected by an amulet carrying supernatural powers. This outcome is measured using a question that asks: "*Do you have any fetish in use today?*" and they choose "yes" or "no." This step can be viewed as a zero-stage estimate or a check on the validity of our treatment.

There are many reasons that people may not believe they are protected despite undergoing a protection ritual. For example, they may not believe that the traditional practitioner was legitimate. Alternatively, they could believe in the ritual but were unable to follow the conditions necessary for it to remain in effect.

Table 1: Balance in pre-experimental covariates and outcomes between believers and non-believers

Pre-study outcome	Mean	Estimate	95% CI
Male indicator	0.900	-0.003	[-0.238, 0.334]
Revenues (IHS USD)	2.901	-0.657	[-1.912, 0.634]
Bottles sold (IHS #)	2.679	-0.549	[-1.812, 0.776]
Bottles purchased (IHS #)	0.872	-0.302	[-1.229, 0.499]
Bottles in stock (IHS #)	3.780	0.291	[-1.830, 2.641]
Stock-outs	0.136	-0.006	[-0.194, 0.146]
Theft expectations [0-1]	0.027	0.145	[-0.029, 0.326]
Theft indicator	0.028	-0.013	[-0.070, 0.021]
Stress level [1-3]	1.066	0.183	[-0.066, 0.435]

Notes: The table presents coefficients for nine OLS regressions of pre-determined characteristics and pre-experimental outcomes on the believer indicator, i.e., an indicator equal to one if the beer seller reports using an active amulet during at least one day before the start of the experiment. All regressions also include date fixed effects. The characteristics include beer sellers' gender and the outcome variables of the analysis but measured between May 1st to July 15th, 2017, before the start of the experiment. 95% confidence intervals computed using seller-clustered bootstrap standard errors based on 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses.

The effectiveness of the rituals is summarized in Figure 3, which shows the event-study estimates among believers (left panel) and non-believers (right panel). The days are measured as days before and after ritual attendance. Four facts are apparent: First, some sellers (about 15% among believers on average) have an active supernatural protection amulet before treatment. Second, there is a noticeable increase in the proportion of believers with an active amulet and thus feel protected following their ritual attendance. Third, there is no such increase among non-believers. Fourth, the share of believers feeling protected does not increase to one hundred percent, suggesting that only some trust in the effectiveness of the ritual.

Appendix Figure A5 reports the same data, but using daily data and focusing on the seven days before and after the ritual. It shows that perceived protection jumps on precisely the day the rituals were held and persists afterwards.

Theft Expectations and Perceived Stress

Next, we look at beer sellers' perceived expectations that they will be robbed. These expectations are measured using three questions that ask: (*i*) *"Do you think that a theft will occur tomorrow?"*, (*ii*) *"Do you think that a theft will occur next week?"*, (*iii*) *"Do you think that a theft will occur next*

Figure 3: Event-study estimates of the effects on perceptions of protection
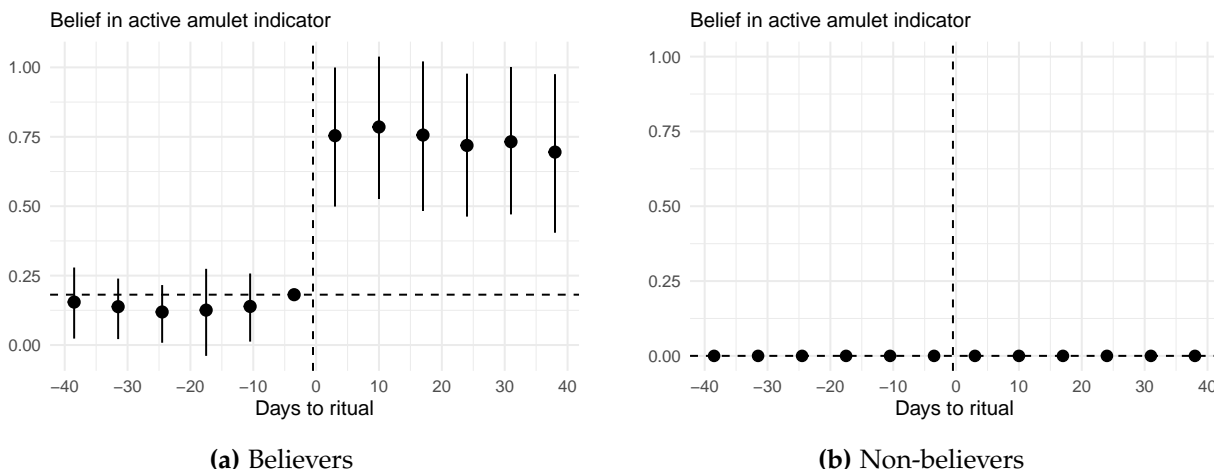
(a) Believers   **(b)** Non-believers
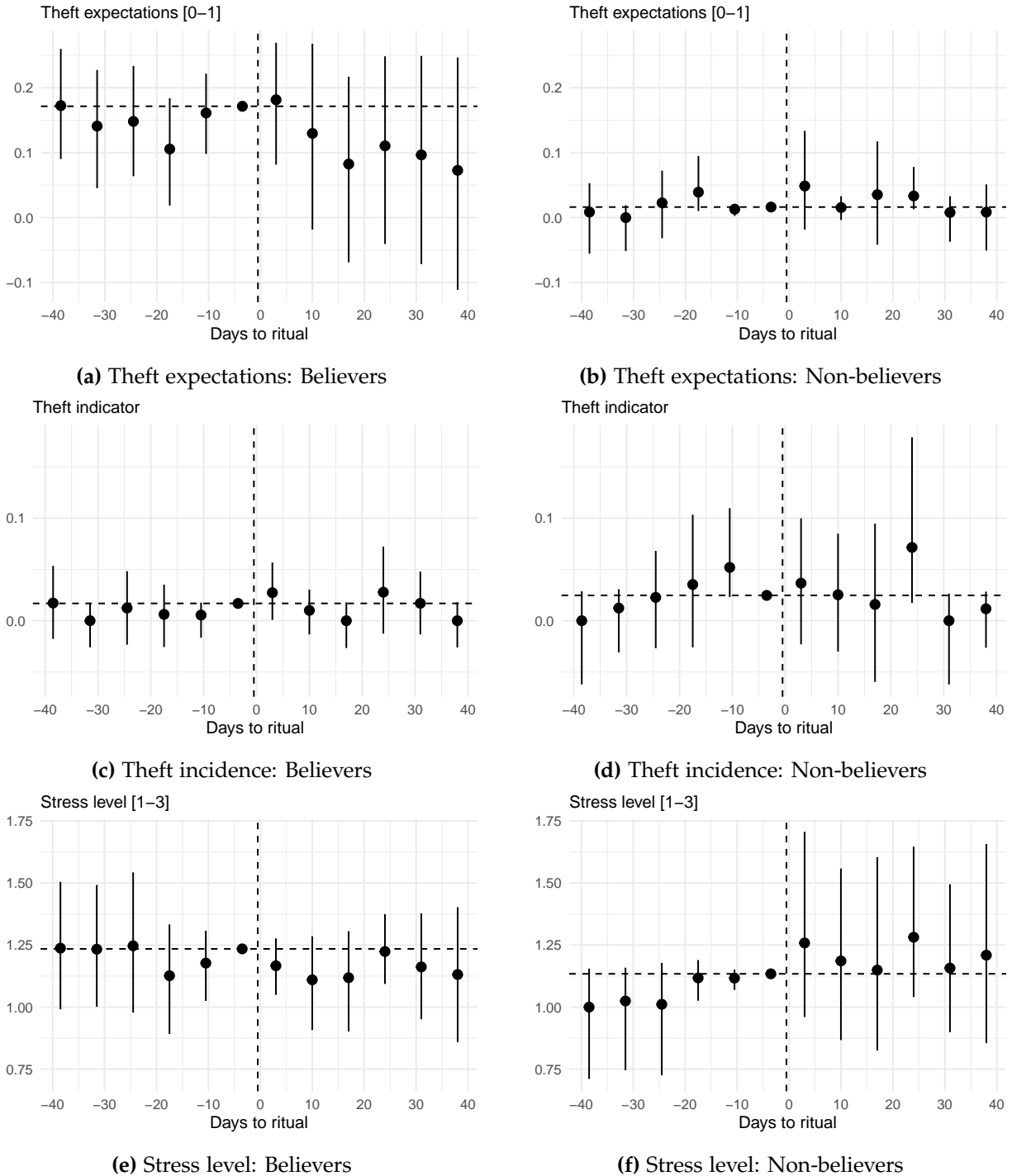
Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of the perceived protection indicator on weeks-to-ritual dummies (each dot represents seven days), the β_j's of equation (1). Left panel: believers; right panel: non-believers. We define believers (non-believers) as sellers that ever (never) reported believing in an active amulet before the start of the experiment. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The dotted line indicates the value of the perceived protection indicator during the week before the rituals were conducted.

month?", and they choose "yes" or "no." We compute indicators that equal one if the sellers answer "yes" and average over the three indicators. The event study estimates with this index are displayed in Figure 4. Consistent with believers feeling protected, we find that beer sellers who believe in supernatural protection are less likely to expect theft after the ritual than before the ritual (Panel a). The effect takes time to arise: It is not present in the first week after the ritual but evolves over the following weeks. It is strongest in the third week following the ritual, and continues to persist after. The effect size is also substantial. Theft expectations are roughly cut in half after the ritual compared to before. None of these effects are found among non-believers (Panel b). If anything, non-believers are more likely to expect theft the week after the rituals, possibly because they left their shops for one or two days to travel to the locations of the rituals.

We also report the estimated effects of the ritual on actual theft among believers in Figure 4c and among non-believers in Figure 4d. This analysis provides a placebo test (assuming the ritual does not work). It is also useful as evidence for whether there are experimenter demand effects, which generate reporting bias. If so, we might expect individuals to be less likely to report actual thefts that occur so as not to disappoint us. Reassuringly, we find no evidence that the ritual affects the actual incidence of theft.

Next, we examine the effects of the rituals on a self-reported measure of stress. Respondents

Figure 4: Event-study estimates of the effects on theft beliefs and perceived stress

(a) Theft expectations: Believers

(b) Theft expectations: Non-believers

(c) Theft incidence: Believers

(d) Theft incidence: Non-believers

(e) Stress level: Believers

(f) Stress level: Non-believers

Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of three outcomes on weeks-to-ritual dummies (each dot represents seven days), the β_j's of equation (1). The outcomes are our theft expectations index, as defined in the text, theft incidences and stress level. Left panels: believers; right panels: non-believers. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The dotted line indicates the value of the outcome during the week before the rituals were conducted.

are asked, "What is your stress level today?" They then rate their stress level from 0 (no stress) to 10 (high stress). Since the responses cluster on 0, 5, and 10, we recode the responses to low (1), medium (2), and high (3). Consistent with previous findings in the literature, we find a negative effect of the ritual on self-reported stress among believers (Panel e). However, these estimates are very small in magnitude, especially when compared to the effects on theft expectations. We find no evidence that the rituals helped to reduce self-reported stress among non-believers (Panel f). If anything, the ritual increased their stress level, though these estimates are also small in magnitude and imprecise.

Purchases, Inventory, and Stock-outs

The findings of increased perceived protection and decreased expectations about future theft raise the question of whether the rituals also affect economic choices. Specifically, we test whether the rituals have an impact on the purchases of new bottles, bottles held in stock, and the incidence of stock-outs, defined as the share of brands with zero inventory at the end of the day.[13] Since the rituals decrease beer sellers' perceived probability of being robbed, this might increase the stock of inventories that sellers feel is safe to hold, which, in turn, could reduce the intensity of stock-outs.
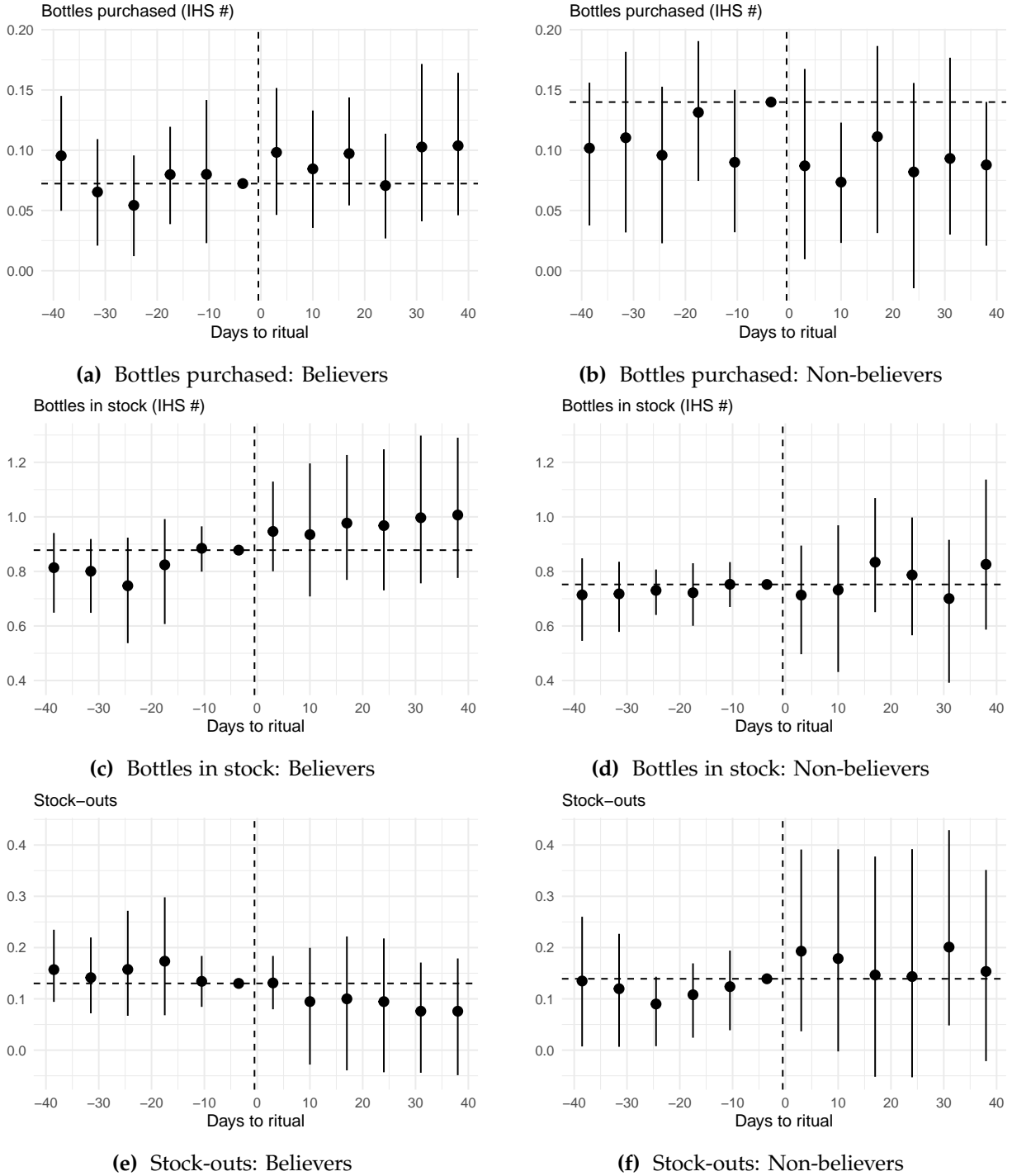
The event study estimates of these outcomes are shown in Figure 5 . We find that beer sellers who believe in supernatural powers buy more bottles in the weeks after the ritual than before (Panel a), while there is no such effect among non-believers (Panel b). The estimates of inventory and stock-outs show similar patterns. Believers hold more bottles in stock in the weeks after the ritual (Panel c) and suffer fewer stock-outs (Panel e) than before, while we find no effects on bottles held in stock (Panel d) or stock-outs (Panel f) among non-believers.

Revenues, Bottles Sold, and Sale Prices

The findings of increased inventories and decreased stock-outs raise the question of whether the rituals also affect sellers' business performance. To shed light on this question, we also test for an effect of the rituals on beer sellers' revenues. Given that the ritual increased beer sellers'

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[13]Since some sellers hold and sell only a small number of brands during our data collection, we consider brands that the beer seller ever held or sold to compute this share.

Figure 5: Event-study estimates of the effects on purchases, inventories, and stock-outs

(a) Bottles purchased: Believers

(b) Bottles purchased: Non-believers

(c) Bottles in stock: Believers

(d) Bottles in stock: Non-believers

(e) Stock-outs: Believers

(f) Stock-outs: Non-believers

Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of outcomes on weeks-to-ritual dummies (each dot represents seven days), the β_j's reported equation (1). The outcomes are bottles purchased, bottles held in stock and stock-outs, defined as the share of brands with zero bottles in stock at the end of the day (see text). Left panel: believers; right panel: non-believers. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The horizontal dotted line indicates the value of the outcome during the week before the rituals were conducted.

inventories and reduced the intensity of stock-outs, it might expand sellers' ability to sell more bottles, which, in turn, could increase revenues.

We report these estimates in Figure 6. We find that revenues increase in the weeks after the ritual among believers (Panel a), but not among non-believers (Panel b). Among believers, the effect kicks in the days following the ritual and persists.

We disaggregate the estimated effect on revenues into quantities and prices by separately examining the number of bottles sold (Panel c and Panel d) and the unit price of bottles sold (Panel e and Panel f). The results show that the increase in revenues is mainly due to higher sales and not prices. Among believers, we find positive effects on bottles sold in the weeks after the ritual, with the magnitudes of the effects being similar to the effect sizes on revenues, and positive but very small effects on prices. Among non-believers, we find no effects on quantities sold and positive but small effects on sales prices.

How is it possible that the effects on revenues, purchases, and inventories kick in within a few days after the rituals, while the effect on theft expectations takes a week to evolve? Our local team made weekly calls to each seller to discuss the data entered. During these calls, the team member also learned that many sellers had doubts about the effectiveness of the ritual initially and therefore experimented with limited additional purchases, which in the absence of theft, then led to increases in sales. After observing this success, sellers further increased their inventories, which decreased the frequencies of stock-outs substantially.
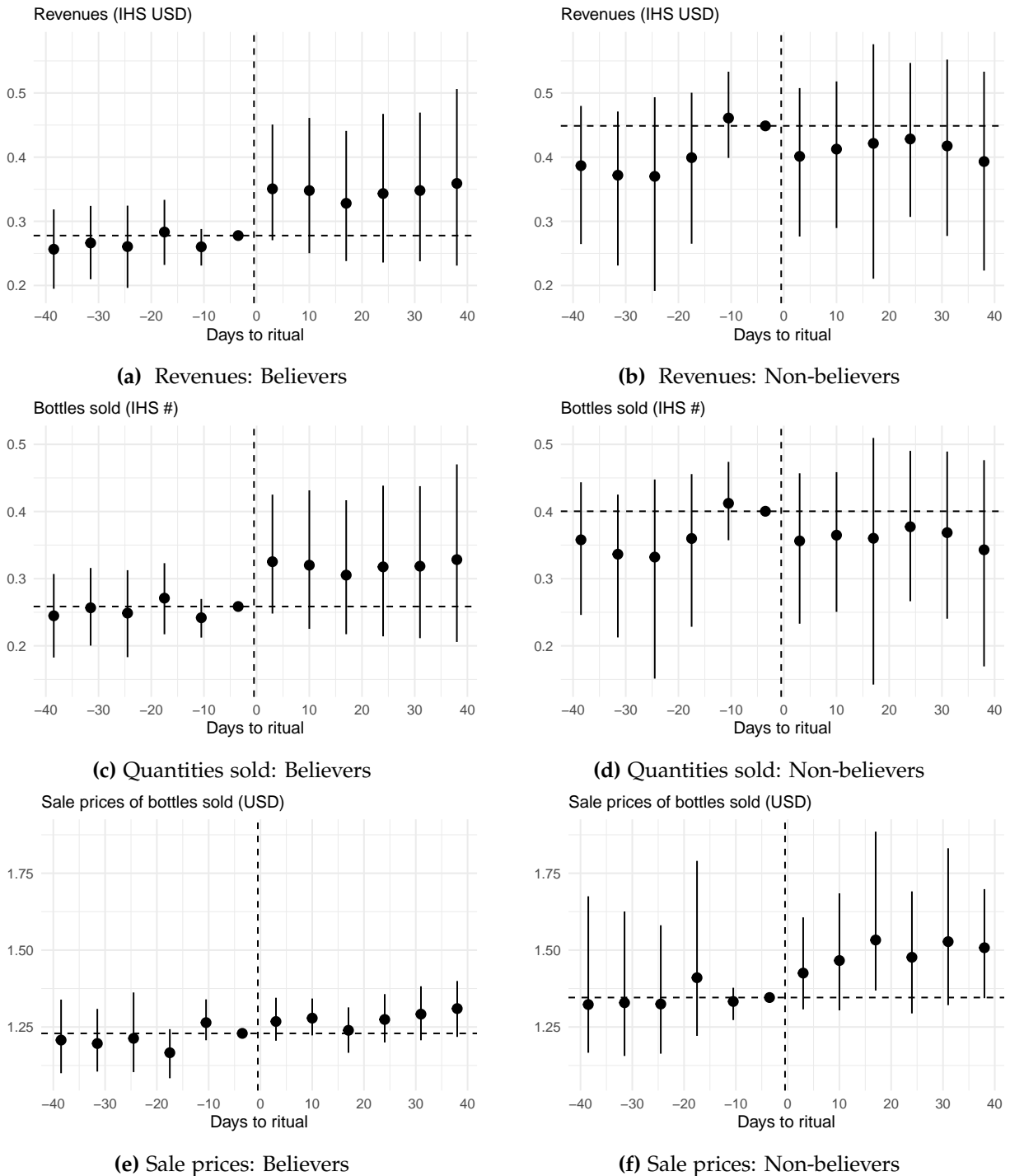
B. *Two-Way Fixed Effects, Callaway and Sant'Anna, and Waitlist Design Estimates*

We now turn to more formal estimates of treatment effects, using three different estimators: two-way fixed effects, Callaway and Sant'Anna (2021), and a waitlist design. We now describe each in turn, beginning with the two-way fixed effects (TWFE) estimator:

$$Y_{it} = \alpha_i + \alpha_t + \beta^{twfe} D_{it} + u_{it}, \qquad (2)$$

where as before, i indexes beer sellers. Now, t indexes days from the first (July 16th) to the last (August 14th) day that beer sellers participated in the rituals. Y_{it} is one of our outcomes of interest;

Figure 6: Event-study estimates of the effects on revenues, bottles sold and sale prices

(a) Revenues: Believers

(b) Revenues: Non-believers

(c) Quantities sold: Believers

(d) Quantities sold: Non-believers

(e) Sale prices: Believers

(f) Sale prices: Non-believers

Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of three outcomes on weeks-to-ritual dummies (each dot represents seven days), the β_j's reported equation (1). The outcomes are our revenues, bottles sold and sale prices of bottles sold. Left panel: believers; right panel: non-believers. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The dotted line indicates the value of the outcome during the week before the rituals were conducted.

