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September 15, 2026

Professors Emily Breza, Davide Viviano, and Arun Chandrasekhar
Workshop on Development Economics and Networks
National Bureau of Economic Research

Dear Professors Breza, Viviano, and Chandrasekhar,

I am writing to apply for the Workshop on Development Economics and Networks for Early-Career Researchers. My research interests lie in development economics and political economy, with a focus on how informal institutions sustain collective action.

I would like to present my job market paper, *Can Religion Bridge Ethnic Divides? How Pastors in DRC Promote Collective Action Where Chiefs Cannot.* The paper studies a field experiment embedded in a voluntary public-works program in the Democratic Republic of the Congo. We cross-randomize local leadership, team ethnic composition, and the disclosure of individual contributions to examine whom leaders influence and how they do it. The project speaks to the workshop's focus on empirical networks by studying how ethnic networks may facilitate or constrain local authorities' ability to shape cooperation.

I would welcome the opportunity to receive feedback on this work and learn from the lectures and other participants' research. Please find attached my CV with a list of current and completed PhD courses, a statement of research interests and motivation, and the working paper. You can reach out to Frederico Finan if I am shortlisted:

Name:	Professor Frederico Finan
Affiliation:	University of California, Berkeley
Email:	ffinan@berkeley.edu

Thank you for considering my application.

Sincerely,

Gabriel Granato

Gabriel Granato

Haas School of Business, University of California, Berkeley

gabrielgranato@berkeley.edu | gabrielgranato.github.io

Education

University of California, Berkeley 2027 (expected)
Ph.D. in Business and Public Policy, Haas School of Business

M.Sc. in Economics, PUC-Rio 2020

B.A. in Economics, IBMEC 2017

Research Fields

Development Economics; Political Economy; Organizational Economics

Work in Progress

Can Religion Bridge Ethnic Divides? How Pastors in DRC Promote Collective Action Where Chiefs Cannot

With Elie Kabue Ngindu
Job Market Paper

Ethnic fragmentation is thought to undermine economic development. In one prominent mechanism, greater free-riding in heterogeneous societies erodes the provision of local public goods. This paper examines whether traditional and religious leaders can sustain cooperation in a voluntary public-works program in an ethnically diverse city in DRC. Residents are randomly assigned to ethnically homogeneous or mixed work teams that either include a chief, a religious leader, or no leader at all. Chiefs raise participation by 4.8 percentage points in homogeneous teams and have no detectable effect in mixed ones; pastors raise participation by 6.3 points in mixed teams and have no detectable effect in homogeneous ones, a 9.6-point reversal in their relative advantage ($p = 0.023$). A cross-randomized disclosure intervention helps identify distinct enforcement strategies. Chiefs are effective only when each resident's own contribution is visible to teammates, consistent with social sanctioning. Pastors, by contrast, are effective only when the team record only reports aggregate contributions, consistent with internal incentives governed by a fear of divine oversight. The paper provides the first experimental evidence on when religious leaders can substitute for customary authority in sustaining voluntary public-goods provision in fragmented societies.

Preaching a Fair Share: Religious Appeals and the Moral Economy of Taxation

With Clotaire Boyer and Jonathan Weigel
RCT ongoing; endline data collection in progress.

Why should the rich pay more? An individual moral economy treats prosperity as the fruit of faith and effort, carrying a personal duty to give back. A collective moral economy emphasizes shared responsibility for poverty and support for the disadvantaged. Religious traditions place different emphasis on these accounts. We study their implications in the context of a progressive taxation reform through a randomized experiment with churches in Kananga, Democratic Republic of the Congo. The tax administration asks pastors to relay property-tax information to their congregations. We randomly assign pastors to receive this request alone, the request with suggested sermons justifying higher taxes on the better-off through individual or collective responsibility, or no materials. Pastors choose whether and how to convey these messages. Recorded sermons, congregant surveys, and administrative payment records measure message uptake, beliefs about poverty and wealth, support for progressive taxation and redistribution, and tax compliance. The experiment tests whether these appeals motivate citizens to finance the state and whether their effectiveness depends on pastors' and congregants' religious beliefs.

Legal Antecedents of Fiscal Capacity: Experimental Evidence from Congo

With Augustin Bergeron, Eva Davoine, Marina Mavungu Ngoma, James Robinson, and Jonathan Weigel
Data collection complete

This project studies a low-capacity state, the D.R. Congo, seeking to establish legal capacity and how its efforts to do so shape citizens' demand for the state. We examine the randomized rollout of a legal capacity-building program implemented at scale in the city of Kananga by the Provincial Ministry of Justice. Local property disputes are randomly assigned to receive free arbitration offered by state-licensed lawyers. We study how this legal capacity program shapes property rights security, social cohesion and trust, and citizens' willingness to fund the formal state through taxes. An information treatment helps shed light on mechanisms.

Research Grants

2026: CEPR ReCIPE Compact Research Grant, £30,000, with Jonathan Weigel, James Robinson, Augustin Bergeron, and Marina Mavungu Ngoma.

2025: Weiss Fund for Research in Development Economics, $36,894; CEPR ReCIPE PhD Research Grant, £15,000 direct research budget; CEGA Development Economics Challenge, $19,500; Center for Governance and Markets, University of Pittsburgh, $5,000; Haas Departmental Seed Grant, $30,000; Berkeley Center for Workplace Culture and Innovation, $5,000; Institute for Business Innovation, Williamson PhD Fellowship Fund, $5,000.

2024: Berkeley Economy and Society Initiative / Network for a New Political Economy, $3,454.25.

2023: J-PAL Governance Initiative, Multidisciplinary Grant, $9,999.

Fellowships and Honors

Andrew and Mary Thompson Rocca Fellowship for Dissertation Research, UC Berkeley, $10,000 (2026); Network for a New Political Economy Fellowship, UC Berkeley (2021–); Business and Public Policy Doctoral Fellowship, UC Berkeley (2020–); Sumon C. Mazumdar Fellowship, UC Berkeley; CAPES and PUC-Rio master's fellowships, including CAPES internationalization fellowship (2018–2020); highest honors and highest graduating-class GPA, Economics, IBMEC (2017); full undergraduate scholarship, IBMEC (2014–2017).

Teaching Experience

Graduate Student Instructor, University of California, Berkeley

2026	Ethics and Responsible Business Leadership, MBA (Spring)
2024, 2025	Economics for Business Decision Making, Executive MBA (Fall)
2024	Microeconomics, undergraduate advanced mathematics track (Spring)
2023	Economics for Business Decision Making, MBA (Fall)
2022	Econometrics, undergraduate (Spring)

Teaching Assistant, IBMEC

2015–2016	Linear Algebra; Statistics I; Econometrics I; Microeconomics III; Macroeconomics III; Financial Markets

PhD coursework: Economic Theory (ECON 201A–201B); Mathematical Tools for Economics (ECON 204); Econometrics (ECON 240A–240B); Applied Econometrics (ECON 244); Industrial Organization (ECON 220B); Public Economics (ECON 230A–230B); Microeconomics of Development (ECON C270A); Comparative Political Economy (POLSCI 209A); Research and Theory in Business: Behavioral Science (PHDBA 297B); Corporate Strategy and Technology (PHDBA 279C); Economic Institutions in Historical Perspective (PHDBA 279D); Doctoral Topics in Business Administration (PHDBA 297T, two courses); Workshop in Institutional Analysis (PHDBA C270); Research Seminar in Business and Public Policy (PHDBA 279S); Individual Research in Business Problems (PHDBA 299A); Individual Study for Doctoral Students (PHDBA 602); Teaching Business (PHDBA 375)[1].

Selected Presentations

2026	CEGA Research Retreat (forthcoming), Association for the Study of Economics, Religion, and Culture, Chapman University; Development and Political Economics Ph.D. Student Conference (DevPEC), Stanford University; Oliver E. Williamson Seminar, Berkeley
2025	Center for Governance and Markets, University of Pittsburgh
2020	North East Universities Development Consortium (NEUDC); Sociedade Brasileira de Econometria

[1] For internal administrative reasons at the business school, some PHDBA courses carry titles that do not fully reflect their content, which includes political economy of development, public economics, and industrial organization. While conducting fieldwork, I also took Stanford's online course *Social and Economic Networks: Models and Analysis*, taught by Matthew O. Jackson.