D_{it} is an indicator equal to one the day after[14] seller i receives the ritual.[15] The parameter α_i denotes seller fixed effects, which account for constant unobserved differences across the sellers; α_t denotes date (i.e., day) fixed effects, which flexibly capture any differences over time, including differences in the region's insecurity or demand for beverages. The coefficient of interest is β^{twfe}, the effect of attending the ritual on the outcome of interest. For inference, we compute seller-clustered and Bonferroni corrected bootstrap 95% confidence intervals based on 10,000 iterations and four hypotheses.

Our second estimator is motivated by recent findings showing that standard two-way fixed effects estimates with staggered treatment timing may not provide reliable estimates of the causal effects of interest, even when the treatments are randomly assigned. To address this issue, several alternative estimators have been proposed (Roth, Sant'Anna, Bilinski and Poe, 2022). Given our experimental design, in which a never-treated group is unavailable, the procedure developed by Callaway and Sant'Anna (2021) is particularly appropriate as it allows us to compare the treated observations to all not-yet-treated observations, setting the the not-yet-treated observations as the control group. We calculate cohort-specific time average effects, as recommended by Callaway and Sant'Anna (2021), to summarize the overall effect of the rituals.[16] We report seller-clustered bootstrapped confidence intervals based on 10,000 iterations and corrected using the Bonferroni based on four hypotheses.

Our third estimator draws from a standard design from the medical literature. The set-up of the experiment, where all participants eventually receive treatment but at different randomly-determined times, is common in the medical field where it is often not ethical to withhold treatment from a control group. Instead, as in our design, all participants receive treatment, but in a random order. A key aspect of waitlist estimators is driven by the fact that the probability of assignment to treatment varies each day throughout the experiment, as the pool of treated and untreated participants changes. For example, the probability that an untreated participants receives treatment is higher during the last days of the experiment (when there are fewer untreated participants) than during the first days (when there are more). Thus, to obtain

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[14] Our baseline measure takes on the value of one on the day that the ritual occurs. The results are very similar if we drop the days on which the rituals took place from the analysis (see Appendix Table A2).

[15] As noted, logistical and security issues caused minor deviations from the randomized protocol on the actual days the rituals could be performed in some cases. Given this, we also report estimates of regressions in which we instrument actual ritual attendance D_{it} with the randomized ritual assignments, denoted by Z_{it}. These are referred to as IV estimates and are reported in Appendix B.

[16] Our implementation relied on the *did* package in R, allowing for an unbalanced panel, to estimate the models.

unbiased estimates of the average treatment effects, one needs to weight the treated observations by the inverse probability of being assigned to the treatment group, and the control observations by the inverse probability of being assigned to the control group. The weighting accounts for the time-varying aspect of treatment assignment propensities (see Gerber and Green (2012) for full details).

The waitlist design estimation strategy is given by the following equation:

$$Y_{it} = \gamma_i + \beta^{wl} D_{it} + \varepsilon_{it}, \tag{3}$$

where, as before i indexes sellers, t days, Y_{it} denotes an outcome of interest, and D_{it} is the post-ritual indicator. The parameter γ_i denotes seller fixed effects. The coefficient of interest is β^{wl}. As before, we compute seller-clustered and Bonferroni corrected bootstrap 95% confidence intervals based on 10,000 iterations and four hypotheses.

When the dependent variable of interest is revenues, bottles sold, bottles purchased, bottles in stock, or sale prices of bottles sold, we estimate a variant of our equations – namely, equations (2) and (3) and the Callaway and Sant'Anna (2021) estimator – where an observation is a seller i, a brand b, and a day t. We then account for constant observed differences across the brands each seller holds by including beer seller-brand fixed effects, i.e., α_{ib} or γ_{ib}, which absorbs the seller fixed effects.

Estimates for each of our three empirical strategies are reported in Table 2. The TWFE estimates are reported in Panel A, the Callaway and Sant'Anna (2021) estimates in Panel B, and the waitlist design estimates in Panel C.

Column 1 reports the estimated effect of the ritual on the belief in an active amulet indicator variable among the sample of believers. All three panels show that beer sellers who are believers feel more protected following the ritual. We cannot estimate this coefficient among non-believers because non-believers always respond "no" to the question of interest. In this sense, for this group, the ritual did not have an effect.

The estimates of the effect of the ritual on theft expectations are reported in columns 2 (for believers) and column 5 (for non-believers). We find that the rituals lower theft expectations among the believers. The magnitude of the estimated effect is sizable. The ritual is found to result in a reduction in average theft expectations by 2.4–4.8 percentage points or 18–37 percent relative to the control mean. However, the estimates lack precision and are not statistically significant at

Table 2: Estimates of the effects on beliefs and emotions

	Believers				Non-Believers		
	Belief in active amulet indicator	Theft expectations [0-1]	Theft indicator	Stress level [1-3]	Theft expectations [0-1]	Theft indicator	Stress level [1-3]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Two-way fixed effects						
Post-Ritual	0.558	-0.024	0.009	0.001	0.037	0.015	0.122
95% CI	[0.262, 0.846]	[-0.118, 0.067]	[-0.036, 0.050]	[-0.214, 0.265]	[-0.026, 0.130]	[-0.054, 0.107]	[-0.224, 0.678]
	Panel B: Callaway and Sant'Anna (2021)						
Post-Ritual	0.383	-0.048	-0.008	-0.048	0.047	0.128	0.047
95% CI	[0.232, 0.533]	[-0.131, 0.035]	[-0.083, 0.066]	[-0.295, 0.198]	[-0.005, 0.099]	[-0.129, 0.386]	[-0.295, 0.390]
	Panel C: Waitlist design						
Post-Ritual	0.536	-0.045	-0.002	-0.089	0.036	0.007	0.106
95% CI	[0.240, 0.825]	[-0.184, 0.071]	[-0.024, 0.019]	[-0.295, 0.050]	[0, 0.101]	[-0.032, 0.056]	[-0.133, 0.471]
	All panels:						
Control mean (sd) DV	0.14 (0.34)	0.13 (0.31)	0.01 (0.11)	1.20 (0.55)	0.01 (0.06)	0.03 (0.17)	1.10 (0.33)
Observations	818	788	797	810	351	333	352

Notes: This table presents ATT estimates of the effects of the ritual on the belief in active amulet indicator, theft expectations, theft indicator, and reported stress level. The unit of observation is beer seller by day. The main independent variable is an indicator taking value from the day an individual beer seller attended the ritual, and zero otherwise. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Columns 1-4 (5-7) show the results for the believer (non-believer) sample. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3). Means and standard deviations of the dependent variables are computed based on untreated observations.

conventional levels. The lack of significance is likely due to the low mean of theft expectations among untreated observations (0.13 among believers).

We find that ritual increases the expectations of being robbed among non-believers in supernatural powers (column 5). The effect is again sizable and statistically significant at conventional levels for two of three estimation strategies. We also note that the mean value reported by non-believers during the days before the rituals is 0.01, a very low level compared to the findings from our baseline data collection. For completeness, we also report the estimated effects of the ritual on actual theft in columns 3 and 6. We find no evidence that the ritual affects the actual incidence of theft.

In columns 4 and 7, we report the estimates with perceived stress as the dependent variable. We find a negative effect of the ritual on self-reported stress among believers (column 4). However, the estimates are statistically insignificant and very small in magnitude. Similar to the case of theft expectation, this lack of statistical significance might be because baseline levels of reported stress tend to be low. The mean value reported is only 1.20 (on a 1-3 scale) among believers. We find no evidence that the rituals helped to reduce self-reported stress among non-believers. If anything, the ritual increased their stress level, though the point estimates are also small in magnitude and

not statistically significant.

To understand and interpret these estimates, one needs to grapple with the particular challenges associated with conducting research in the Democratic Republic of Congo (DRC). Many individuals in our research setting do not have a nuanced understanding of the mathematical concept of probability. As such, our data collection methods had to be carefully adapted. We had to limit our questioning about future events to very specific, short-term time frames such as tomorrow, next week, or next month. Further, we had to restrict response options to binary choices: yes or no. This lack of probabilistic precision has likely worked against finding a statistically significant effect.

In addition, the concept of stress is complex and often misunderstood in the context of the DRC. The idea of quantifying something as subjective and personal as stress is itself a challenge, and this is amplified by cultural differences in the perception and understanding of what stress truly means. Therefore, the measurement of self-reported stress levels, as shown in our estimates, is bound to be less precise. Thus, the results on theft beliefs and perceived stress should be viewed through the lens of these contextual difficulties. The estimates provided are not perfect, but they represent our best attempt to quantify these effects given the significant constraints we faced.

We now turn to the effects of the protection rituals on economic outcomes. The estimated effects on the purchases, inventories, and the frequency of stockouts are reported in Table 3. Among the believers, we find that the ritual led to increased purchases of bottles (column 1), more bottles held in stock (column 2), and fewer stockouts (column 3). In most specifications, the magnitudes are large and the estimates are statistically significant. The coefficients for stockouts imply that rituals reduced the occurrence of stockouts by 1.6 to 6.3 percentage points, which is a very large effect, especially when compared to the mean occurrence of stockouts among untreated units, which is 13%. As reported in columns 4–6, we do not find these effects for non-believers. The estimates are close to zero, actually opposite in sign, and always statistically insignificant.

The estimated effects of the rituals on revenues are reported in columns 1 and 4 of Table 4.[17] Among the believers, we find a positive and statistically significant effect of the ritual on revenues across our empirical strategies. The magnitude of the effect ranges from an approximately 5–11

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[17]As noted, since right skewness is present in the raw data for revenues and bottles sold, we take the inverse hyperbolic sine of the measures and use this as our dependent variables. Our findings do not depend on this decision. Estimates using untransformed outcomes are reported in Appendix Tables A3 and A4.

Table 3: Estimates of the effects on purchases, inventories, and stock-outs

	Believers			Non-Believers		
	Bottles purchased (IHS #)	Bottles in stock (IHS #)	Stock-outs	Bottles purchased (IHS #)	Bottles in stock (IHS #)	Stock-outs
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Two-way fixed effects					
Post-Ritual	0.041	-0.017	-0.016	-0.066	-0.088	0.048
95% CI	[-0.035, 0.123]	[-0.156, 0.101]	[-0.097, 0.073]	[-0.161, 0.034]	[-0.407, 0.169]	[-0.154, 0.270]
	Panel B: Callaway and Sant'Anna (2021)					
Post-Ritual	0.170	0.201	-0.063	-0.012	-0.130	0.041
95% CI	[0.081, 0.259]	[0.017, 0.384]	[-0.129, 0.003]	[-0.103, 0.079]	[-0.187, -0.072]	[-0.045, 0.128]
	Panel C: Waitlist design					
Post-Ritual	0.036	0.062	-0.053	-0.039	-0.030	0.049
95% CI	[-0.001, 0.079]	[-0.033, 0.159]	[-0.136, 0.008]	[-0.095, 0.024]	[-0.176, 0.102]	[-0.068, 0.218]
	All panels:					
Control mean (sd) DV	0.07 (0.54)	8.20 (34.24)	0.13 (0.27)	0.13 (0.71)	6.61 (30.07)	0.12 (0.22)
Observations	12,394	12,617	760	5,494	5,223	293

Notes: This table presents ATT estimates of the effects of the ritual on number of bottles purchased, number of bottles in stock, and share of brands that are out of stock, as defined in the text. The first two outcomes are inverse hyperbolic sine (IHS) transformed. The unit of observation is beer seller by beer variety by day, except in columns 3 and 6, where it is beer seller by day. The main independent variable is an indicator taking value from the day an individual beer seller attended the ritual, and zero otherwise. Columns 1–3 (4–6) show the results for the believer (non-believer) sample. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3). Means and standard deviations of the dependent variables are computed based on untreated observations.

percent increase after the ritual. Given that daily revenues among untreated believers amount to $10.90 on average, the effect implies around $0.55 to $1.20 more revenue each day after the ritual for the average beer seller who believes in the protection.

We disaggregate the estimated revenue effect into quantities and prices by separately examining the effects on the number of bottles sold (columns 2 and 5) and on the price of bottles sold (columns 3 and 6). The results show that the increase in revenues is mainly due to higher sales and not increased prices. Although we find a positive and statistically significant effect on prices in the waitlist design, the magnitude of this effect is much smaller than the effect on the quantity sold. The estimated effects for price are reassuring since market prices fluctuate very little and the vendors tend to be price takers. If we saw large effects working through prices, we might be worried that optimistic reporting, influenced by the ritual treatment, may be driving the effect on revenues. As reported in columns 4–6, reassuringly, none of these effects are found among our sample of non-believers.

Table 4: Estimates of the effects on revenues, bottles sold, and sale prices

	Believers			Non-Believers		
	Revenues (IHS USD) (1)	Bottles sold (IHS #) (2)	Sale prices of bottles sold (USD) (3)	Revenues (IHS USD) (4)	Bottles sold (IHS #) (5)	Sale prices of bottles sold (USD) (6)
	Panel A: Two-way fixed effects					
Post-Ritual	0.050	0.046	-0.009	-0.024	-0.017	0.016
95% CI	[-0.002, 0.106]	[-0.001, 0.094]	[-0.054, 0.043]	[-0.118, 0.053]	[-0.101, 0.057]	[-0.054, 0.090]
	Panel B: Callaway and Sant'Anna (2021)					
Post-Ritual	0.112	0.106	0.032	-0.024	-0.021	0.173
95% CI	[0.021, 0.204]	[0.018, 0.195]	[-0.349, 0.413]	[-0.117, 0.069]	[-0.104, 0.063]	[-0.468, 0.815]
	Panel C: Waitlist design					
Post-Ritual	0.048	0.043	0.025	-0.020	-0.019	0.068
95% CI	[0.005, 0.098]	[0.003, 0.084]	[0.002, 0.058]	[-0.107, 0.054]	[-0.107, 0.054]	[-0.028, 0.163]
	All panels:					
Control mean (sd) DV	0.26 (0.75)	0.25 (0.71)	1.24 (0.64)	0.41 (0.99)	0.36 (0.89)	1.40 (0.53)
Observations	12,936	12,936	3,905	5,627	5,627	1,531

Notes: This table presents ATT estimates of the effects of the rituals on revenues (converted to USD), number of bottles sold, and sale prices of bottles sold (converted to USD). Revenues and number of bottles sold are inverse hyperbolic sine (IHS) transformed. The unit of observation is beer seller by beer variety by day. The main independent variable is an indicator taking value from the day an individual beer seller attended the ritual, and zero otherwise. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3). Control means and standard deviations of the dependent variables are computed based on untreated observations. The number of observations for sale prices of bottles sold is lower than for the other outcomes because price data is missing when a particular beer brand is not sold on a given date.

While the beer sellers' primary sales item is beer, they also stock and sell local soft drinks. The estimates reported to this point have included all non-alcoholic beverages. We check the robustness of our estimates to distinguishing between beer and soft drinks. Ex ante, we do not have any reason to expect that one would be affected more than another. Both beverages are valuable and can be stolen. The estimates, which are reported in Appendix Table A5, show that we obtain similar estimates for soft drinks and beer.

C. *Difference-in-Differences Estimates: Believers vs. Non-Believers*

To this point, our analysis has distinguished between the effects of rituals on believers and non-believers. The latter provides a helpful comparison group. In particular, since the ritual treatment is a bundle, that includes not only perceived protection, but also leaving one's village and traveling to the local city, which might have its own effects. Thus, comparing the estimated effects between believers and non-believers is helpful, since all experience the logistics and travel associated with the rituals, but only the believers experience the perceived protection from the ritual.

We now turn to a more formal treatment of the comparison between effects of the believers and non-believers by analyzing the data using difference-in-differences (DD) variants of our empirical specifications. That is, we pool the believers and non-believers samples and include an interaction of the treatment indicator with a $Believer_i$ indicator variable, which is equal to one for individuals who had an active amulet at least one day before the experiment began. This interaction equation allows us to estimate the differential effects of the rituals on believers and non-believers.

For the TWFE design, we estimate the equation as follows:

$$Y_{it} = \psi_i + \psi_t + \beta_1^{dd\,twfe}\, Believer_i \times D_{it} + \beta_2^{dd\,twfe} D_{it} + \nu_{it}, \tag{4}$$

where, as before, i indexes sellers, t days; Y_{it} is an outcome of interest; and D_{it} is the post-ritual indicator variable. The parameter ψ_i denotes seller fixed effects; ψ_t captures date fixed effects. The new variable, $Believer_i$, is an indicator variable that equals one if seller i is from our sample of believers. The variable is allowed to interact with the post-ritual indicator, $Believer_i \times D_{it}$, while its direct effect is absorbed by the seller fixed effects. The coefficient $\beta_2^{dd\,twfe}$ denotes the effect of participation in the ritual on the outcome of interest among non-believers. The coefficient of interest is $\beta_1^{dd\,twfe}$, which is the differential effect of ritual participation for believers relative to non-believers. We compute seller-clustered and Bonferroni corrected bootstrap 95% confidence intervals based on 10,000 iterations for inference and four hypotheses.

For the waitlist design, the equation is given by:

$$Y_{it} = \phi_i + \beta_1^{dd\,wl}\, Believer_i \times D_{it} + \beta_2^{dd\,wl} D_{it} + \omega_{it}. \tag{5}$$

where all variables are as defined in equation (4); ϕ_i denotes seller fixed effects. The coefficient $\beta_2^{dd\,wl}$ denotes the effect of participation in the ritual on the outcome of interest among non-believers. The coefficient of interest is $\beta_1^{dd\,wl}$, which is the differential effect of ritual participation for believers relative to non-believers.

In the case of the Callaway and Sant'Anna (2021) estimator, we estimate the differential effects of the ritual between believers and non-believers by treating non-believers as never-treated observations.

As before, when the dependent variable of interest is revenues, bottles sold, bottles purchased, bottles in stock, or sale prices of bottles sold, we estimate variants of equations (4) and (5) where an observation is a brand b, a seller i, and a day t, and the specification includes beer seller-brand fixed effects.

Identification in the difference-in-differences strategy rests on the parallel-trends assumption. We are unable to test for this assumption directly. However, we can assess the extent to which observable characteristics differ between believers and non-believers by regressing pre-experimental characteristics and business outcomes on the $Believer_i$ indicator and including date fixed effects. We report these results in Appendix Table 1. We find no systematic differences in these variables between believers and non-believers, lending empirical support for the validity of the difference-in-differences strategy. The notable exception to this finding are theft expectations, a point we will return to.

We report the DD estimates in Table 5. We find a substantial negative effect of rituals on theft expectations among believers relative to non-believers (column 1); no differential effect on actual theft (column 2); and a negative, but imprecisely estimated, effect on perceived stress (column 3). Moreover, we find precisely estimated positive effect of the ritual on bottles purchased (column 4); a positive and statistically significant effect of bottles held in inventory (column 5); and a negative and statistically significant effect on stock-outs (column 6). Finally, we find positive and significant effects on revenues and bottles sold (columns 7 and 8) and a negative but imprecisely estimated effect on prices (column 9). For completeness, we also report dynamic difference-in-differences estimates in Appendix A.

To understand the size of the effect on revenues, we multiply the estimated coefficients, which range from approximately 7–12 percent, with the average daily revenues among beer sellers who are untreated or do not believe in the ritual, which amount to $19.70 on average. Based on this calculation, the effect implies around $1.38 to $2.36 more revenue each day after the ritual for the average beer-seller who believes in the protection. These numbers are as large as the ones estimated in the previous section, and actually even larger, suggesting that the previous estimates, which examine the causal effects of a bundle of treatments, do not overstate the causal effects of the belief in theft protection. The fact that the DD estimates are larger than the baseline estimates is consistent with the other aspects of treatment having an opposite effect to the perceived protection. That is, being absent from the village for a period of time may have caused individuals to worry more about theft subsequently.

The estimates using the DD strategy confirm our baseline estimates. This is reassuring and helps us better understand mechanisms. Since the estimates identify an effect by comparing individuals, all of whom participated in the travel and logistics of the ritual, but only the 'treated'

Table 5: Difference-in-differences estimates (believers vs. non-believers)

	Theft expectations [0-1] (1)	Theft indicator (2)	Stress level (3)	Bottles purchased (IHS #) (4)	Bottles in stock (IHS #) (5)	Stock-outs (6)	Revenues (IHS USD) (7)	Bottles sold (IHS #) (8)	Sale prices of bottles sold (USD) (9)
			Panel A: Two-way fixed effects						
Post-Ritual	0.050	0.013	0.142	-0.031	-0.112	0.096	-0.016	-0.015	0.033
95% CI	[-0.030, 0.132]	[-0.034, 0.064]	[-0.190, 0.561]	[-0.096, 0.032]	[-0.313, 0.088]	[-0.062, 0.284]	[-0.106, 0.061]	[-0.102, 0.060]	[-0.059, 0.120]
Believer × Post-Ritual	**-0.077**	**-0.007**	**-0.152**	**0.066**	**0.104**	**-0.129**	**0.069**	**0.065**	**-0.049**
95% CI	[-0.224, 0.066]	[-0.055, 0.034]	[-0.502, 0.138]	[-0.001, 0.131]	[-0.080, 0.292]	[-0.315, 0.028]	[-0.021, 0.166]	[-0.020, 0.158]	[-0.139, 0.050]
			Panel B: Callaway and Sant'Anna (2021)						
Believer × Post-Ritual	**-0.058**	**-0.023**	**-0.050**	**0.106**	**0.171**	**-0.077**	**0.122**	**0.114**	**-0.027**
95% CI	[-0.140, 0.025]	[-0.057, 0.011]	[-0.204, 0.103]	[0.039, 0.172]	[-0.007, 0.348]	[-0.164, 0.009]	[0.033, 0.211]	[0.028, 0.200]	[-0.171, 0.117]
			Panel C: Waitlist design						
Post-Ritual	0.036	0.007	0.106	-0.039	-0.030	0.049	-0.020	-0.019	0.068
95% CI	[0.000, 0.100]	[-0.033, 0.056]	[-0.130, 0.464]	[-0.094, 0.024]	[-0.179, 0.105]	[-0.068, 0.222]	[-0.109, 0.054]	[-0.103, 0.054]	[-0.027, 0.164]
Believer × Post-Ritual	**-0.081**	**-0.009**	**-0.195**	**0.074**	**0.092**	**-0.102**	**0.068**	**0.063**	**-0.043**
95% CI	[-0.218, 0.052]	[-0.058, 0.034]	[-0.577, 0.099]	[0.005, 0.142]	[-0.062, 0.252]	[-0.267, 0.024]	[-0.018, 0.163]	[-0.017, 0.152]	[-0.129, 0.049]
			All panels:						
Control mean (sd) DV	0.08 (0.24)	0.02 (0.14)	1.19 (0.49)	0.09 (0.60)	0.79 (1.58)	0.14 (0.25)	0.34 (0.90)	0.31 (0.82)	1.33 (0.59)
Observations	1,139	1,130	1,162	17,888	17,840	1,053	18,563	18,563	5,436

Notes: This table reports difference-in-differences estimates of the ritual and its interaction with the believer dummy on all outcome variables. The unit of observation is beer seller by day in columns 1–3 and 6, and it is beer seller by beer variety by day in columns 4–5 and 7–9. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (4). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (5) Means and standard deviations of the dependent variables are computed based on untreated and non-believer observations.

individuals believed in the efficacy of the ritual itself. Thus, they provide a refined estimate of treatment that is more likely to be driven solely by the seller's belief in the protection offered by the ritual.

The DD estimates confirm the findings from the TWFE, Callaway and Sant'Anna (2021), and waitlist design estimates. All suggest that the rituals resulted in increased purchases, greater inventories, fewer stockouts, and, as a result, increased sales and revenues. Thus, the protection rituals did have important economic effects.

The Rituals Created More Accurate Perceptions about the Probability of Theft

An important aspect of our findings is that the ritual affected beliefs about the probability of theft such that they became more accurate. Using all data points from the start of the experiment until the end of our data collection on Nov 22nd, 2017, Figure 7 provides additional evidence for this point. It shows the sample means for the questions *"Do you think that a theft will occur tomorrow?"*, to which beer sellers choose "yes" or "no," and actual theft, an indicator equal to one if theft occurred the following day, by treatment status and separately for believers and non-believers.

Figure 7 reports averages in perceived and actual theft incidence for believers and non-believers before and after the ritual. From the figure, a number of important facts emerge. First, we see that perception of risk among believers decreases from 8.9% to 5.2% — a 42% decrease. Thus, participating in the ritual does decrease the average perceived probability of theft among

believers. Even after the treatment, the perceived probability of theft of treated individuals (i.e., believers), 5.2%, is still much higher than the actual probability of theft, which is approximately 0.9%. This suggests that the ritual did not induce a greater distortion in beliefs about the risk of theft, but instead corrected a bias in their beliefs, leading to more accurate perceptions. By correcting this biased belief, the ritual led to behavior that was closer to optimal, resulting in increased inventories, reduced stock-outs, higher sales, and greater profits.

Looking at the reported statistics for non-believers, a number of patterns are noteworthy. The first is that non-believers experienced a slight increase in their perceived probability of theft, although this effect was small, with a mean increase of only 2.1 percentage points from the very low control mean. This effect could be, as noted before, partially due to the logistics around the rituals, including traveling to and from the locations of the rituals and time spent outside the village, or it could reflect reversion to a higher long-term higher risk perception. The second is that, prior to treatment, non-believers have lower perceptions of the likelihood of theft compared to believers. Given the existing evidence from the anthropology literature of the importance of rituals for alleviating stress (Malinowski, 1948), it is not surprising that those who perceive greater risk due to theft would also hold on to beliefs that help to address the perceived risk.

The last pattern that we note is that for both believers and non-believers, the actual probability of theft is unaffected by the ritual, which although expected, is reassuring to confirm in the data.

Do Supernatural Protection Rituals Pay Off?

We now turn to the question of whether the rituals are a profitable investment for beer sellers. To assess this, we compare the ritual's cost to the extra profit sellers make from increased sales due to the ritual. The market price for a ritual is around \$40–50. Using the highest estimate for the effect of the rituals on IHS revenues (reported in Table 5), we estimate that the average increase in daily profits amounts to \$0.61.[18] Since the sample consists of ten post-ritual days for the average beer seller, the average additional profits in this short period are \$6.10.

However, the benefits of the rituals are likely to persist beyond these few days. The event-study estimates shown in Figure 6 are relatively stable over six weeks after the rituals. To examine the

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[18]Specifically, we multiply the estimated coefficient of 0.122 with the control mean of revenues per brand, \$1.24, and with sixteen, the number of brands in the data set. This results in an average increase in daily sales of \$2.42. This number is very close to the average loss due to stock-outs, which occurs once a week on average, reported before the experiment (see *Fact 2* in Section 2), providing another validation check of our data collection. Sellers have a profit margin of approximately 25%. Thus, the additional daily profits are \$0.61.

Figure 7: Perceived vs. actual risk of theft, treated and untreated, until end of data collection

Share of seller–day observations (in %)
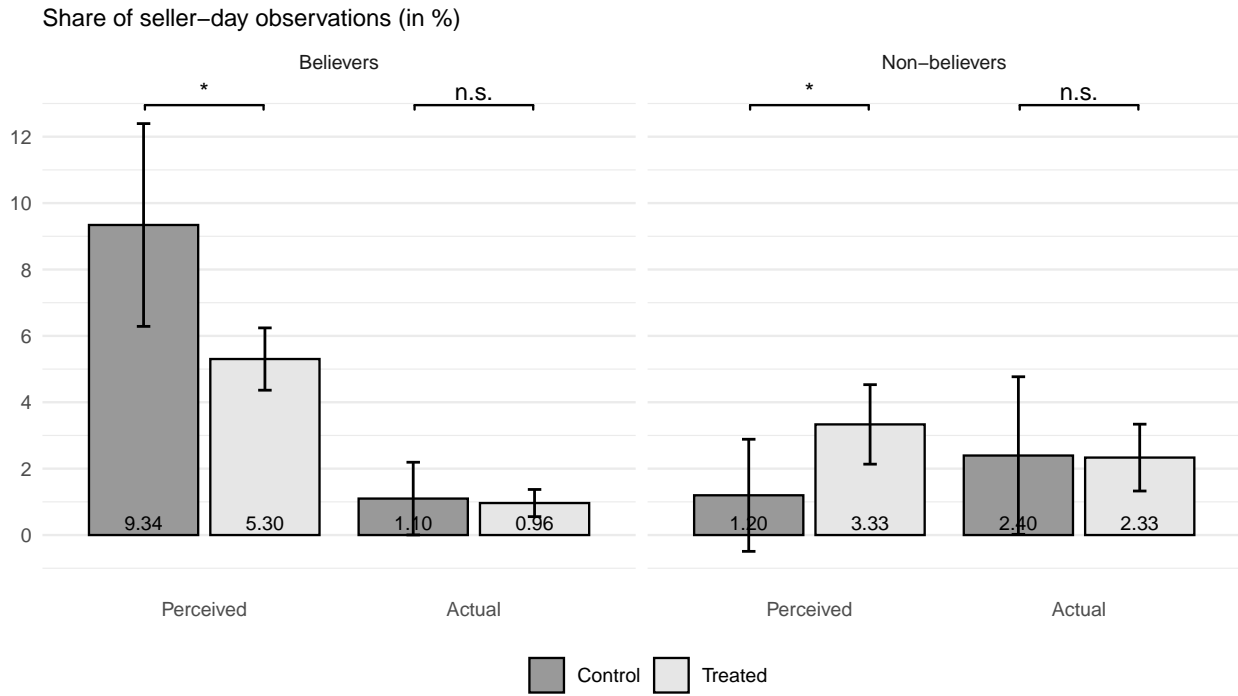
Notes: Each set of bars represents the share of sellers who (i) expected to become a victim of theft the next day (*"Do you think that a theft will occur tomorrow?"*), or (ii) actually were victims of a theft the next day (*"Was there a theft today?"* responses in $t+1$). The "Control" bars show the mean for observations from the first day of the experiment (July 17th, 2017) until before the ritual was performed. The "Treated" bars show the mean for observations after the ritual was performed until the end of our data collection (Nov 22nd, 2017). Rituals were staggered between July and August 2017. The "Believers" panel includes sellers who reported to have a magical amulet in use at some point during the data collection. The "Non-believers" panel shows the shares for sellers who never reported to have used a magical amulet during data collection. Whiskers show robust standard errors which are corrected using the Bonferroni procedure based on four hypotheses. Upper brackets indicate statistical significance of difference in means between Control and Treated.

long-run effect of the rituals on revenues more rigorously, we estimate the dynamic Callaway and Sant'Anna (2021) procedure using all observations until the end of our data collection and interpreting non-believers as never-treated beer sellers. These estimates are shown in Figure 8. We find that the effect persists during the three months. The summary of ATTs based on event-study aggregation is 0.168, or \$0.87 of additional daily profits.[19] Over 90 days, the additional profits total \$78.30.

This comparison of average values can obscure the considerable heterogeneity among individual beer sellers. In Appendix Figure A9, we examine the distribution of additional profits over a span of 90 days for the sellers, juxtaposing this data with the estimated cost of a ritual. Our

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[19]Here, the control mean of revenues is USD 1.30 per brand. We multiply this number with the estimate of 0.168 and sixteen, the number of brands. This amounts to \$3.49 in increased sales and \$0.87 of additional daily profits, on average.

Figure 8: Dynamic DD estimates of the effects on revenues until end of data collection

Revenues (IHS USD)
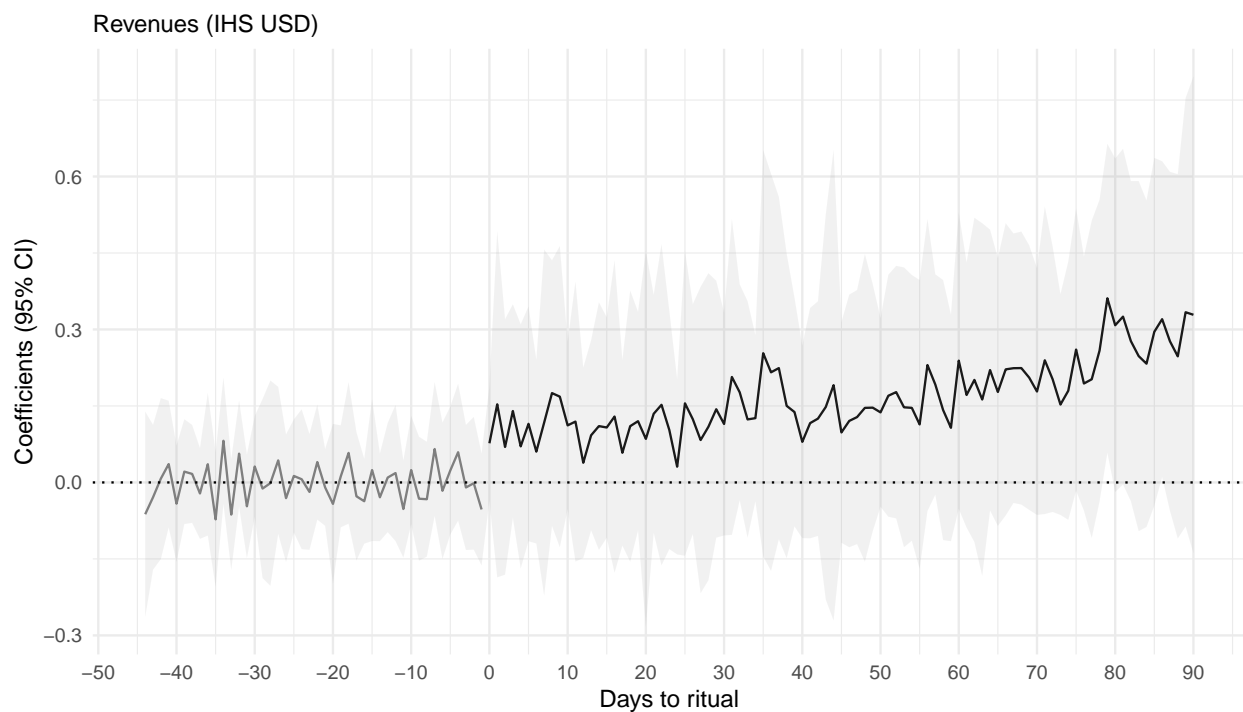
Notes: This figure plots the coefficients and 95 percent confidence intervals for the event-study estimation of the Callaway and Sant'Anna (2021) procedure with IHS revenues (USD) as outcome and using all observations until the end of the data collection. Non-believers are regarded as never-treated observations. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses.

findings reveal that the ritual costs are offset by the profits for nine beer sellers, which constitutes about 22% of all the beer sellers who took part in the experiment.

5. Conclusion

Traditional African religions, which are characterized by beliefs in a divine creator, ancestral spirits, supernatural powers, and ritual, continue to be widespread in sub-Saharan Africa. As we have seen, survey data indicate that the majority of the population of sub-Saharan Africa continue to hold these traditional religious beliefs. Within the field of anthropology, it is understood that these beliefs permeate all aspects of daily life, and they are widely studied. Despite this, the beliefs have been largely ignored within economics and have yet to be incorporated into our understanding of economic development on the African continent. One reason for this is that it has yet to be established empirically that these beliefs are deeply held, and that they matter for economic decisions and outcomes. This study aimed to establish these facts.

To make progress along these lines, we estimated the effects of a common anti-theft ritual among a random sample of beer sellers in the Eastern DRC. The study provided a protection ritual to the sellers at different points in time that were randomly chosen.

We found that at baseline, beer sellers perceive the risk of theft in their shops to be higher than it actually is, causing them to hold lower inventories, more frequent stock-outs, and reduced profits. The rituals partially corrected this, inducing them to purchase more inventory, experience fewer stock-outs, and have larger sales, revenues, and profits. These effects were only found for sellers who held traditional beliefs (and believed in the efficacy of rituals) at baseline. For non-believers, we find none of these effects.

The findings provide evidence of the importance of African traditional religions, demonstrating that they can influence behavior and outcomes that are important for economic development. This is particularly important given the pervasiveness and persistence of traditional African religious beliefs. In spite of the widespread adoption of Christianity and Islam in the continent, the pre-existing religious beliefs remain. Our findings suggest that such beliefs are real and do affect decisions and outcomes relevant to economic development and point to the importance of additional research to understand these beliefs and their consequences for developing countries.

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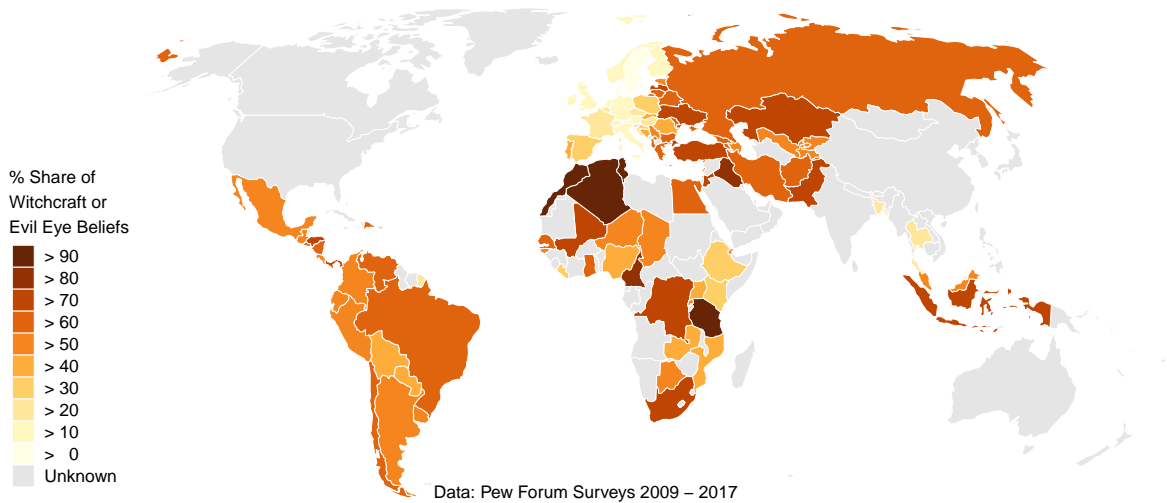
Online Appendix

On the Importance of African Traditional Religion for Economic Behavior

Lewis Dunia Butinda, Aimable Amani Lameke, Nathan Nunn, Max Posch, and Raul
Sanchez de la Sierra

Appendix A. Additional Tables and Figures

Figure A1: Prevalence of beliefs in witchcraft or the evil eye

Notes: The map displays nationally representative data collected by the Pew Forum in several survey waves
between 2009 and 2017. The surveys asked respondents to indicate (i) whether they believe in witchcraft,
and (ii) whether they believe in the evil eye or that certain people can cast curses or spells that cause bad
things to happen to someone. In Western European countries, the survey asked the evil eye but not the
witchcraft question. We calculate the country-level shares of individuals who believe in witchcraft or the
evil eye (or both) using the sampling weights that come with the survey data.

Figure A2: Average daily sales by beer variety
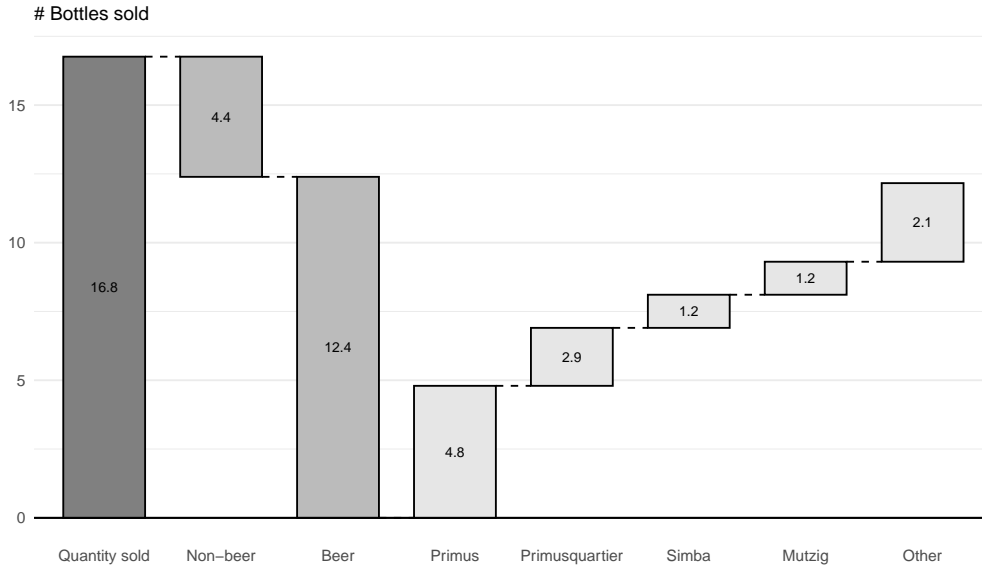
Notes: The graph shows the average number of bottles sold and its decomposition into beer / non-beer and the most frequently sold beer varieties for the period from July 1st 2015 to July 15th, 2017 for the 44 beer sellers who participated in the experiment. 16.8 bottles sold amounts to approximately $20 USD in daily revenues.