Research Experience

2022–2023	Research Assistant to Jonathan Weigel, UC Berkeley; fieldwork in Kananga, DRC (October 2022–July 2023)
2020	Visiting Research Student, University of British Columbia; Claudio Ferraz
2019–2020	Research Assistant to Gabriel Ulyssea, University of Oxford

Languages and Computing

Portuguese (native); English and Spanish (fluent); French (intermediate)
R, Python, Stata, MATLAB, SurveyCTO

References

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Statement of Research Interests and Motivation to Attend the Workshop

Gabriel Granato
University of California, Berkeley

September 15, 2026

My research interests lie at the intersection of development economics and political economy. I am particularly interested in how informal institutions organize collective action and complement public good provision where state reach is limited. I would like to attend the NBER Workshop on Development Economics and Networks to further develop the networks dimension of this agenda and receive feedback on my current work on the Democratic Republic of the Congo.

In my job market paper, *Can Religion Bridge Ethnic Divides? How Pastors in DRC Promote Collective Action Where Chiefs Cannot*, I study a voluntary public-works program through a collaboration with the city of Kananga, the capital of Kasai-Central, DRC. The city launched a campaign to incentivize citizen participation in the provision of public goods and organized volunteers into teams. As part of the collaboration, we randomized residents into ethnically homogeneous or mixed teams that could have included a chief, a pastor, or no leader. We also varied the extent to which teammates were able to keep track of their peers contributions. The program was designed to allow us to study both the social reach of leaders' authority and their dependence on peer monitoring.

Leveraging administrative daily participation data from the program that involved more than a thousand volunteers and hundreds of small neighborhood improvement projects, we have found that religious authorities succeeded in promoting collective action in ethnically fragmented groups where traditional authorities have failed. Importantly, they were especially more likely to succeed when information about participation was unavailable and social enforcement was the hardest. In contrast, we find that religious leadership consistently failed to sustain participation in ethnically homogeneous teams. Chiefs, on the other hand, were effective in sustaining higher participation only when in co-ethnic groups and when contribution tracking was available.

We further combine the administrative dataset with rich survey data that elicited participants' relationships and networks to discuss a conceptual framework about dimensions of leadership that are rarely discussed in tandem: leaders' scope of authority—whom each leader can mobilize—and their source of enforcement—what makes participation consequential. We establish that the chief's scope rests on his personal relationships, only reaching individuals who recognize his authority, and that their source of enforcement works through social incentives related to an *external* image. Contrastingly, we show that pastors' scope depends on the composition of the group, succeeding when groups are fragmented and failing when they are cohesive, while their source of enforcement consists of religiously motivated obligations related to an *internal* self-image.

A next step in this agenda is to examine how existing networks shape collective action and how collective action, in turn, reshapes those networks. Together with the experiment, I collected extensive data on residents' social networks and relationships with local leaders, which I plan to use to further refine the mechanisms studied in the paper. I also collected endline data on how peer relationships have evolved after working together in the campaign, and the randomized assignment of residents to teams offers an opportunity to study whether sustained cooperation creates relationships across ethnic boundaries and whether different leaders facilitate this process. The workshop would be of immense value to gather feedback on how to refine the questions of this agenda, learn how to draw credible conclusions from partially observed networks, and develop future experiments to study how collective action relies on networks.

Can Religion Bridge Ethnic Divides?

How Pastors in DRC Promote Collective Action Where Chiefs Cannot

Gabriel Granato[†] Elie Kabue Ngindu [‡]

September 15, 2026

JOB MARKET PAPER.
PRELIMINARY DRAFT. PLEASE DO NOT CITE OR CIRCULATE.