Figure A3: Average daily sales by beer variety among sellers who could not participate in the experiment
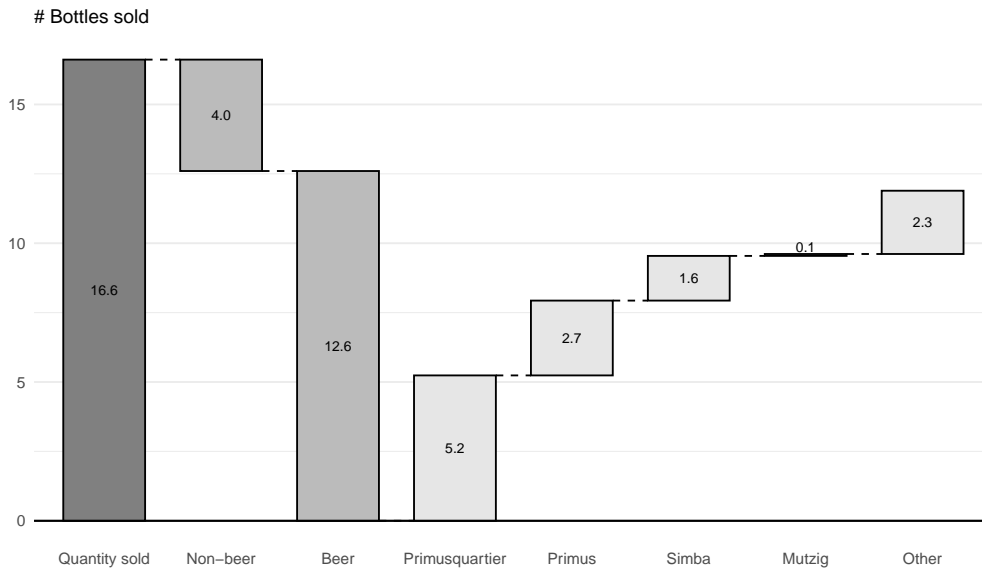
Notes: The graph suggests that the sales figures of beer sellers who could not participate in the experiment are almost identical to the sales figures of beer sellers who participated in the experiment. It shows the average number of bottles sold and its decomposition into beer / non-beer and the most frequently sold beer varieties for the period from July 1st 2015 until the data collection was interrupted in May 2017 for the 37 beer sellers who could not participate in the experiment. 16.6 bottles sold amounts to approximately $20 USD in daily revenues.

Figure A4: Perceived and actual theft, believer and non-believer subsamples

Share of beer sellers, per day (in %)   Share of beer sellers, per day (in %)

(a) Believers   **(b)** Non-believers
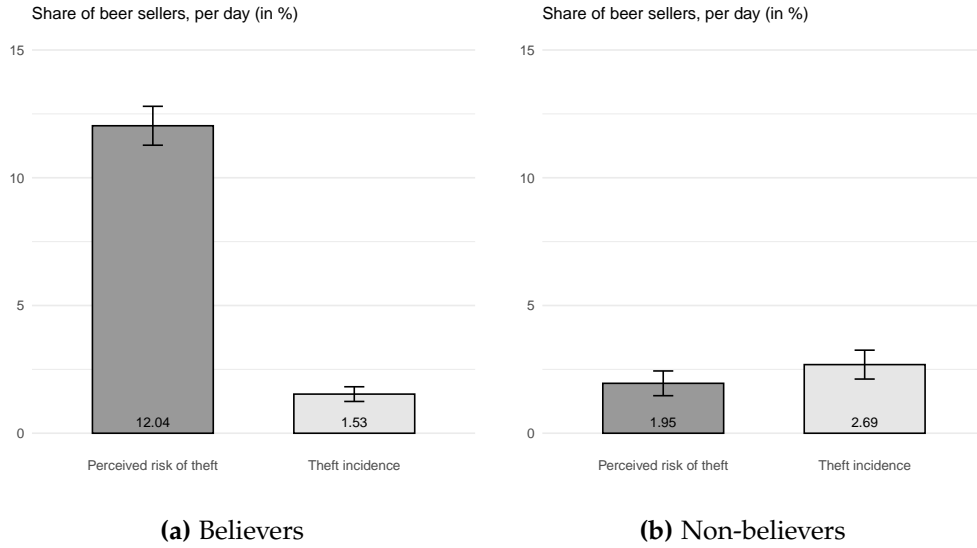
Notes: These figures compare the perceived risk of theft, computed as the proportion of sellers who believe they will be the victim of theft the next day, with the actual risk of theft, estimated as the proportion of sellers who were victims of theft on a given day. Left: 30 beer sellers who believe in supernatural protection. Right: 14 beer sellers who do not believe in supernatural protection. The figures summarize data collected from May to July 15th, 2017. The error bars represent standard errors.

Share of beer sellers who believe in active amulet (in %)

Days to ritual

● Believers
▲ Non–believers
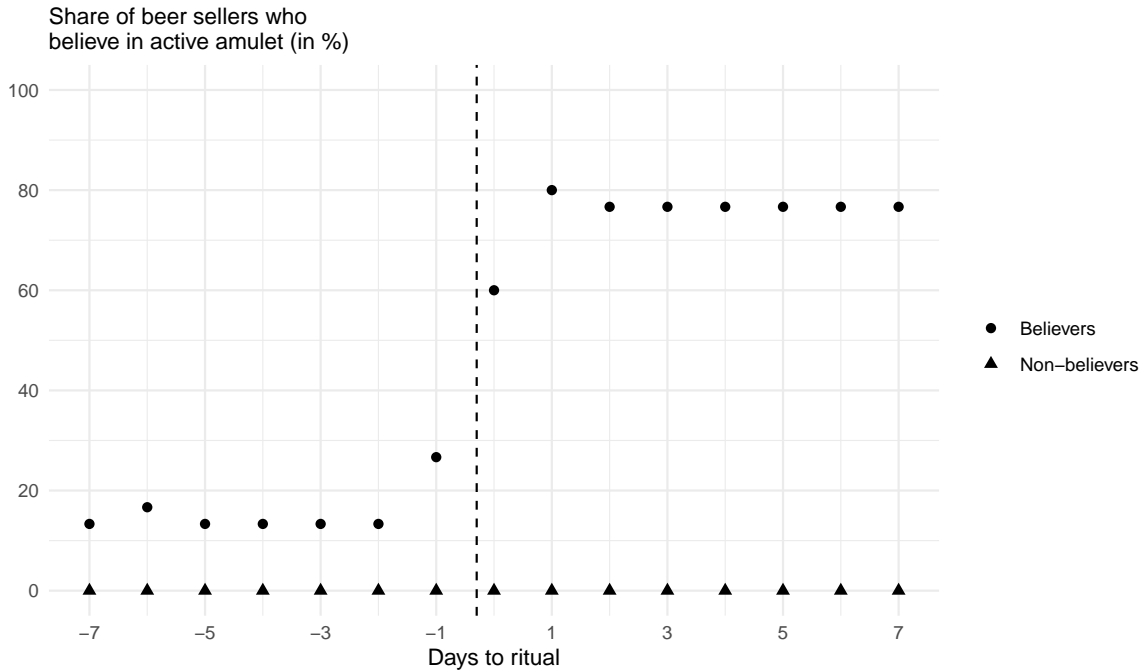
Figure A5: Effect of ritual attendance on perceptions of protection

Notes: The figure shows the share of beer sellers that report having supernatural protection in use in the days before and after the ritual. Dots (triangles) represent the shares among believers (non-believers). We define believers (non-believers) as sellers that ever (never) reported believing in an active amulet before the start of the experiment.

Table A1: Summary statistics of all variables in the experimental sample

	N	Mean	SD	Min	Max
Panel B: Beer seller × day-level observations					
Believer	1170	0.699	0.459	0	1
Post-Ritual	1170	0.509	0.500	0	1
Belief in active amulet indicator	1170	0.285	0.451	0	1
Theft expectations	1139	0.114	0.275	0	1
Theft indicator	1170	0.021	0.145	0	1
Stress level	1162	1.180	0.478	1	3
Stock-outs	1053	0.131	0.252	0	1
Panel A: Beer seller × beer brand × day-level observations					
Believer	18720	0.699	0.459	0	1
Post-Ritual	18720	0.509	0.500	0	1
Bottles purchased (IHS #)	17888	0.095	0.613	0	7.378
Bottles in stock (IHS #)	17840	0.871	1.620	0	7.075
Revenues (IHS USD)	18563	0.346	0.899	0	6.085
Bottles sold (IHS #)	18563	0.316	0.822	0	5.781
Sale prices of bottles sold (USD)	5436	1.304	0.595	0.233	3.333

Notes: The sample contains 29 days, from July 16th to August 14th, 2017. *Believer* takes value 1 for beer sellers who reported believing in any indigenous-religious amulet before the start of the experiment, 0 otherwise. *Post-Ritual* takes value 1 from the day an individual beer seller attended a ritual, 0 otherwise. *Belief in active amulet indicator* takes value 1 during days when retailers report that the magical protection amulets are active, 0 otherwise. *Theft expectations* is the average of three indicators equal to one if sellers expect a theft incidence the following day, week, or month. *Theft indicator* takes value 1 during days when beer sellers report a theft, 0 otherwise. *Stress level* is self-reported, ranging from 1–3, with increasing values indicating a higher stress level. *Stock-outs* is the share of brands for which an individual beer seller reports an inventory equal to zero at the end of the day. To compute this share, we consider brands that the beer seller ever held or sold during the data collection. *Bottles purchased*, *Bottles in stock*, *Revenues*, and *Bottles sold* are inverse hyperbolic sine (IHS) transformed. The number of observations for *Stock-outs*, *Bottles purchased*, *Bottles in stock*, *Revenues*, *Bottles sold*, and *Sale prices of bottles sold* is lower than for treatment status because of missing data for these variables for some varieties on some dates.

Figure A6: Event-study estimates of the effects on theft beliefs and perceived stress (beer seller fixed effects)
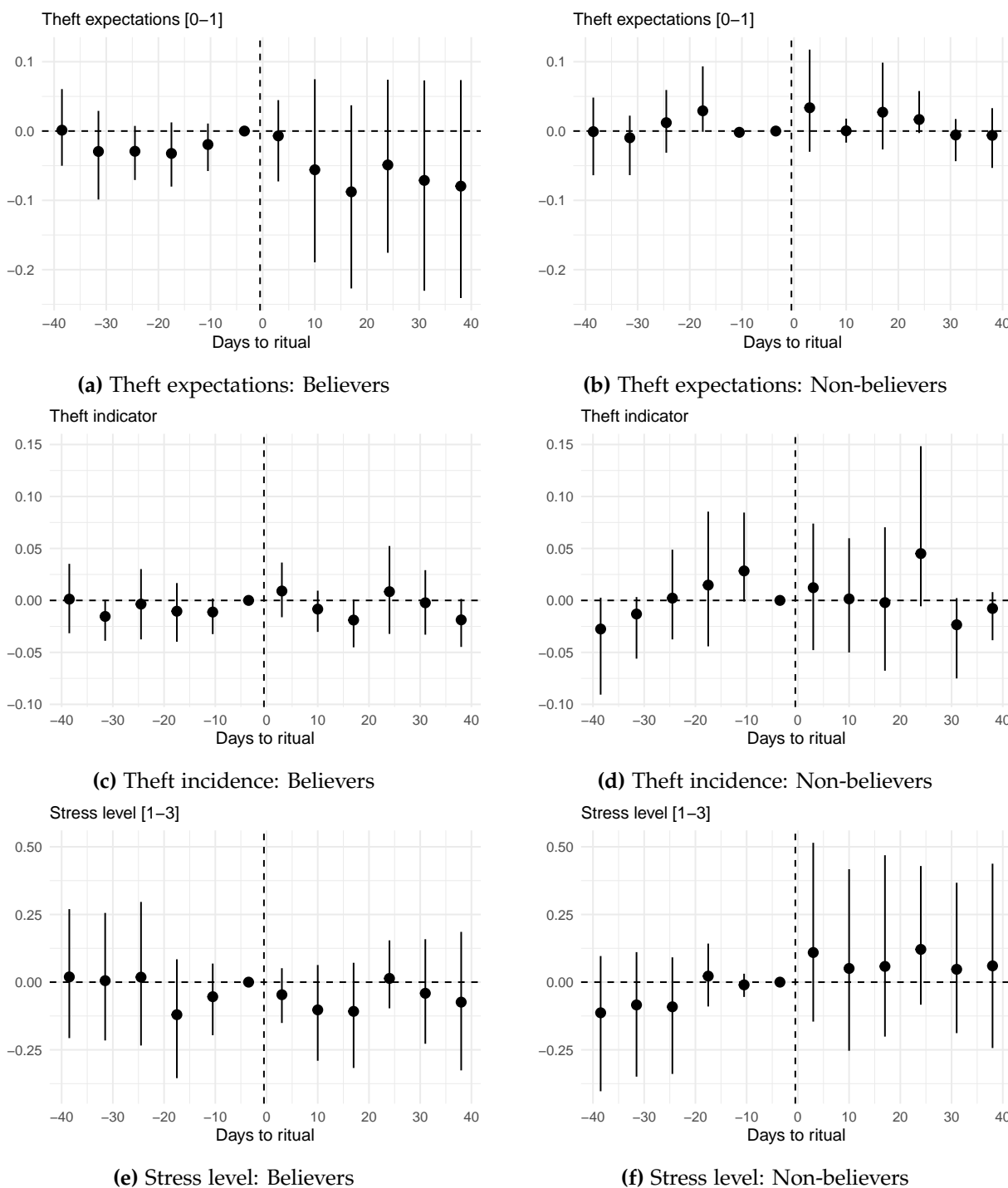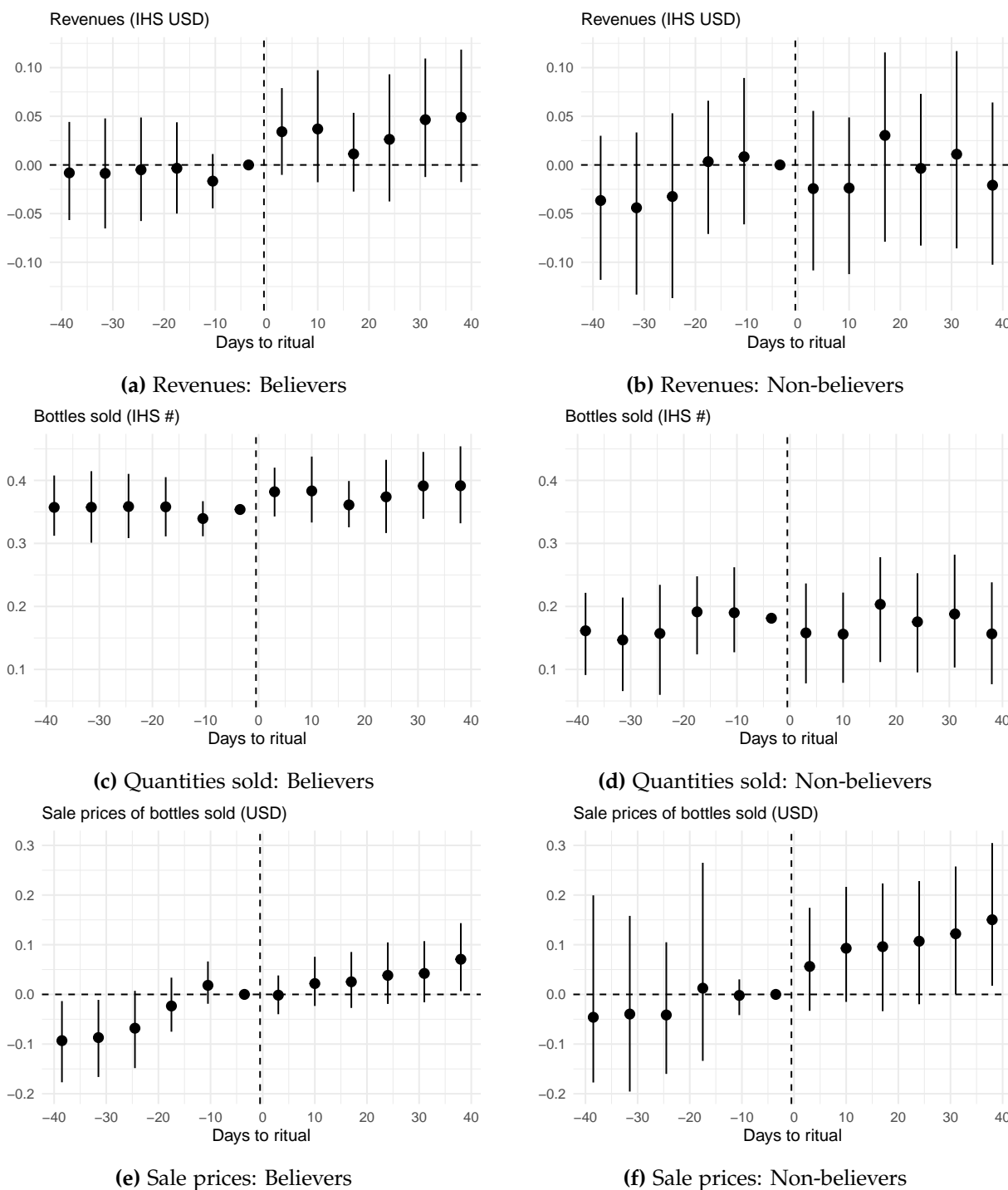

(a) Theft expectations: Believers

(b) Theft expectations: Non-believers

(c) Theft incidence: Believers

(d) Theft incidence: Non-believers

(e) Stress level: Believers

(f) Stress level: Non-believers

Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of outcomes on weeks-to-ritual dummies (each dot represents seven days), the β_j's reported equation (1). The regression also includes seller fixed effects. The unit of observation is seller-day. Left panel: believers; right panel: non-believers. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations.

Figure A7: Event-study estimates of the effects on purchases, inventories, and stock-outs (beer seller fixed effects)

(a) Bottles purchased: Believers

(b) Bottles purchased: Non-believers

(c) Bottles in stock: Believers

(d) Bottles in stock: Non-believers

(e) Stress level: Believers

(f) Stress level: Non-believers

Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of outcomes on weeks-to-ritual dummies (each dot represents seven days), the β_j's reported equation (1). The regression also includes seller fixed effects. The unit of observation is seller-brand-day. Left panel: believers; right panel: non-believers. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations.

Figure A8: Event-study estimates of the effects on purchases, inventories, and stock-outs (beer seller fixed effects)

(a) Revenues: Believers

(b) Revenues: Non-believers

(c) Quantities sold: Believers

(d) Quantities sold: Non-believers

(e) Sale prices: Believers

(f) Sale prices: Non-believers

Notes: This figure plots the coefficients and 95 percent confidence intervals for coefficients of regressions of outcomes on weeks-to-ritual dummies (each dot represents seven days), the β_j's reported equation (1). The regression also includes seller fixed effects. The unit of observation is seller-brand-day. Left panel: believers; right panel: non-believers. Confidence intervals are computed using seller-clustered bootstrap standard errors based on 10,000 iterations.

Table A2: Robustness: Alternative Sample Where Days of Rituals Are Dropped

	Theft expectations [0-1] (1)	Theft indicator (2)	Stress level [1-3] (3)	Bottles purchased (IHS #) (4)	Bottles in stock (IHS #) (5)	Stock-outs (6)	Revenues (IHS USD) (7)	Bottles sold (IHS #) (8)	Sale Prices of bottles sold (USD) (9)
	Panel A: Two-way fixed effects								
Post-Ritual	-0.037	0.012	0.006	0.021	-0.027	-0.014	0.047	0.042	-0.010
95% CI	[-0.158, 0.075]	[-0.038, 0.058]	[-0.241, 0.321]	[-0.045, 0.085]	[-0.179, 0.101]	[-0.103, 0.087]	[-0.009, 0.105]	[-0.007, 0.095]	[-0.059, 0.047]
	Panel B: Callaway and Sant'Anna (2021)								
Post-Ritual	-0.049	-0.014	-0.044	0.106	0.175	-0.055	0.120	0.112	0.037
95% CI	[-0.143, 0.044]	[-0.088, 0.072]	[-0.314, 0.226]	[0.031, 0.182]	[-0.026, 0.377]	[-0.132, 0.023]	[0.027, 0.213]	[0.022, 0.203]	[-0.408, 0.483]
	Panel C: Waitlist design								
Post-Ritual	-0.050	0.000	-0.085	0.022	0.062	-0.053	0.046	0.041	0.026
95% CI	[-0.212, 0.079]	[-0.022, 0.022]	[-0.287, 0.063]	[-0.017, 0.056]	[-0.037, 0.162]	[-0.142, 0.01]	[0.002, 0.097]	[0, 0.081]	[0.002, 0.061]
	All panels:								
Observations	764	793	785	12,017	12,233	737	12,541	12,541	3,788

Notes: This table reports ATT estimates of the effects of the rituals on key outcomes for the believer sample in which we drop the days on which the rituals took place. The main independent variable is an indicator taking value from the day after an individual beer seller attended the ritual, and zero otherwise. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3).