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Abstract

Ethnic fragmentation is thought to undermine economic development. In one prominent mechanism, greater free-riding in heterogeneous societies erodes the provision of local public goods. This paper examines whether traditional and religious leaders can sustain cooperation in a voluntary public-works program in an ethnically diverse city in DRC. Residents are randomly assigned to ethnically homogeneous or mixed work teams that either include a chief, a religious leader, or no leader at all. Chiefs raise participation by 4.8 percentage points in homogeneous teams and have no detectable effect in mixed ones; pastors raise participation by 6.3 points in mixed teams and have no detectable effect in homogeneous ones, a 9.6-point reversal in their relative advantage ($p = 0.023$). A cross-randomized disclosure intervention helps identify distinct enforcement strategies. Chiefs are effective only when each resident's own contribution is visible to teammates, consistent with social sanctioning. Pastors, by contrast, are effective only when the team record only reports aggregate contributions, consistent with internal incentives governed by a fear of divine oversight. The paper provides the first experimental evidence on when religious leaders can substitute for customary authority in sustaining voluntary public-goods provision in fragmented societies.

1 Introduction

Ethnically diverse societies have slower economic growth and worse public goods than homogeneous ones (Easterly and Levine, 1997; Alesina and La Ferrara, 2005). One important channel, particularly in Africa, is the failure of voluntary provision: local public goods depend on contributions of labor, money, and materials made outside the formal tax system (Olken and Singhal, 2011). Voluntary contributions are lower in diverse communities because they cannot enforce social sanctions to increase the cost of free-riding (Miguel and Gugerty, 2005; Habyarimana et al., 2007). In principle, local leaders could solve this enforcement problem (Ostrom, 1990; Kosfeld and Rustagi, 2015). But the authority of chiefs — the leaders best placed to do so — is also ethnically bounded (Acemoglu et al., 2014; Baldwin, 2016). Who, then, can bridge ethnic divides?

*This study was approved by the University of California, Berkeley Committee for Protection of Human Subjects (2025-05-18574) and was preregistered in the AEA RCT Registry as AEARCTR-0016364.

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Religious leaders are natural candidates. Sub-Saharan Africa is now home to more Christians than any other region of the world, and religious leaders play important social roles — from conflict resolution to job training to service provision (Freeman, ed, 2012; World Christian Database, 2025). Religious leaders also differ from chiefs in two crucial ways. First, their scope of authority extends beyond the ethnic group (McCauley, 2014), and past work has documented a correlation between moralizing religions and cooperation (Henrich et al., 2010). Second, rather than relying on social sanctions, religious leaders can promote internal incentives (fear of divine judgment) that do not require an audience of peers (Shariff and Norenzayan, 2007). Yet research on whether religious leaders can promote cooperation in divided societies is scarce.

Drawing on a voluntary public-works program in Kananga, Democratic Republic of the Congo, this paper presents experimental evidence that religious leaders can sustain collective action beyond the ethnic limits of customary authority. Ethnic fragmentation does not always harm cooperation; it changes who can sustain it.

In Kananga, and in the DRC more broadly, local public-good provision has a concrete institutional form. Public maintenance is organized through *salongo*, a community-labor institution used to repair roads, clear drainage, remove waste, and protect neighborhoods from erosion. Local chiefs coordinate participation in such activities through customary authority and dense social ties (Balán et al., 2022). Yet the Kamuina Nsapu conflict (2016–2019) recently disrupted this equilibrium by displacing 1.4 million people and killing several thousand. The war drove many villagers into Kananga, where their arrival weakened the link between where people lived and their customary networks, potentially making salongo harder for chiefs to organize (Stearns, 2018; Corps, 2019). Amid this instability in customary authority, Kananga's religious institutions remained strikingly stable and vibrant. Churches are widespread neighborhood institutions whose congregations often cut across origins and customary jurisdictions, and where leaders frame community service as a moral obligation governed by divine oversight and judgment (Kapepula et al., 2022; Ngoma Mavungu et al., 2025). This contrast makes Kananga a natural setting to ask whether religious leaders can sustain cooperation where customary authority is weakened by ethnic mixing.

We partnered with the Kananga city administration during an effort to encourage greater citizen participation in salongo. As part of this campaign, the administration introduced a contribution-tracking system and organized teams of voluntary residents to carry out public maintenance over several weeks. We worked with them to implement the program and embedded an experiment within it. The main experimental variation randomly assigned teams to include an urban chief, a religious leader, or no leader. We additionally cross-randomized two features of the environment in which these leaders operated.