Table A3: Purchases

	Believers			Non-Believers		
	# Bottles purchased (1)	# Bottles in stock (2)	Stock-outs (3)	# Bottles purchased (4)	# Bottles in stock (5)	Stock-outs (6)
	Panel A: Two-way fixed effects					
Post-Ritual	0.664	0.240	-0.016	-1.126	-0.744	0.048
95% CI	[-0.334, 1.791]	[-6.332, 5.945]	[-0.097, 0.073]	[-2.652, 0.512]	[-6.491, 5.780]	[-0.154, 0.270]
	Panel B: Callaway and Sant'Anna (2021)					
Post-Ritual	2.553	2.847	-0.063	-1.082	-3.052	0.041
95% CI	[1.289, 3.817]	[-0.691, 6.386]	[-0.129, 0.003]	[-2.074, -0.091]	[-3.872, -2.232]	[-0.045, 0.128]
	Panel C: Waitlist design					
Post-Ritual	0.299	4.355	-0.053	-0.533	-0.092	0.049
95% CI	[-0.339, 1.043]	[-0.916, 13.631]	[-0.136, 0.007]	[-1.267, 0.214]	[-2.612, 2.864]	[-0.068, 0.218]
	All panels:					
Control mean (sd) DV	0.84 (12.71)	8.20 (34.24)	0.13 (0.27)	1.17 (9.68)	6.61 (30.07)	0.12 (0.22)
Observations	12,394	12,617	760	5,494	5,223	293

Notes: This table presents ATT estimates of the effects of the ritual on number of bottles purchased, number of bottles in stock, and share of brands that are out of stock, as defined in the text. The unit of observation is beer seller by beer variety by day, except in columns 3 and 6, where it is beer seller by day. The main independent variable is an indicator taking value from the day an individual beer seller attended the ritual, and zero otherwise. Columns 1–3 (4–6) show the results for the believer (non-believer) sample. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3). Means and standard deviations of the dependent variables are computed based on untreated observations.

Table A4: Revenues

	Believers			Non-Believers		
	Revenues (USD) (1)	# Bottles sold (2)	Sale prices of bottles sold (USD) (3)	Revenues (USD) (4)	# Bottles sold (5)	Sale prices of bottles sold (USD) (6)
	Panel A: Two-way fixed effects					
Post-Ritual	0.174	0.143	-0.009	-0.303	-0.247	0.016
95% CI	[-0.031, 0.382]	[-0.011, 0.321]	[-0.053, 0.043]	[-1.714, 0.543]	[-1.253, 0.320]	[-0.054, 0.090]
	Panel B: Callaway and Sant'Anna (2021)					
Post-Ritual	0.508	0.437	0.032	-1.454	-1.019	0.173
95% CI	[0.092, 0.925]	[0.081, 0.793]	[-0.349, 0.413]	[-2.794, -0.114]	[-1.872, -0.167]	[-0.468, 0.815]
	Panel C: Waitlist design					
Post-Ritual	0.201	0.148	0.025	-0.163	-0.202	0.068
95% CI	[0.003, 0.393]	[-0.005, 0.303]	[0.002, 0.058]	[-1.194, 0.558]	[-0.998, 0.271]	[-0.028, 0.163]
	All panels:					
Control mean (sd) DV	0.69 (2.50)	0.60 (2.23)	1.24 (0.64)	1.70 (8.66)	1.24 (6.34)	1.40 (0.53)
Observations	12,936	12,936	3,905	5,627	5,627	1,531

Notes: This table presents ATT estimates of the effects of the rituals on revenues (converted to USD), number of bottles sold, and sale prices of bottles sold (converted to USD). The unit of observation is beer seller by beer variety by day. The main independent variable is an indicator taking value from the day an individual beer seller attended the ritual, and zero otherwise. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3). Control means and standard deviations of the dependent variables are computed based on untreated observations. The number of observations for sale prices of bottles sold is lower than for the other outcomes because price data is missing when a particular beer brand is not sold on a given date.

Table A5: Beer vs. non-beer beverages

	Believers		Non-Believers	
	Revenues (IHS USD)			
	Beer	Non-beer	Beer	Non-beer
	(1)	(2)	(3)	(4)
Panel A: Two-way fixed effects				
Post-Ritual	0.043	0.071	-0.063	0.093
95% CI	[-0.008, 0.096]	[-0.010, 0.158]	[-0.184, 0.062]	[-0.128, 0.324]
Panel B: Callaway and Sant'Anna (2021)				
Post-Ritual	0.103	0.139	-0.093	0.183
95% CI	[-0.020, 0.227]	[0.050, 0.227]	[-0.154, -0.032]	[-0.037, 0.403]
Panel C: Waitlist design				
Post-Ritual	0.046	0.054	-0.047	0.060
95% CI	[-0.002, 0.094]	[-0.019, 0.138]	[-0.146, 0.029]	[-0.100, 0.208]
All panels:				
Observations	9,664	3,272	4,219	1,408

Notes: This table presents ATT estimates of the effects of the rituals on revenues from beer and non-beer beverages, converted to USD and inverse hyperbolic sine (IHS) transformed. The unit of observation is beer seller by beer variety by day. The main independent variable is an indicator taking value from the day an individual beer seller attended the ritual, and zero otherwise. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Panel A reports the results of the two-way fixed-effects estimator of equation (2). Panel B reports the results of the Callaway and Sant'Anna (2021) estimator. Panel C reports the results of the waitlist design estimator of equation (3).

Figure A9: Heterogeneity in the cost-benefit analysis of the rituals

Distribution of additional profits over 90 days and cost of the ritual
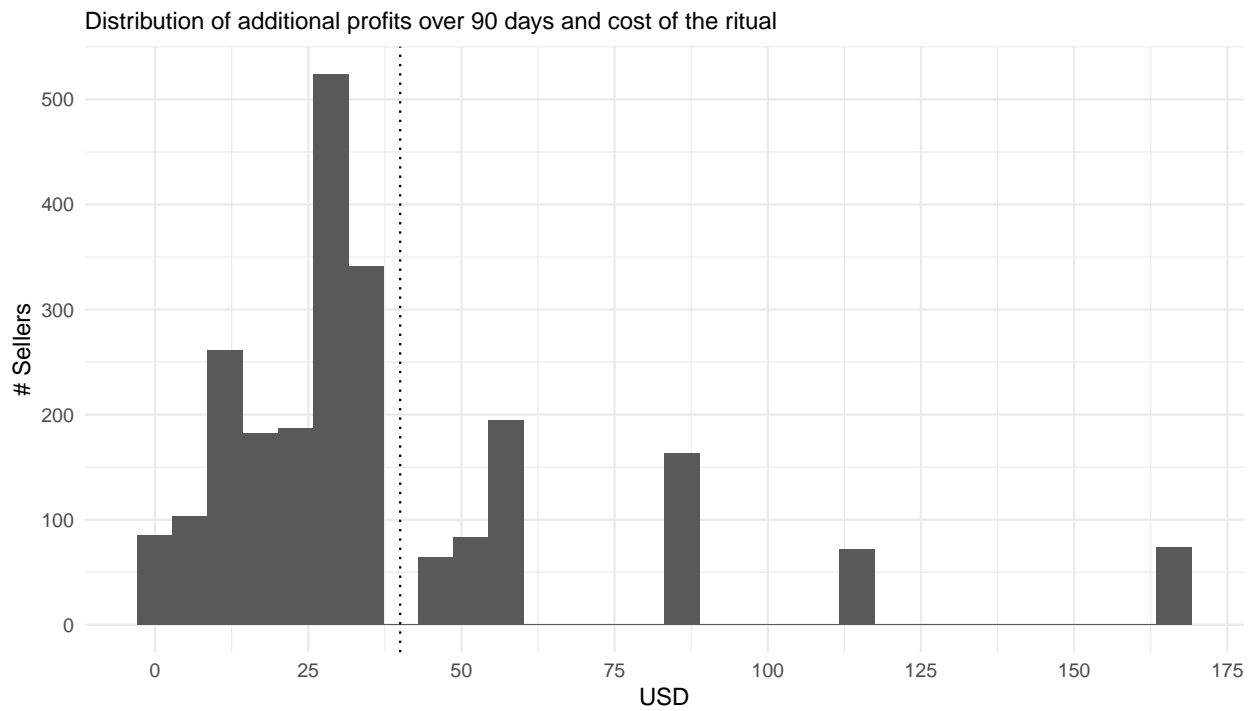
Notes: The graph shows the distribution of additional profits over 90 days across beer sellers (dark bars) and the approximate cost of a ritual (dotted line). Each bar represents the number of beer sellers making a certain additional profit. Bars to the right (left) of the dotted line mean that the ritual cost is (not) offset by the additional profits. Additional profits are computed by multiplying average daily revenues in the pre-treatment period with the summary of the estimates shown in Figure 8 based on event-study aggregation (0.168) and the approximate profit margin (25%). The sample is restricted to believers because we do not find an effect of the rituals among non-believers.

Figure A10: Theft expectation
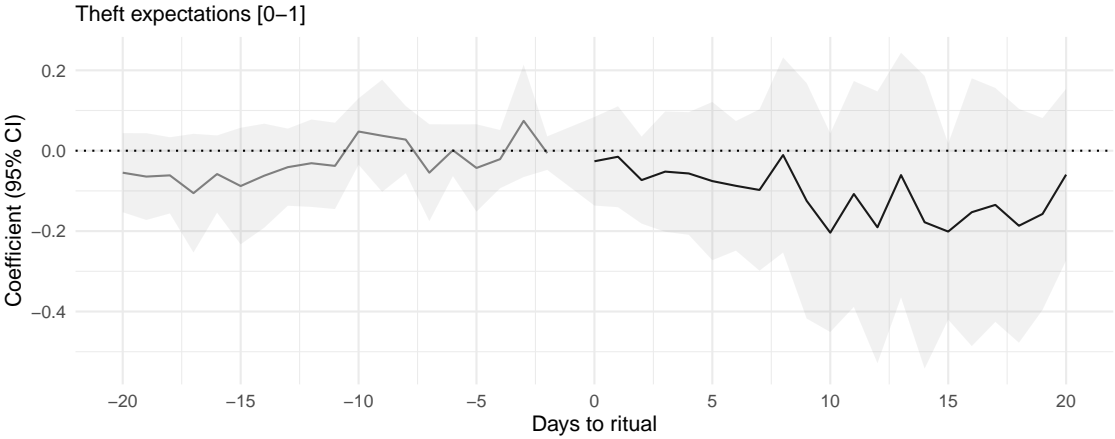
(a) Two-way fixed effects
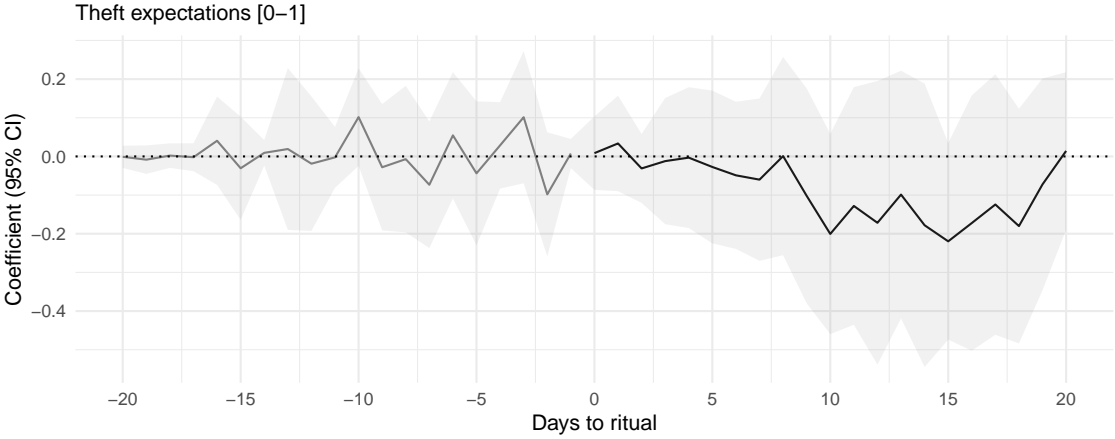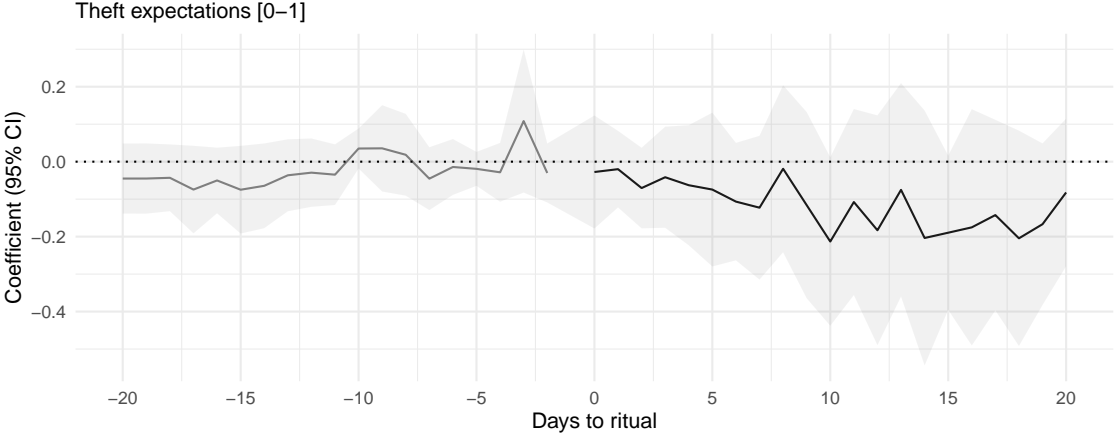
(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is theft expectations. Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.
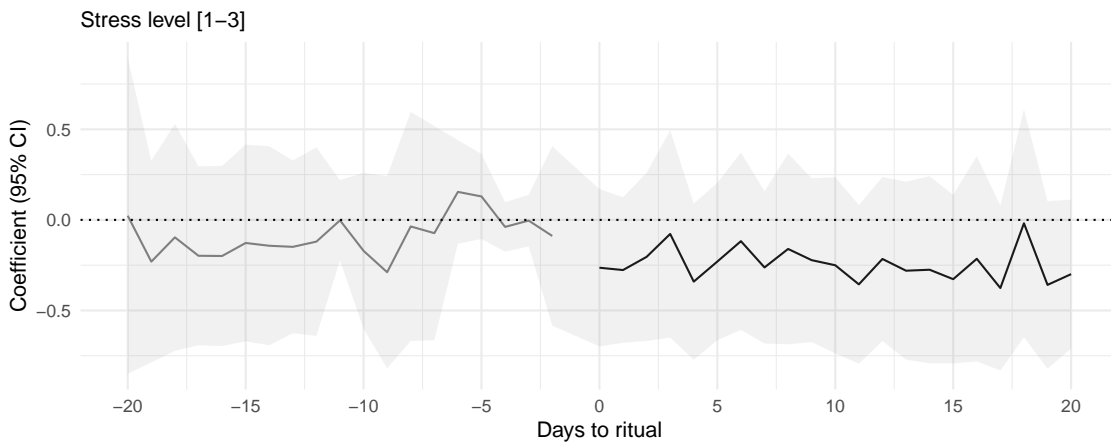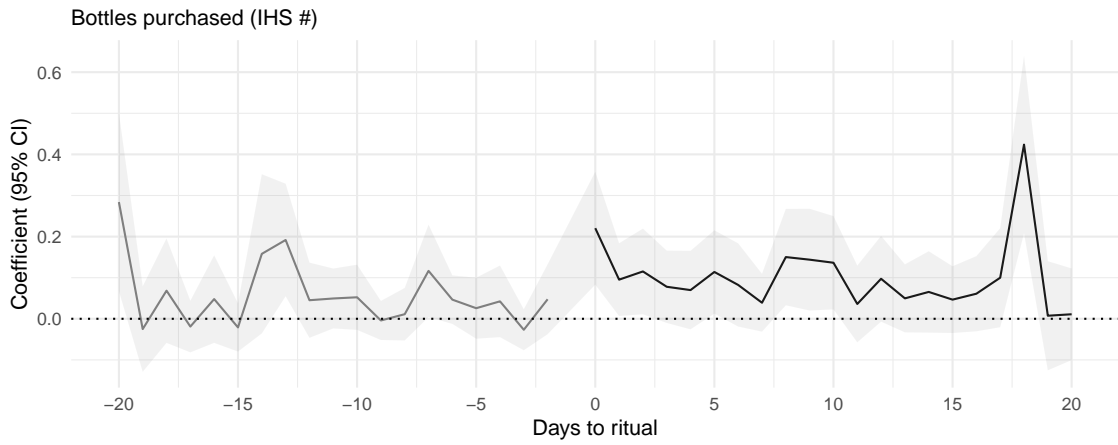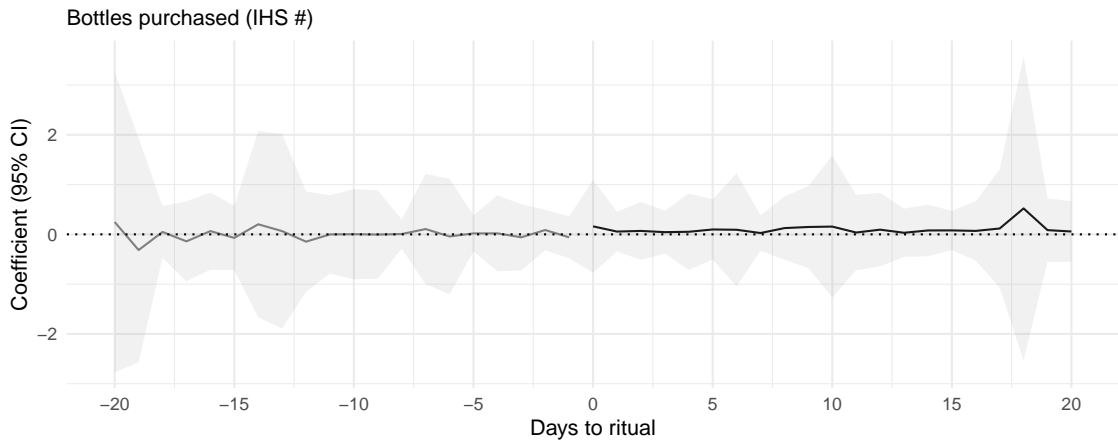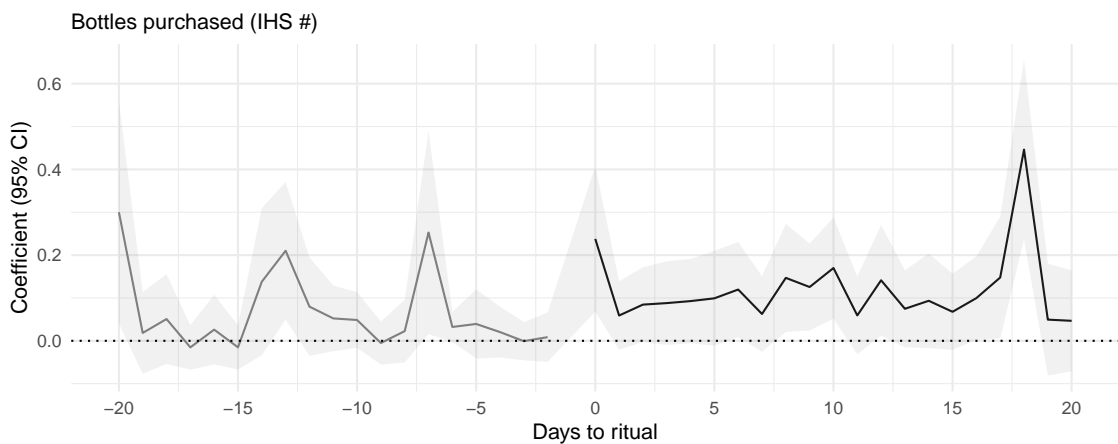
Figure A11: Theft indicator
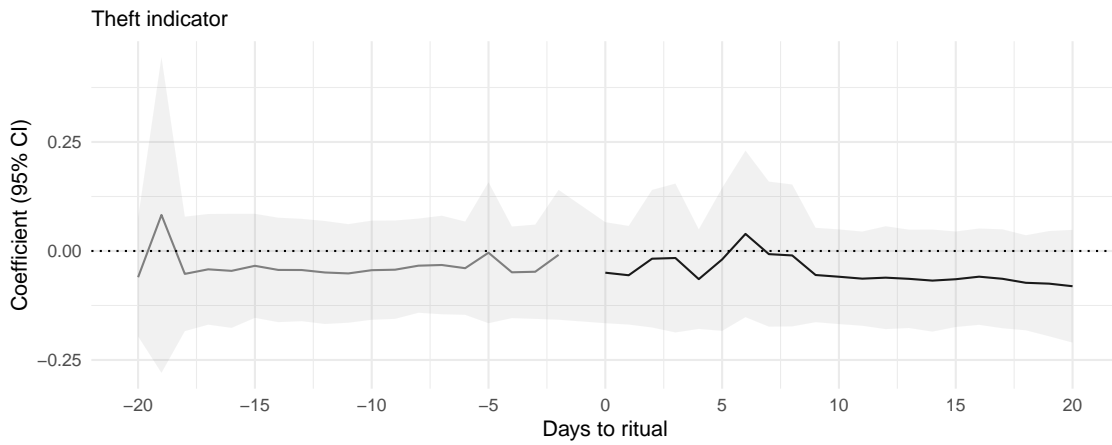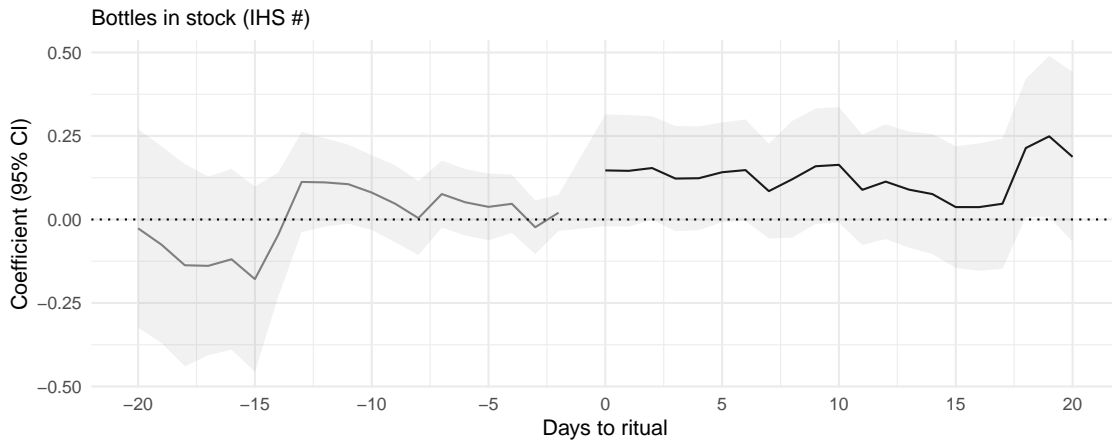