The first arm randomized whether teams were composed either of residents from the same ethno-territorial community as the chief or of residents drawn from different origins. We designed this variation to study leaders' *scope of authority* across social groups—whether leaders' influence depends on their ties with residents or on the identities shared by the group. On the one hand, one should expect chiefs to be more effective in ethnically homogeneous groups, where they share ethnic ties with residents, and participants are more likely to recognize

them as ingroup leaders (Bhalotra et al., 2023; Roy and Houser, 2024). On the other hand, the implications of group composition for pastors' influence are less clear. One account emphasizes the broad reach of religion across ethnic groups and expects pastors to influence participants of homogeneous and mixed groups, as long as the individuals share these religious beliefs. Another account follows from how different societies have developed distinct enforcement systems of kin and kith communal sanctions versus internalized moral obligations (Enke, 2019). These systems may coexist within individuals, with social context shaping which identities and related obligations become salient and guide behavior (Akerlof and Kranton, 2000). In this view, the groups' shared ethnic identity leaves less room for a pastor to leverage religious identities.

Our main results reveal that the presence of a chief increases participation by 4.8 percentage points in homogeneous groups ($p = 0.099$) and by 0.7 points ($p = 0.800$) in mixed groups. The presence of a pastor leads to an opposite pattern, where participation rises by 6.3 points ($p = 0.033$) in mixed groups and by 0.8 points ($p = 0.742$) in homogeneous groups. The chiefs' advantage over pastors changes by 9.6 percentage points ($p = 0.023$) once we move from homogeneous to mixed composition. Similar no-leader participation across team types provides little evidence that baseline cooperation explains the result. These results favor the account in which ethnic proximity enables chiefs' authority while at the same time curbing pastors' ability to influence the group. A shared ethnic identity displaces the religion's influence on behavior.

We then present several pieces of evidence that further characterize the boundaries of each leaders' scope. We begin by presenting evidence on how chiefs' reach extends beyond homogeneous teams by leveraging residents' relationships with and ratings of assigned leaders. We show that chiefs also improve participation in mixed teams, but only among those participants who: (i) perceive the chief as being in a position to sanction and reward behavior, (ii) rate the chief more favorably on an opinion index, or (iii) are the chief's co-ethnics. These results strengthen the interpretation that chiefs' effects depend on whether residents' recognize their authority, with co-ethnicity being one path for such recognition. Accordingly, chiefs' influence operates through personal ties; what matters is their relationship with the individual rather than with the group.

Turning to religious leaders, we show that chiefs' co-ethnics positively respond to pastors influence when placed in mixed groups, even if their response in homogeneous groups is zero. This contrast reinforces the account that pastors' fail to reach homogeneous teams because of a social context mismatch, rather than a general failure of pastors among the chief's co-ethnics. In particular, it is consistent with an interpretation where group co-ethnicity precedes a shared religious identity and weakens pastors' influence. We complement this evidence by showing how a higher group dependence on co-ethnic ties weakens pastors' effects, especially in homogeneous teams. Finally, we show that pastors' influence depends on how amenable participants are to recognize religion as an internal guide versus a social activity, concentrating among those who score higher in intrinsic religiosity (Allport and Ross, 1967). Overall, these results provide evidence that pastors' influence operates through an alignment of individual and group conditions; individuals must rely on religion as an internal guide, and the group context must give religious identity a role in organizing cooperation.

The limits of leaders' reach hint at different sources of enforcement but provide insufficient evidence for a stronger conclusion. Do teammates respond because they expect others to observe and judge their behavior, or because they feel an obligation to contribute even when others are not watching? We investigate these questions by leveraging the second cross-randomized arm, where we varied what teammates learned about individual participation.[1] Team meeting sheets disclosed either each member's participation or only the team's aggregate contribution. This variation targeted leaders' *sources of enforcement*, helping to distinguish motives that are *external* to the individual and require peer monitoring to operate, such as fear of reputational losses and social sanctions, from identity motivated *internal* incentives that do not need to rely on an external audience to enforce behavior.

The implications of cross-randomizing public accountability are straightforward when it comes to chiefs. As in previous research, residents seem to be influenced by the chief because they are *external*, social enforcers of behavior (Kosfeld and Rustagi, 2015). Therefore, increased public accountability should strengthen chiefs' capacity to enforce behavior, as it becomes easier for them to target shirkers. Conversely, how public accountability influences the pastor effect is unclear. On one account, costly behavior serves as a social signal of religious commitment that enables future cooperation (Iannaccone, 1992; Levy and Razin, 2012). If pastors' influence operates through this social account of religion, one would expect public accountability to strengthen pastors' impact as well. On another channel, religious leaders operate through internalized moral obligations related to fear of divine judgment or other internal religious motivations (Shariff and Norenzayan, 2007). Such an *internal* enforcement account of pastors' influence would predict a null or even negative influence of disclosure on pastors' effect. Public accountability may shift the attention of residents from their internal reasons to participate towards social image concerns that are less relevant in ethnically fragmented teams.

We find that chiefs' positive impacts in homogeneous teams are concentrated in groups where we made individual participation visible — when the group falls within their authority *and* they can judge individual behavior — corroborating with an external source of enforcement interpretation. Under those conditions, chiefs raise participation by 10.0 percentage points ($p = 0.006$). Without the disclosure of individual participation, chiefs have no statistically significant effect, even in homogeneous teams, and they remain ineffective in mixed teams regardless of visibility. Pastors produce a distinct pattern. In mixed teams, their presence increases contributions only when individual participation has *not* been disclosed (9.2 percentage points, $p = 0.021$). In mixed teams where individual behavior is made public, their effect falls toward zero, contrary to what a peer-enforcement interpretation would predict. This lack of a positive change is instead consistent with pastors working through a universal language that triggers religious internal self-evaluation, a different source of enforcement. After being exposed to the pastor's discourse, residents may begin to treat participation as evidence that they have honored a moral obligation before God.

[1] Although residents were part of small teams, they could contribute at different times and work sites, leaving teammates with incomplete knowledge of one another's efforts.

After ruling out simpler accounts of the effects, including the possibility that leaders exerted different follow-up efforts with different teams of participants, we examine two complementary pieces of evidence for this account. First, we study the different topics that teams discuss during meetings. A treatment-blind text classification model groups speeches into three different categories: calls for attendance and better group performance, requests for material support, and appeals linking divine oversight to solidarity and community work. Unprompted leaders speak substantially more than ordinary participants, and pastors speak more than chiefs. Chiefs emphasize attendance and request material support most of the time, while pastors focus on speeches associating divine rewards with solidarity and community work. We find that their style is stable throughout experimental conditions, and what differs is how much they influence the discussion. Leaders shape the agenda of the meeting, but only in contexts where leaders would later sustain higher participation.

We then complement the meeting evidence by studying a behavioral implication of the different sources of enforcement. If chiefs raise the reputational cost of shirking, their effects should be strongest among prior *nonparticipants* when disclosure exposes their lack of contribution. Conversely, if pastors work through internal accountability before God, there is no reason to expect that their effects will vary with prior participation, and their influence may extend to already engaged residents. The data align with these predictions. Disclosure allows chiefs to draw prior nonparticipants into salongo, while pastors without disclosure generate similar effects across prior participation types. The decomposition also helps explain why pastors' effects disappear under disclosure. Without disclosure, pastors induce already engaged residents to contribute more; once individual behavior becomes public, disclosure itself raises participation among these residents, and pastors add little beyond the effect of disclosure itself. The aggregate null under disclosure masks substitution between the two sources of enforcement.

Lastly, we turn to whether sustained collective action may have reshaped social and political attitudes. We investigate three sets of outcomes: identity and general beliefs about cooperation and accountability, opinions about group members, and views about government and other local institutions. Additional participation induced by leaders produces no common shift in beliefs about social or divine accountability, the salience of religious and social identities, or views of the state and other authorities. The clearest effects arise from how sustained cooperation reshapes attitudes towards others in the group. Residents become more willing to help one another, while their broader assessments of peers remain unchanged. Pastor influence over sustained cooperation also improves the pastor's own standing, whereas chief influence does not. The asymmetry is consistent with their initial positions since chiefs are already established in homogeneous groups, while pastors enter mixed groups without the same preexisting social standing and earn it after successfully sustaining cooperation. The evolution of these outcomes sheds light on how religion may help build informal insurance networks in contexts where the state fails to provide social security (Townsend, 1994; Berman, 2000; Chen, 2010; Auriol et al., 2020). It also helps explain how religious leaders build broader social and political influence by sustaining cooperation (Chaney, 2013; Bazzi et al., 2020).