(a) Two-way fixed effects
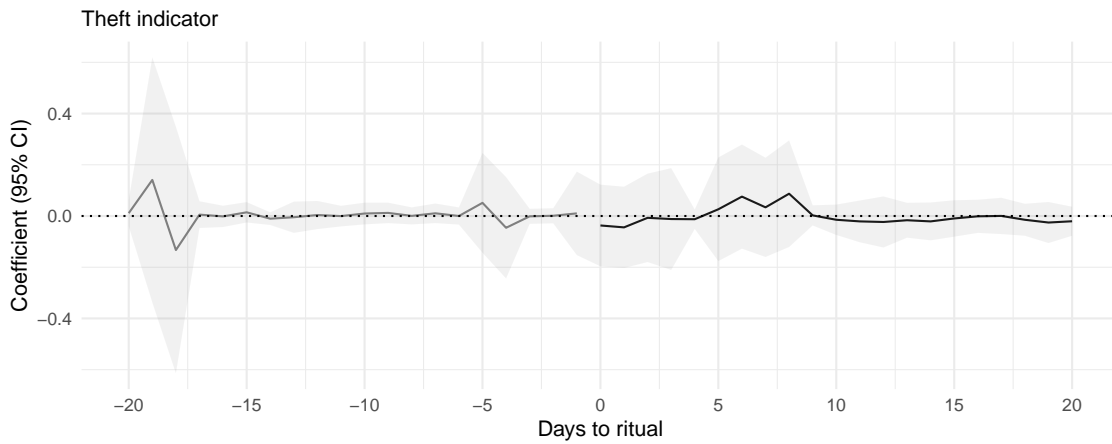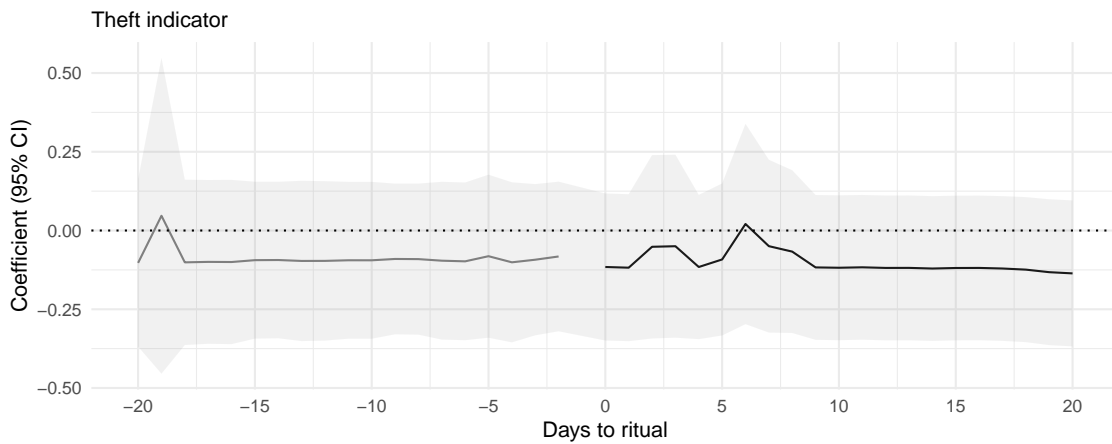
(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is a theft indicator equal to one if the beer seller experienced a theft during the day. Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.

Figure A12: Stress level
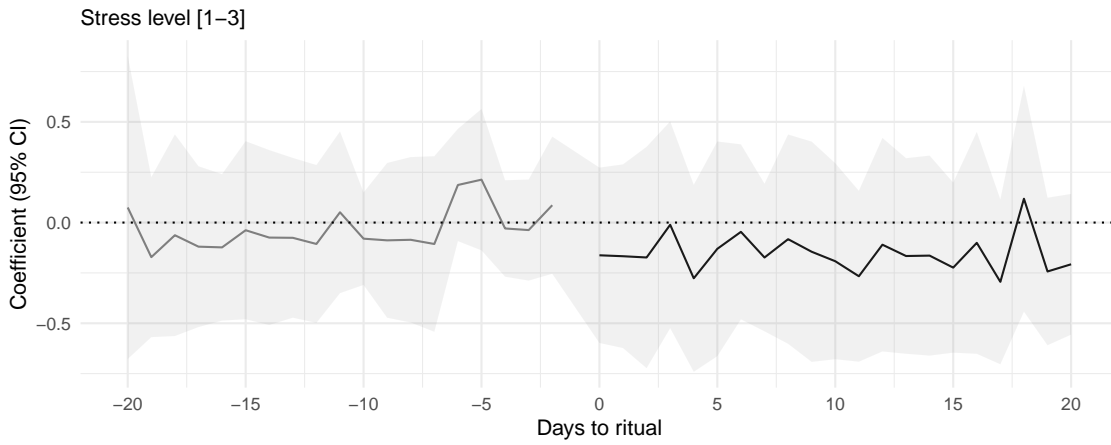
(a) Two-way fixed effects
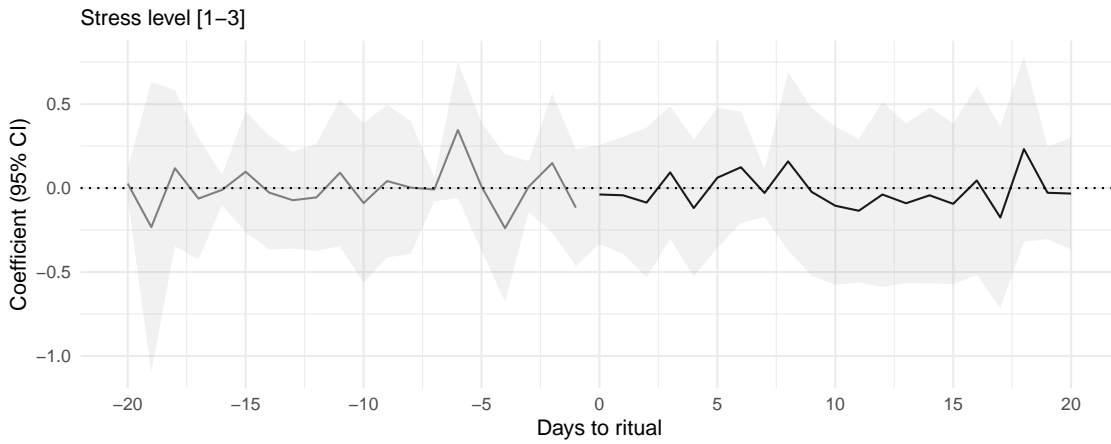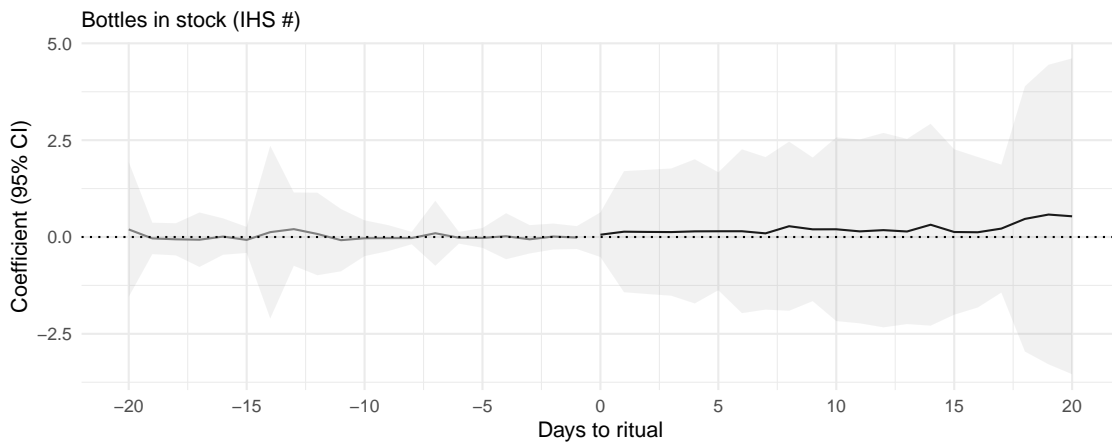

(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is perceived stress. Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.

**Figure A13:** Bottles purchased

Bottles purchased (IHS #)

(a) Two-way fixed effects

Bottles purchased (IHS #)

(b) Callaway and Sant'Anna (2021)

Bottles purchased (IHS #)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is number of bottles purchased (IHS transformed). Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.

Figure A14: Bottles in stock

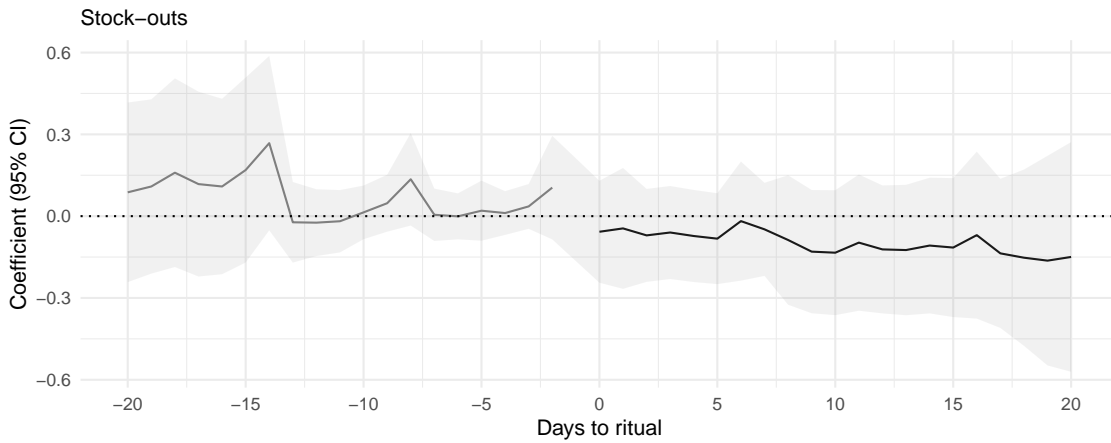**(a)** Two-way fixed effects
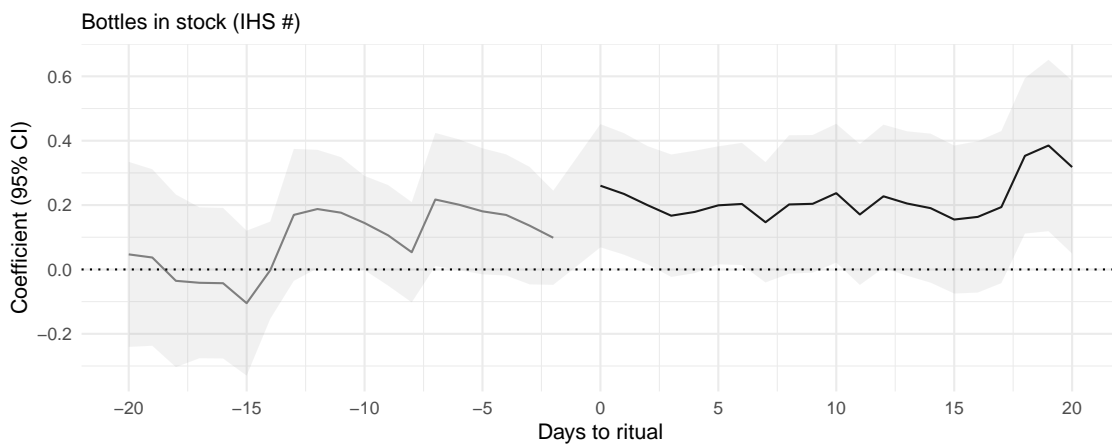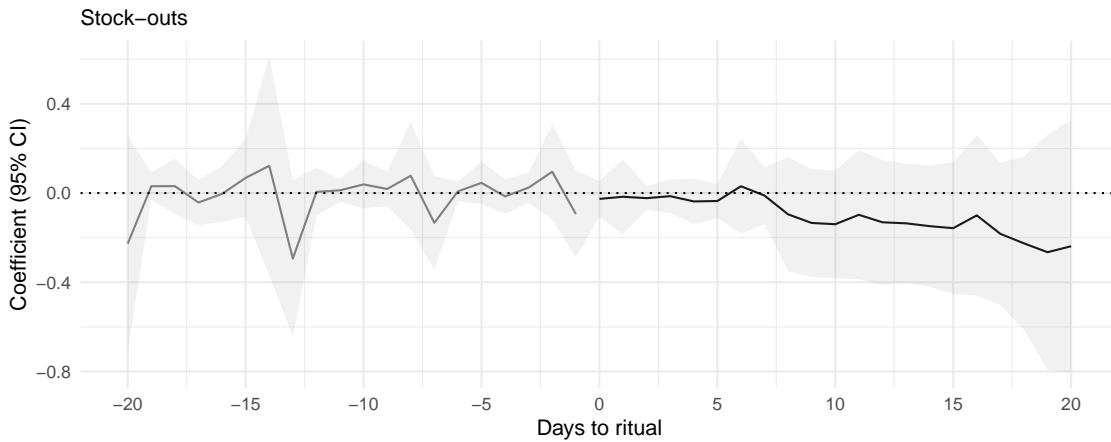

(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is number of bottles in stock (IHS transformed). Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.
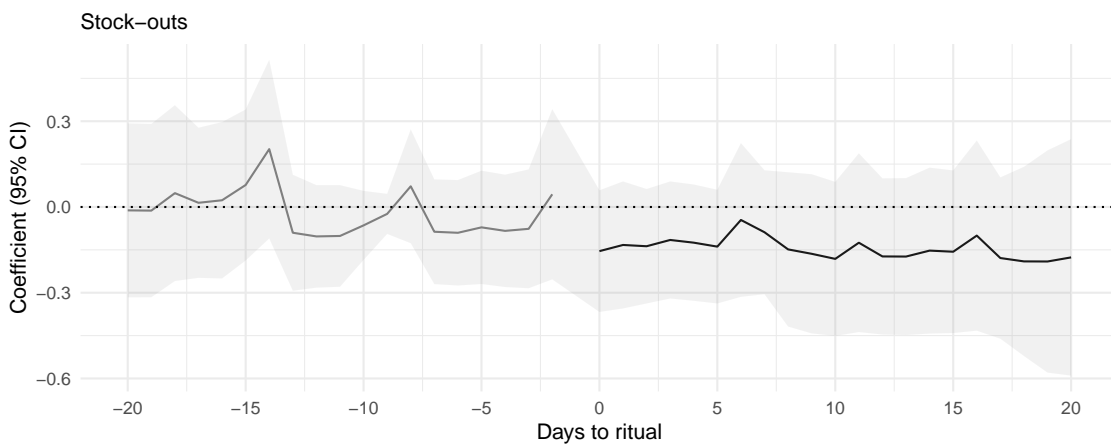
Figure A15: Stock-outs
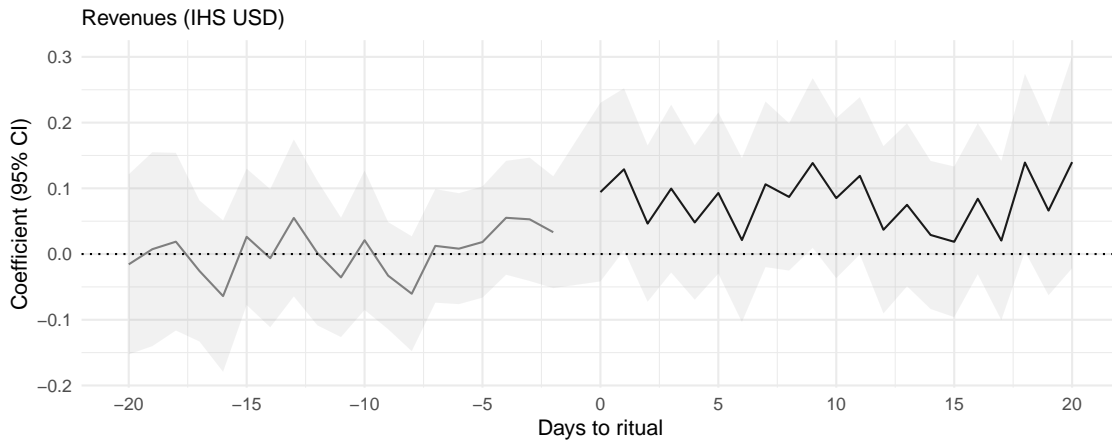

(a) Two-way fixed effects

(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is stock-outs. Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.

Figure A16: Revenues

(a) Two-way fixed effects
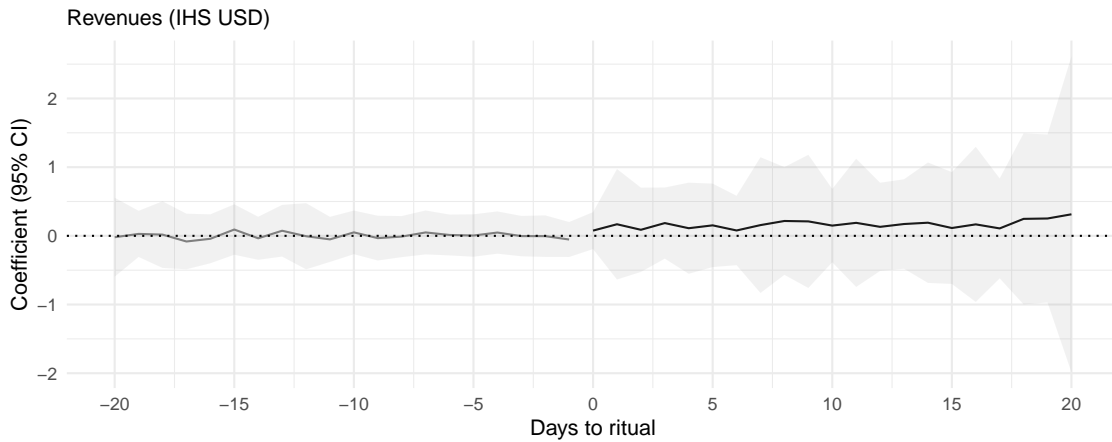

(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is revenues (IHS transformed). Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.
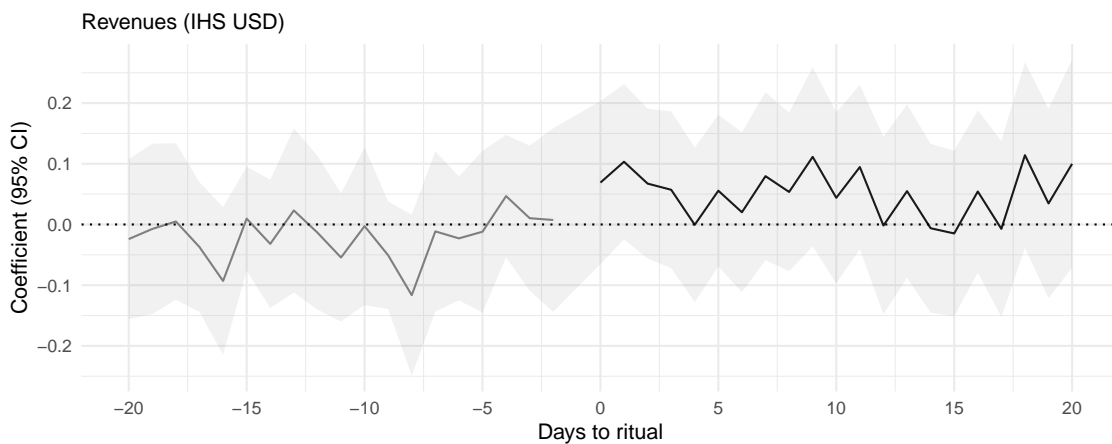
Figure A17: Bottles sold

(a) Two-way fixed effects
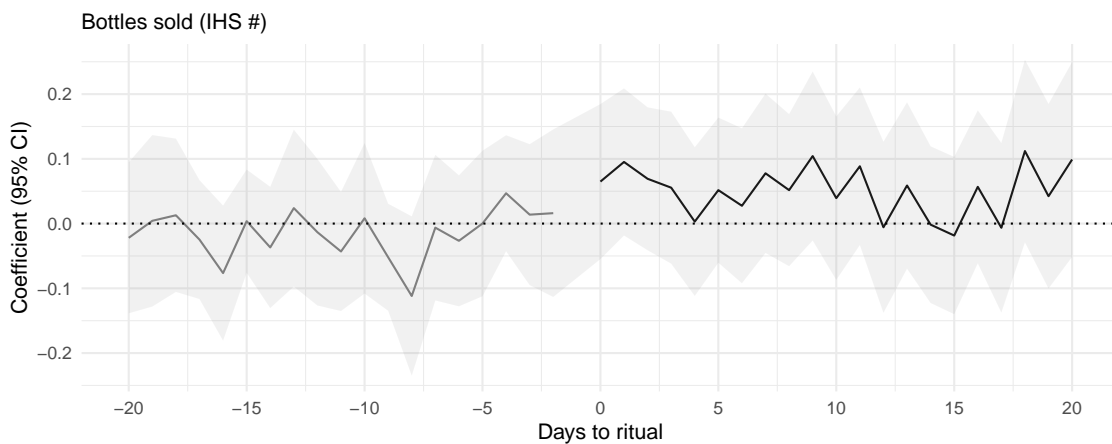
(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is number of bottles sold (IHS transformed). Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.

Figure A18: Sale prices

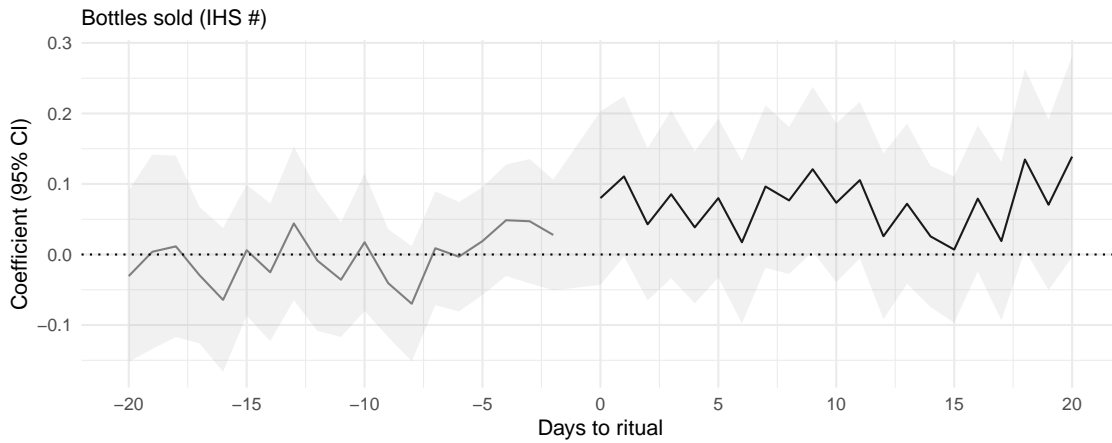**(a)** Two-way fixed effects
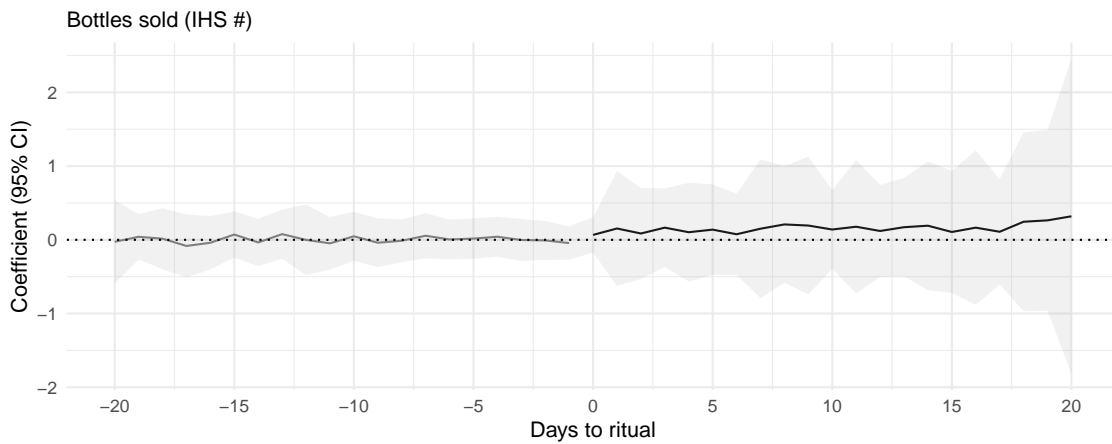

(b) Callaway and Sant'Anna (2021)

(c) Waitlist design

Notes: This figure presents the dynamic difference-in-differences estimates underlying the estimates in Table 5. The dependent variable is sale prices of bottles sold. Standard errors are clustered by beer sellers and corrected using the Bonferroni procedure based on four hypotheses.

B. *Instrumental Variable Estimates*
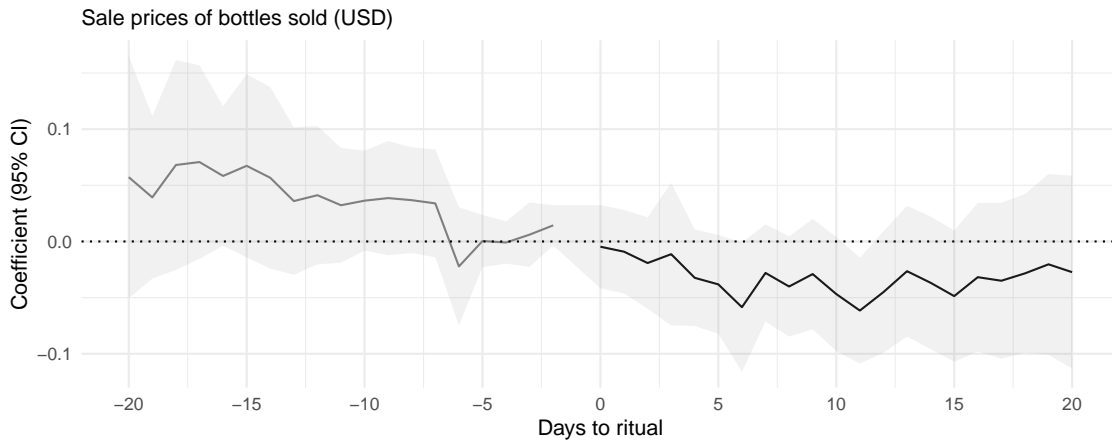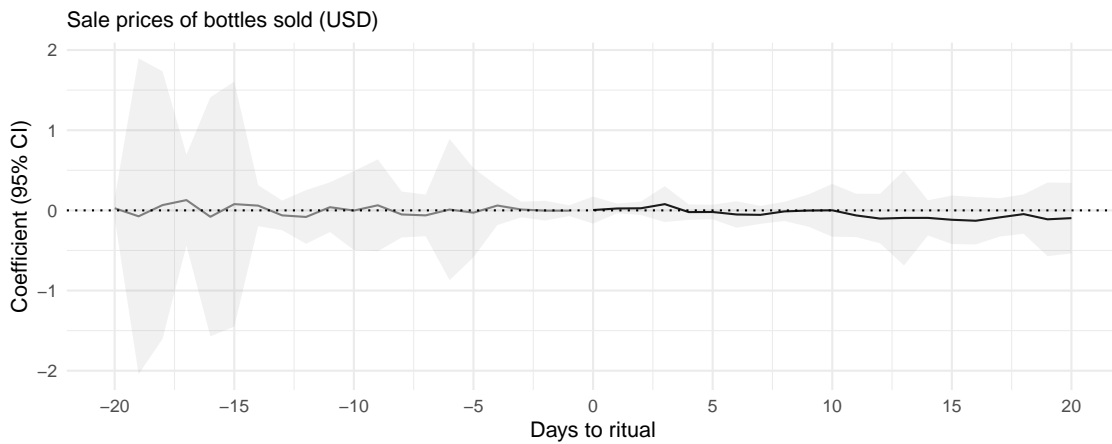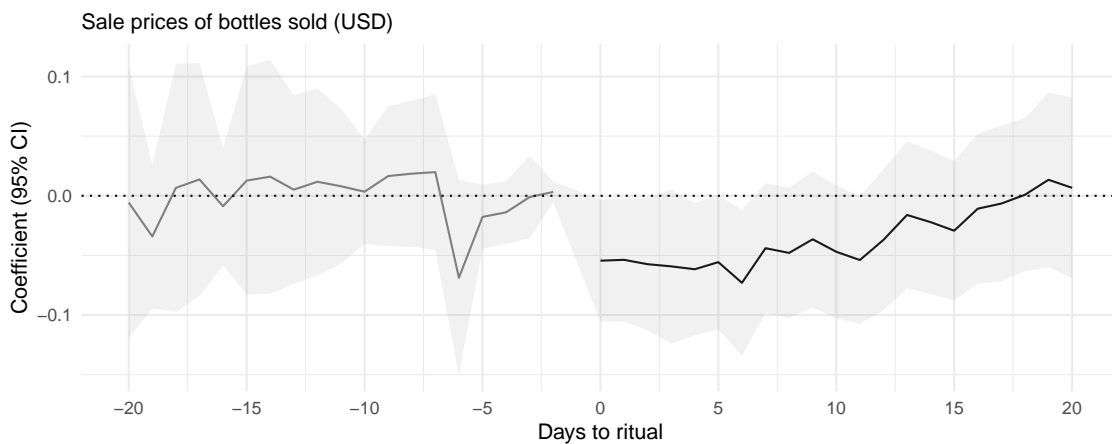
a. Violations to the randomized procedures

In general, the randomization protocol was implemented as planned. However, sellers and traditional practitioners had to deviate from the protocol in some cases, primarily due to security considerations. This section provides details on these cases.

Sellers R.A.10, R.A.11, R.A.12, R.A.13, and R.A.17, all located in Virunga Park, the most dangerous zone in the region, were scheduled for the July 28th, 29th, 31st, August 1st, and 2nd, respectively. A significant spike in killings and kidnappings made it impossible for our local collaborators to travel safely across Virunga Park. Instead, they moved to one location with a military escort and coordinated with the sellers to come to that location to receive the rituals. sellers R.A.10, R.A.12, and R.A.17 arrived and received the ritual on the same day, July 27th. Seller R.A.11 received the ritual on July 28th, and seller R.A.13 received it on July 29th.

Sellers W.B.11, W.A.11, and W.A.13 were scheduled for July 23rd, 24th, and 25th, respectively. Their rituals were delayed by one day (to July 24th, 25th, and 26th, respectively) because the local security situation made it impossible to travel on July 22nd.

Seller R.A.14, scheduled for July 17th, was unable to travel the day of his ritual. He received the treatment on July 26th, the next possible day the traditional practitioner had capacity.

b. Estimates

Table A6: IV estimates: beliefs and emotions

	Believers				Non-Believers		
	Belief in active amulet indicator	Theft expectations [0-1]	Theft indicator	Stress level [1-3]	Theft expectations [0-1]	Theft indicator	Stress level [1-3]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Two-way fixed effects						
Post-Ritual	0.627	-0.060	0.006	0.067	0.038	0.016	0.125
95% CI	[0.291, 0.959]	[-0.233, 0.087]	[-0.042, 0.050]	[-0.215, 0.405]	[-0.025, 0.133]	[-0.050, 0.105]	[-0.219, 0.676]
	Panel B: Waitlist design						
Post-Ritual	0.598	-0.069	-0.002	-0.021	0.037	0.007	0.108
95% CI	[0.288, 0.876]	[-0.259, 0.090]	[-0.027, 0.022]	[-0.113, 0.072]	[-0.001, 0.106]	[-0.033, 0.057]	[-0.142, 0.483]
	All panels:						
Observations	818	788	818	810	351	352	352

Notes: This table presents IV estimates of the effects of the ritual on the belief in active amulet indicator, theft expectations, theft indicator, and reported stress level. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The Olea and Pflueger (2013) effective first-stage F statistic is 188 (in column 1). Means and standard deviations of the dependent variables are computed based on untreated observations.

Table A7: IV estimates: purchases, inventories, and stock-outs

	Believers			Non-Believers		
	Bottles purchased (IHS #)	Bottles in stock (IHS #)	Stock-outs	Bottles purchased (IHS #)	Bottles in stock (IHS #)	Stock-outs
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Two-way fixed effects					
Post-Ritual	0.031	-0.035	-0.019	-0.067	-0.085	0.051
95% CI	[-0.052, 0.118]	[-0.169, 0.082]	[-0.099, 0.064]	[-0.165, 0.034]	[-0.417, 0.183]	[-0.151, 0.274]
	Panel B: Waitlist design					
Post-Ritual	0.027	0.059	-0.062	-0.041	-0.028	0.051
95% CI	[-0.017, 0.074]	[-0.029, 0.165]	[-0.151, 0.009]	[-0.099, 0.028]	[-0.182, 0.117]	[-0.070, 0.222]
	All panels:					
Observations	12,394	12,617	760	5,494	5,223	293

Notes: This table presents IV estimates of the effects of the ritual on number of bottles purchased, number of bottles in stock, and share of brands that are out of stock, as defined in the text. The first two outcomes are inverse hyperbolic sine (IHS) transformed. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The Olea and Pflueger (2013) effective first-stage F statistic is 285 (in column 1, Panel A). Means and standard deviations of the dependent variables are computed based on untreated observations.

Table A8: IV estimates: revenues, bottles sold, and sale prices

	Believers			Non-Believers		
	Revenues (IHS USD)	Bottles sold (IHS #)	Sale prices of bottles sold (USD)	Revenues (IHS USD)	Bottles sold (IHS #)	Sale prices of bottles sold (USD)
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Two-way fixed effects					
Post-Ritual	0.034	0.025	-0.011	-0.028	-0.021	0.012
95% CI	[-0.031, 0.103]	[-0.035, 0.085]	[-0.073, 0.057]	[-0.121, 0.059]	[-0.104, 0.057]	[-0.059, 0.084]
	Panel B: Waitlist design					
Post-Ritual	0.037	0.029	0.032	-0.021	-0.021	0.066
95% CI	[-0.015, 0.097]	[-0.018, 0.081]	[0.005, 0.064]	[-0.109, 0.053]	[-0.106, 0.052]	[-0.029, 0.150]
	All panels:					
Observations	12,936	12,936	3,905	5,627	5,627	1,531

Notes: This table presents IV estimates of the effects of the ritual on revenues (converted to USD), number of bottles sold, and sale prices of bottles sold (converted to USD). The first two dependent variables are inverse hyperbolic sine (IHS) transformed. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. The Olea and Pflueger (2013) effective first-stage F statistic is 196 (in column 1, Panel A). Means and standard deviations of the dependent variables are computed based on untreated observations.

Table A9: Difference-in-differences IV estimates (believers vs. non-believers)

	Theft expectations [0-1]	Theft indicator	Stress level [1-3]	Bottles purchased (IHS #)	Bottles in stock (IHS #)	Stock-outs	Revenues (IHS USD)	Bottles sold (IHS #)	Sale prices of bottles sold (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Two-way fixed effects								
Post-Ritual	0.036	0.013	0.170	-0.036	-0.118	0.095	-0.024	-0.024	0.030
95% CI	[-0.034, 0.113]	[-0.035, 0.064]	[-0.190, 0.589]	[-0.102, 0.028]	[-0.323, 0.079]	[-0.065, 0.277]	[-0.113, 0.057]	[-0.109, 0.054]	[-0.062, 0.118]
Believer × Post-Ritual	**-0.095**	**-0.007**	**-0.118**	**0.061**	**0.095**	**-0.131**	**0.062**	**0.056**	**-0.048**
95% CI	**[-0.282, 0.085]**	**[-0.051, 0.033]**	**[-0.468, 0.157]**	**[-0.007, 0.128]**	**[-0.089, 0.282]**	**[-0.314, 0.032]**	**[-0.031, 0.159]**	**[-0.032, 0.147]**	**[-0.134, 0.050]**
	Panel B: Waitlist design								
Post-Ritual	0.037	0.007	0.108	-0.041	-0.028	0.051	-0.021	-0.021	0.066
95% CI	[0.000, 0.106]	[-0.033, 0.057]	[-0.139, 0.483]	[-0.100, 0.022]	[-0.182, 0.120]	[-0.068, 0.226]	[-0.108, 0.055]	[-0.102, 0.054]	[-0.029, 0.151]
Believer × Post-Ritual	**-0.105**	**-0.010**	**-0.129**	**0.068**	**0.087**	**-0.113**	**0.059**	**0.050**	**-0.033**
95% CI	**[-0.290, 0.071]**	**[-0.060, 0.036]**	**[-0.470, 0.122]**	**[-0.003, 0.139]**	**[-0.079, 0.254]**	**[-0.279, 0.023]**	**[-0.031, 0.154]**	**[-0.035, 0.139]**	**[-0.113, 0.055]**
	All panels:								
Observations	1,139	1,130	1,162	17,888	17,840	1,053	18,563	18,563	5,436

Notes: This table reports difference-in-differences IV estimates of the effects of the ritual and its interaction with the believer dummy on all dependent variables. All panels report estimates and 95% confidence intervals based on seller-clustered bootstrap standard errors with 10,000 iterations and corrected using the Bonferroni procedure based on four hypotheses. Means and standard deviations of the dependent variables are computed based on untreated and non-believer observations.

Not for Publication:

Supplemental Online Ethics Appendix

On the Importance of African Traditional Religion for Economic Behavior

Lewis Dunia Butinda, Aimable Amani Lameke, Nathan Nunn, Max Posch, and Raul Sanchez de la Sierra

Appendix B. Ethics Appendix

Given centuries of racialized depictions of African traditional religious beliefs as false, "backwards," or associated with the devil, there is a concern that Western researchers may contribute to perpetuating false and infantilizing stereotypes when they study, or experiment with, such beliefs. This is especially concerning in situations like this study in which researchers from the West, who tend not to hold these beliefs, have the ability to experiment with them, while individuals who hold the beliefs are the participants in the study.

For this reason, we begin our ethics discussion by presenting what, in our view, is the central ethical challenge of the study and discussing the steps we have taken to mitigate it—the success of these steps at mitigating some of the largest concerns is also why we decided to proceed with this research in the first place. We then discuss standard ethical problems that can arise in any experiment.

A. The Potential Ethical Problem: Positionality

The first two authors on the project, Lewis Butinda and Aimable Lameke, are from the same population and region as the participants of the study. The other three coauthors, Nathan Nunn, Max Posch, and Raul Sanchez de la Sierra, are all Western-based scholars who hold non-negligible positions of power relative to the Congolese research team members and the

experiment participants. The majority of these Western scholars, naturally, do not hold the same beliefs as the Congolese researchers, many of whom consider the beliefs sacred. The asymmetry of power and beliefs is an important fact about the research taking place.

B. *How the Context of this Study Nuances this Potential Problem*

Demand for accessing religious practitioners' rituals emerged from the participants themselves. This came out of discussions following a longstanding relationship between the participants (the beer sellers) and the data collection organization that hired them in the two years leading up to the study (Marakuja).

Two co-authors (Amani Lameke and Sanchez de la Sierra) helped to create the research organization, Marakuja, in 2015. The organization is a Congolese, pan-African organization composed of eight founding members, including seven Congolese graduates from the Kivu region. The relationship with the eight founding members and the informal external members of Marakuja extends to 2009 when a continuous relationship of work and friendship began.

The non-profit organization managed the data collection over multiple years and built a relationship with the beer sellers, which provided the foundation for the current study. Before the experiment, Marakuja had worked for three consecutive years with the sellers, and Amani Lameke and Sanchez de la Sierra had also been involved in building relationships of trust and mutual help with them. These relationships resulted in consultations with the beer sellers in 2017. Marakuja was concerned about how they could thank the beer sellers for providing their business data, which they had sent in exchange for appropriate and pre-negotiated compensation for their costs and time over two years. Marakuja's interest, at the time, was to extend a symbolic but potentially costly gesture of gratitude in recognition of the beer seller's work.

In those rounds of consultations, while all beer sellers mentioned direct monetary support as being the preferred avenue to support them, most of them requested help getting an amulet to help them succeed with their business. Surprising to the Western researchers (but not the Congolese researchers), a protection ritual was preferred over hiring a security guard, even though the costs are comparable, because there is no guarantee that the guard would not rob the beer seller. By contrast, there was greater certainty that a protection amulet would work. Thus, the request to have a subsidized purchase of a protection amulet was driven by the beer sellers themselves, not by the researchers, and was the origin of our experiment.

An important question is whether beer sellers felt free to participate into the treatment. Believers gained access to a service they had previously demanded, and hence cannot have been coerced. For non-believers, the picture is more nuanced: how sure can we be that they did not feel coerced to taking the ritual? Why did they do it if they did not believe in the efficacy of the rituals? Importantly: did they feel that they would lose future employment opportunities with Marakuja if they refused to participate?