As far as we are aware, this is the first field experiment to show that ethnic fragmentation changes which local leaders can sustain collective action. Chiefs are effective when cooperation is organized within ethnic communities and individual participation is publicly visible. Pastors are effective when cooperation crosses ethnic boundaries and individual participation is less visible. Without a leader, participation is similar across these conditions. Fragmentation, therefore, does not necessarily harm cooperation. It changes which forms of authority can sustain it.

1.1 Related literature

This finding contributes to a large literature documenting lower growth, civic participation, and public-good provision in more fragmented settings (Easterly and Levine, 1997; Alesina et al., 1999; Alesina and La Ferrara, 2000, 2005; Miguel and Gugerty, 2005; Habyarimana et al., 2007; Baldwin and Huber, 2010; Glennerster et al., 2013)[2], and to research on governing the commons, which identifies shared rules, local monitoring, and informal sanctions as foundations of cooperation outside formal enforcement (Olson, 1965; Ostrom, 1990, 2000). A central lesson from this literature is that fragmentation disrupts the social ties and enforcement norms that prevent free-riding in voluntary provision, making it harder for groups to achieve collective goals. We build upon this research and ask which institutions can sustain collective action when ethnic fragmentation complicates social enforcement. In doing so, we complement research showing that cross-cutting identities can reduce ethnic divisions by shifting the social boundaries around which behavior is organized (Miguel, 2004; Posner, 2004; Clingingsmith et al., 2009; Dunning and Harrison, 2010; Sambanis and Shayo, 2013; Blouin and Mukand, 2019; Bazzi et al., 2019; Depetris-Chauvin et al., 2020)[3].

Second, the paper studies how two forms of accountability can sustain cooperation. Social-image models make behavior responsive to how others judge it (Bénabou and Tirole, 2006; Andreoni and Bernheim, 2009; DellaVigna et al., 2012; Schram and Charness, 2015; Bursztyn and Jensen, 2017; Breza et al., 2026), while models of identity and self-image make behavior responsive to what one's actions reveal about oneself (Akerlof and Kranton, 2000; Bénabou and Tirole, 2003, 2006, 2011; Dal Bó and Terviö, 2013; Dal Bó and Dal Bó, 2014; Grossman, 2015; Bénabou and Tirole, 2016). Indeed, traditional authority is understood to enforce behavior through external sanctions (Kosfeld and Rustagi, 2015), while religious institutions both embed believers in communities that socially enforce prosocial conduct and invoke divine monitoring of private actions (Iannaccone, 1992; Berman, 2000; Auriol et al., 2020; Johnson, 2005; Shariff and Norenzayan, 2007; Norenzayan, 2013; Purzycki et al., 2016; Lang et al., 2019; Lowes et al., 2026a). In particular, field experiments show that religious authority and messages affect behavior through religious beliefs and social norms (Bryan et al., 2021; Buccione, 2023), but they do not experimentally separate these channels. We do so by cross-randomizing

[2]Beyond public-good provision, another strand of research studies how ethnic and religious divisions shape discrimination and productivity within firms and organizations (Hjort, 2014; Marx et al., 2021; Ghosh, 2025).

[3]A related literature tests whether intergroup contact can weaken social divisions (Scacco and Warren, 2018; Rao, 2019; Mousa, 2020; Lowe, 2021; Ghosh et al., 2026). Findings in this literature emphasize that the effects of intergroup contact depend on how interaction is structured and often do not extend much beyond those directly encountered (Lowe, 2025).

religious and traditional leadership with the disclosure of individual participation. Our analysis shows that while chiefs' influence is consistent with a social enforcement account, as it is complemented by public accountability, pastors' effectiveness actually decreases with participation disclosure, establishing a pattern that aligns with previous results where external control displaces internal motivation (Fehr and Rockenbach, 2003; Bénabou and Tirole, 2006; Falk and Kosfeld, 2006; Gneezy et al., 2012; Savary and Goldsmith, 2020).