We addressed this eventuality in how the intervention was communicated to the beer sellers. Every beer seller was offered the option to participate in the ritual, or to opt out. Participation in the ritual was simply a way for Marakuja to provide a gift for participating in data gathering for 2-3 years before the experiment (for which they had received compensation and reimbursements), symbolizing the end of the period. Marakuja made abundantly clear that beer sellers were free to participate, and that there were no expectations of future benefits being dependent on participating in the ritual. Debriefing conversations with all beer sellers indicate that the non-believers were often amused by the idea, certainly not bothered, and did not incur any loss from participating. We note the case of one non-believer who, a few days after the ritual, decided to throw the amulet in the toilet.

In addition to the communication, the beer sellers had no reasonable basis to expect future employment opportunities by Marakuja. Regarding the possibility to be recruited as participants in future research studies, the beer sellers are located across hundreds of remote villages in North Kivu, and Marakuja is not specialized in beer — it is in fact the only study conducted over eight years involving beer sellers. With regard to the possibility to be recruited as data collectors for Marakuja, beer sellers also do not have the required level of education to be hired as data collectors in future studies and, while they are the elite of the rural areas, the level of their skills cannot match that of university educated urban young elites — which is typically the labor market where Marakuja recruits.

a. The Positionality of the Western and Congolese Co-Authors

All co-authors of this paper consider the indigenous-religious beliefs studied in this paper to be, at least partly, true. One of the Western-educated co-authors has carried amulets ever since the first pilot for this study and has reactivated the amulets each time he traveled to the DRC.[20]

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[20]The first spell this co-author received was the power to convey ideas in papers — it did not include writing skills.

While this is rare for Western-educated researchers, it is nonetheless a detail that informs the positionality of the authors, *vis-a-vis* the study and the participants.

b. There Was No Allocation of Participants to a Treatment and a Control Group

It is important to emphasize that we did not create a control and a treatment group. Instead, we responded to the beer sellers' request by facilitating access to a subsidized ritual for all beer sellers. This staggered timing of the delivery helps reduce the problematic positionality of researchers "experimenting" with the participants' fate by giving a ritual to some and not others, the central ethical challenge in this study, as well as some of the standard concerns that we discuss in what follows.

C. Standard Equipoise Concerns

An ethical dilemma arises in a clinical trial when the researchers believe that the treatment is significantly outperforming the control or even that the treatment is clearly under-performing the control. Equipoise as a principle makes sense as a normative assumption for clinical trials if one assumes that researchers have therapeutic obligations to their research participants. Here, we discuss whether the intervention provided by the local NGO was known (or expected) to produce benefits or harm to the participants, and whether random assignment violated any therapeutic obligation towards some of the study participants.

The principle of equipoise is not violated in this study for a very simple design feature: in our study, all participants that opted into the experiment received the amulet. The only difference between participants was the day on which they would become eligible. Even more importantly, the traditional practitioners were working at full capacity, thus the role of the researchers was to introduce randomness into a treatment assignment process that was already unable to deliver to a larger number of participants in a given time. This feature rules out concerns that arise if our team believed prior to the study that the amulet was clearly going to outperform not having an amulet.

Furthermore, at the time of the study design in 2017, there was genuine uncertainty among the members of the research team, and in the general scientific community, regarding the benefits of providing (voluntary) access to the amulets.

Regarding the existing knowledge in the scientific community, there is no body of clinical research on the effectiveness of rituals and amulets. Indeed, this paper is the first to experimentally examine whether the treatments of interest have any positive effect. For this reason, while there is ample informal knowledge, which is based on experience, there was no expert knowledge on the effectiveness of these rituals as recognized science by Western science standards.

Regarding the beliefs held by the research team, we ensured that there was certainty that the treatment would *not* have adverse effects. Indeed, prior to starting the study, the study's team amassed a wealth of qualitative and quantitative evidence about the potential mental health benefits of the amulets, as well as about their potential to correct beliefs about theft that were seemingly biased upwards.

D. Modified Equipoise Concerns

A potential concern is that if the Western researchers of the team do not hold the religious belief under study, then the treatment could be perceived as misguiding the participants and inducing them to make suboptimal or misinformed decisions. As the following discussion of the steps taken by the team explains, the treatment was deemed to be beneficial for participants.

In the first step, the research team collected the beer seller data using the smartphone system for two years prior to the assignment to the intervention. The data collected during that period provides the first quantitative motivation supporting the notion that our treatments could have positive welfare effects on the participants. The data include beer sellers' self-reports about the likelihood of theft as well as the actual occurrence of theft. We found that sellers experience anxiety and over-estimate the risk of theft, consistent with the existing qualitative evidence of amulets and rituals (Malinowski, 1948). Figure 7 shows that, while non-believers do not update their expectations about theft, untreated individuals over-estimate the probability of theft.

In the second step, during the two years preceding the assignment to the treatment, Marakuja (the local non-profit research organization) consulted the participants during an extended period of time, trying to identify ways in which their (compensated) cooperation entering the data of their retail business during two years could be further compensated. The beer sellers' responses show that, in addition to financial support, the sellers overwhelmingly requested access to amulets to increase luck in their business or to decrease the occurrence of negative events like

theft. The price of such rituals is often prohibitive for these sellers. Thus, the design followed widespread demands for these amulets among beer sellers.

In the third step, prior to deciding whether to provide access to the amulets and rituals, the research team, in collaboration with Marakuja, created a committee composed of members of various ethnic groups (and thus knowledgeable in different types of rituals) to assess the validity of the different rituals and amulets under consideration. For each ritual, the committee members were unanimous in their views about the legitimacy of the rituals.[21]

E. Role of Researchers with Respect to Implementation

The researchers all played an active role in the research. Three of the authors were, at the time of the study, founding members of the non-profit organization, delivering the amulets. They had full control over the design of the research and its implementation. One of them (Sanchez de la Sierra) is no longer formally a member, but provides scientific advice informally to Marakuja. While Marakuja's overhead financed the costs of the rituals, the supervisors and implementers were paid by Marakuja, using funds for data collection provided by the Private Enterprise Development in Low-Income countries grant. Of particular importance is that neither Marakuja nor the other researchers had any stake in demonstrating the effectiveness of the intervention. Marakuja is a research organization specializing in data collection and receives no compensation by any provider of the research, nor are the results of the research publicly disseminated by them, nor is the identity of the religious practitioners that took part in delivering the amulets disclosed.

As noted, the delivery of the treatment came following demands from the participants. After two years collecting data on their businesses, they volunteered to the research organization ideas about how they could be further compensated for their efforts. No researcher, at any given time, exerted any type of pressure on anyone to undergo treatment – quite the reverse, the business owners did express that they were impatient over waiting to receive the benefits they had suggested.

The treatment was presented as an additional, and last, compensation for two years of data entry by the business owners, given at a time when all business owners knew that data collection

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[21]During focus groups that followed each pilot rituals, the committee members discussed the reasons why a particular ritual was correct and legitimate. The members of the committee expressed disapproval for some of the religious practitioners, believing that they were not authentic and had performed invalid rituals.

would be terminated in the upcoming months, and that it was independent of their decision of whether to participate in the treatment.

No member of the research team tried to convince the participants about the efficacy or validity of the amulets. The religious practitioners that were selected through our screening process were generally well accepted and known in their communities. Importantly, the participants were not told that Marakuja was taking steps to assess the validity of the religious practitioner, as this could have sent a signal about the credibility of the practitioners, that we wanted to avoid. The beer sellers were never aware of the steps we took and we simply told that a local practitioner would be subsidized.

The informed consent process to participants (as well as the intervention and data collection was approved by the Institutional Review Boards (IRB) of Berkeley (Protocol: 2016-06-8849; FWA: 00006252), Chicago (Protocol: IRB20-0927), and Harvard (Protocol: SITE18-0356; FWA: 00004837)

Informed Consent: Determination of Waiver of One Component of Informed Consent

We obtained informed consent from the study participants for providing their business data.

We obtained a waiver from the Berkeley ethics review board for one component of the informed consent process. We obtained a waiver for the component of the informed consent that provided information to participants of their participation in a randomized rollout designed study that is aimed at determining the effects of the rituals on business practices. We sought the waiver because if we to inform participants that their business decisions were used to measure the outcome of the amulet, they could have had incentives to manipulate those in order to obtain future treatment.

In order for a waiver of one (or more) components to be granted, either of two criteria had to be met. We hereby indicate the criteria as well as our determination for each criterion, as stated in our IRB protocol (Protocol: 2016-06-8849; FWA: 00006252):

Consent/Waiver Description

Criterion A*: yes*

(1) The research involves no more than minimal risk of harm to the participants;

(2) The waiver or alteration will not adversely affect the rights and welfare of the participants;

(3) The research could not practicably be carried out without the waiver or alteration; and

(4) Whenever appropriate, the participants will be provided with pertinent information after participation.

I affirm that the study meets criteria 1-4 above related to the luck amulet amendment procedures.

Related to point number 3, letting the participants know that this is part of a research will bias their interpretation of the amulet and their behavior, leading to misleading and unreliable results. The only way to carry out this research is to let this organic transaction take place, and use the measurement system already developed and approved to measure the effects as described.

Criterion B: *not met*

(1) The research or demonstration project is to be conducted by or subject to the approval of state or local officials and is designed to study, evaluate, or otherwise examine: (i) public benefit or service programs; (ii) procedures for obtaining benefits or service; (iii) possible changes in or alternatives to those programs or procedures; or (iv) possible changes in methods or levels of payment for benefits or services under those programs; and

(2) The research could not practicably be carried out without the waiver or alteration."

Our determination for point (1)*"the research involves no more than minimal risk of harm to the participants"* is based on the step-wise procedure detailed in the section of this appendix *"Equipoise."* Our determination for point (2) was based on the fact that all participants who expressed their desire to obtain an amulet were ultimately receiving the amulet, and not disclosing the purpose for the delay (rather than withholding of treatment) did not constitute a significant violation of participant rights. Not knowing that they were part of an experiment also did not harm their welfare.

As stated in the IRB amendment, our determination for point (3) was that if the participants knew that they were part of an experiment, knowledge thereof could motivate them to change their responses and their choices. As they were also the individuals reporting on their business, they could, for example, have inflated the revenues if they had the expectation that if the experiment shows positive results, they would receive additional amulets. The risk of knowledge of it being an experiment influencing their business choices was a threat to the validity of the experiment.

Our determination for point (4) was that Marakuja would debrief the participants at the end of the study to share with them the results of the study, letting them know that random variation in the rollout enabled conducting statistical analysis to measure its effects on business outcomes. This debrief has already occurred informally and by phone, conducted by one of the authors when attempting to interpret qualitatively the results of our analysis.

Informed Consent: Implementing the Waiver of Informing of Participation in an Experiment

Having obtained waiver from IRB, we implement the informed consent process communicating the following text to our participants. This text is quoted from Section 13 from our IRB application to Berkeley, under Confidentiality and Privacy notes. We quote the relevant portion here to illustrate what was communicated to the participants, upon approval by the IRB:

> *"The organization will not disclose to anyone which beer seller accepted to take the amulet. We record the meeting by phone and store it in a password protected computer in Berkeley. However, the audio contains no identifiable information related to the seller and will not be analyzed for research purposes. This recording is for study related quality assurance only."*

F. Potential Harms to Research Participants from the Interventions or Policies

The participants are among the best educated and most well-off in the communities we study. However, there is the concern that treatment induced participants into suboptimal behavior – inducing them to taking risks or over-investing – potentially leading to economic losses and even an increased risk of theft. Our initial conviction was that the intervention would not cause harm. This conviction is due to the three steps that we undertook to mitigate the risk that the study caused harm to the participants, which we have described in Section D. Based on these steps, we found that, even if the amulets do not have any direct effect on luck as purported by the religious practitioner, business owners have biased beliefs over the probability of adverse outcomes. Specifically, the business owners tended to over-estimate the probability of theft and were operating with anxiety, which potentially undermined their ability to make optimal decisions. Given the potential of the amulets to reduce anxiety and to correct biased beliefs that otherwise lead to suboptimal under-investment, we determined that the intervention entailed minimal risks of harm.

The findings of our study underscore that this concern was not warranted. The beer sellers over-estimated the probability of theft, the amulets partially correct such bias, leading business owners to increase revenue. In short, participation in the ritual made them better off.

Two remaining aspects that could potentially cause harm are important to consider: coercion and monetary costs to participants.

Coercion: participants were at no point compensated nor incentivized to undergo the treatment. They had been compensated for two years for their data collection. The delivery of the treatment was thus consensual, and there is thus no risk that compensation for taking the treatment (which was not present) might have been coercive, potentially leading participants to be exposed to risk in order to obtain compensation. The amulet was voluntary, and was perceived itself as a part of compensation for the data collection.

Costs to participants: to obtain a valid amulet, participants had to travel typically one hour to a central location where the religious practitioner provided a ritual, typically lasting one hour, to the participant. The transport costs and overnight accommodation, if necessary, were covered by Marakuja. Thus, the participants did not incur any financial costs to receive the amulet. The opportunity cost of their time away from the business was mitigated by the fact that all of them had a family member or a business partner who kept running the operations during their absence, and entering data to ensure there was no gap in data nor economic loss to the beer seller for not entering data.

In addition, we created a monitoring system enabling us to get real time information about adverse events and respond immediately.[22]

G. Potential Harms to Research Participants from Data Collection

Data collection and/or research procedures are adherent to privacy, confidentiality, risk management, and informed consent protocols with regard to human subjects as described in the IRB protocols disclosed in this study. They are also respectful of community norms and communities understood that only the beer sellers would be hired for reporting data on the beer retails. The detailed data collection protocols were approved by the Institutional Review Boards (IRB) of

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[22]Of all beer participants and in the entire experimental period, we learned of one participant who had received the amulet and nonetheless was victim of theft. The bandits stole the solar charger that Marakuja had given him to charge his smartphone for the data entry. The charger was replaced by Marakuja.

Berkeley (Protocol: 2016-06-8849; FWA: 00006252), Chicago (Protocol: IRB20-0927), and Harvard (Protocol: SITE18-0356; FWA: 00004837). Here we provide a brief summary.

Risks regarding storage are below reasonable risk, and comparable to any data collection project today using state-of-the-art data collection technology. We used ODK with storage of the data in a password-protected computer, in a password-protected folder by the PI's. Even if the data were to be accessed for malicious purposes, the retail's accounting would be of little use to damage the seller's reputation or business operations, as this type of activity and the associated profit is relatively well known in the context. Given the nature of the questions (business actions, quantities, prices), there is no discomfort associated to enter these questions. The participants had been entering the data for two years under a tight monitoring structure and were content with the exercise.

There are costs for participants to enter the data, because the participants are the beer sellers themselves who, every day, enter the accounting information about their business and then incur financial costs to upload the data to the ODK corresponding server. To mitigate this cost, we compensated the beer sellers 15 USD monthly for their time, and 4.4 USD weekly of additional income to compensate for the costs of uploading the data to the server using the phone network. This compensation was the same for the two years before the experiment and was identical whether or not they participated in the experiment. Overall, the beer sellers viewed this activity as part of their job, and we estimate that the income we provided was slightly superior to the financial and opportunity cost of the task.

H. Potential Harms to Nonparticipants

Real risks to non-participants would exist: (i) if the treatment is effective to deter potential thieves from targeting a treated shop; and (ii) if this deterrence translates into the displacement of their intended crime towards other beer sellers.

For this to hold it must be that the treatment deters thieves. In principle, if beer sellers had advertised their amulet, it is possible that it would have been effective at deterring believing thieves. However, the beer sellers went at length to keep the treatment secret and asked us to protect their privacy in accepting the treatment, because they were concerned that, if other neighbors found out, the neighbors would begin suspecting them in using witchcraft for malicious purposes. Since

the amulet is easy to hide, it was not surprising to us that we recorded that none found out about the existence of the amulet.

For this reason, this type of spillover risk to non-participants is unlikely.

I. Potential Harms to Research Staff

The communities of the study are communities where the research staff had undertaken research since 2015. The staff are familiar with those communities and some staff members came from these communities directly. There were known conflicts concurrently occurring in North Kivu, involving different armed actors in different districts.

To mitigate the risks associated with traveling through insecure areas, the team had daily access to INSO alerts about all the security incidents in the region, provided to the NGO and humanitarian community operating in the area to plan their operations, and based on an extensive network of informants.

Marakuja furthermore had an extensive network themselves. We used this information on a daily basis to determine whether research staff were allowed to travel to certain areas. Since the visits were not regular, this was not an impediment to the project. We had to prevent a supervisor from visiting the area of Rutshuru for a significant period, as there was a risk of kidnappings.

For security reasons, we also excluded the areas of Beni and Lubero from the study of amulets, even as those had been providing data for two years, as we could not guarantee the safety of the research staff traveling in those areas.

The management of the data entry was then conducted by phone for beer sellers in those areas, and is thus of lesser quality for that period (and outside our experiment sample).

There is no other risk to the research staff regarding political violence, disease, mistrust, or emotional difficulty other than that which they normally are exposed to in the absence of the study as they operate in their political and climatic environment where their actions are not subject to any form of mistrust.

J. Scarcity

Random assignment influenced only the timing of who got the amulet. The study is a randomized rollout where all participants received the treatment, hence both treated and control participants ultimately received the treatment, with a maximum gap of 40 days. The study took advantage

of a limited research budget that helped finance data entry compensation in the form of amulets. There was scarcity in the budget, not enabling distribution to participants outside the experimental sample - but everyone in the experimental sample did receive the treatment. Neither the treated nor the control individuals (the experimental sample), nor the individuals outside the experimental sample, would have received the treatment had it not been for the research.

K. Counterfactual Policy

Had the research not been conducted, is the counterfactual situation that would have happened instead predictably better for participants than what they actually received in any of the arms of the study?

The study is a randomized rollout where all participants received the treatment, hence this question does not apply. There was furthermore no program to our knowledge in the area whose benefits were dependent on the ownership of an amulet.

L. Researcher Independence

There were no contractual limitations on the ability of the researchers to report the results of the study.

M. Financial Conflicts of Interest

Three of the authors were, at the time of the study, founding members of the non-profit organization, delivering the amulets. One of them (Sanchez de la Sierra) is no longer formally a member, but provides scientific advice informally to Marakuja. Marakuja is a research organization specializing in data collection and receives no compensation by any provider of the research, nor are the results of the research publicly disseminated in the population, nor is the identity of the religious practitioners that took part in delivering the amulets disclosed.

While Dunia Butinda and Amani Lameke are hired by Marakuja as research managers and field supervisors regularly, their compensation is not tied to the outcome of the research on the efficacy of amulets. Sanchez de la Sierra receives no compensation from Marakuja.

N. Reputational Conflicts of Interest

A reputational conflict of interest is one in which prior writing or advocacy could be contradicted by specific results pursued in this study, and such contradiction would pose reputational risks to the author.

There is no reputational conflict of interest. No author ever advocated for the efficacy of the amulets provided by anyone, nor by the specific witch doctors.

A negligible reputational conflict of interest exists at the level of the religious practitioners. It is possible that, had the religious practitioners been informed that this was an experiment rather than a simple delivery (they were not), they would have had incentives to influence the members of Marakuja to, in turn, influence the beer sellers to report more sales and lower rates of theft after the delivery of the amulets.

While it is clearly in their interest to do so: (i) the religious practitioners had no way to compensate Marakuja to induce them into malfeasance because Marakuja's expected lifetime income is orders of magnitude higher if they nurture a healthy relationship with the University members and colleagues due to the future projects they can receive; (ii) Marakuja had, in 2017, a years long relationship with the Western-educated members of the team, but had no relationship with the religious practitioners; (iii) the deontology of the members of Marakuja cannot be influenced by a few dollars of bribes, which is what would be required for the religious practitioners to influence Marakuja; (iv) religious practitioners were never informed that they were part of an experiment and were just asked to deliver one ritual per day to beer sellers that would come in an order pre-determined by Marakuja.

O. Feedback to Participants or Communities

Our decision was that Marakuja would debrief the participants at the end of the study to share with them the results of the study, letting them know that random variation in the rollout enabled conducting statistical analysis to measure its effects on business outcomes. This debrief has already occurred informally and by phone in the years 2018-2020, conducted by one of the authors when attempting to interpret qualitatively the results of our analysis.

We did not plan to provide the results of our study to the religious practitioners, who are not human subjects of the research per se, to avoid them using the results to increase their sales or for

other purposes. De-linking the research from the religious practitioners also provided additional protection to the integrity of the research regarding potential financial conflicts of interests that may ensue if the religious practitioners had incentives to share a share of their expected gains in sales in exchange for a positive result. We did not have any contact with the religious practitioners since the day of the last ritual.

P. Foreseeable Misuse of Research Results

Religious practitioners that gave the amulet could have incentives to use the results of the research to increase their sales and even to support the validity of amulets that were not part of this study.

To mitigate this risk, we did not provide the results of our study to the religious practitioners, who are not human subjects of the research per se. De-linking the research from the religious practitioners also provided additional protection to the integrity of the research regarding potential financial conflicts of interests that may ensue if the religious practitioners had incentives to share a share of their expected gains in sales in exchange for a positive result. We did not have any contact with the religious practitioners since the day of the last ritual.

The beer sellers, to whom we disclosed the results, could use this information, if misinterpreted, to increase their purchases of amulets and rituals. While our research provides evidence for the benefit of one amulet, we do not have evidence for how this scales up to a larger number of amulets, let alone other amulets. For this reason, the acting on these results by the beer sellers, if not advised, could lead to adverse choices. To mitigate this risk, we trained participants to interpret the results as confined to a specific amulet and that those results should not be taken as evidence for other types of amulets or for a larger number of amulets