Lastly, we contribute to research on leadership and informal governance. Existing work studies how leaders coordinate, communicate, set examples, sanction free-riding, and shape expectations (Hermalin, 1998, 2012; Caillaud and Tirole, 2007; Dewan and Myatt, 2008; Bolton et al., 2013; Jack and Recalde, 2015; Kosfeld and Rustagi, 2015; Antonakis et al., 2022; Bhalotra et al., 2023; Boudreau et al., 2025). Related work examines who becomes a leader and how leader identity shapes political outcomes (Besley, 2005; Caselli and Morelli, 2004; Jones and Olken, 2005; Dal Bó et al., 2017), while research on informal governance studies how traditional authorities contribute to public-good provision and state capacity (Acemoglu et al., 2014; Baldwin, 2016; Baldwin et al., 2025; Balán et al., 2022; Henn and Tanutama, 2023; Lowes et al., 2026b). Closely related to our contribution, social identity theories of leadership argue that leaders are more effective when they are perceived as members of the group; yet, accounts are largely correlational or constrained to lab settings (Hogg, 2001; Van Knippenberg and Hogg, 2003; Steffens et al., 2021; Roy and Houser, 2024). We study a field experiment in which we hold the public-good activity and current neighborhood fixed and vary whether participants share the chief's territory of origin. This identifies the reach of authority across social boundaries within a fixed neighborhood, which we call the leader's *scope of authority*. Observed participation reveals that chiefs mobilize within their ethno-territorial constituency, while pastors carry authority across territorial-origin boundaries.

We begin in Kananga, where pastors and chiefs meet in the everyday provision of public goods.

2 Context

The Democratic Republic of Congo is one of the world's most populous countries and also one of the poorest. Kananga, the capital of Kasaï-Central Province and the setting for this study, is a city of roughly one to two million inhabitants. It is governed in a low-capacity fiscal environment. Roads are degraded, constant tropical storms cause severe erosion problems, and access to basic urban services such as electricity, piped water, and sanitation is low. The local administration is responsible for many of these services, but the lack of institutional capacity prevents it from doing so. The setting is one of weak fiscal capacity, underprovided public goods, and a heavy reliance on local authorities to mobilize residents directly (Balán et al., 2022).

Salongo, City Governance and Traditional Authority. The principal institution through which this direct mobilization occurs is *salongo*. Under Mobutu, salongo was formalized as obligatory civic labor and became part of a broader effort to mobilize local labor for public works (Callaghy, 1984; Robinson,

2023). Recent evidence shows that households are still regularly asked to contribute salongo to neighborhood upkeeping projects throughout the country, with participation organized by local authorities and sustained through social pressure, civic obligation, and repeated local interaction (van den Boogaard et al., 2026). In Kananga, salongo remains part of everyday life, and many times it is the only solution residents have for waste management, road maintenance, public space cleaning, and, most urgently, anti-erosion works.[4]

Salongo operates through a local governance hierarchy. In statutory terms, the relevant urban unit is the *quartier*. The city is divided into communes, communes into quartiers, and quartiers into avenues and streets. The *chef de quartier* is the formal neighborhood authority responsible for administrative supervision of the population, hygiene, and public sanitation. In practice, state reach relies on two lower layers of authority. The first is the *point-focaux*, or *chef de localité*, which effectively serves as a focal point for the neighborhood chief.[5] *Point-focaux* transmit instructions from the quartier administration, help identify feasible work sites, and organize the maintenance efforts across several avenue chiefs. They are implementation intermediaries. The second is the *chef d'avenue*, or avenue-level notable, who is the household-facing authority. He is closer to residents than the *chef de quartier* and the *point-focal*.

The traditional-leader treatment focuses on the *chef d'avenue*, the level of chief authority that is closest to the resident population. Two features of their status matter for the experiment. The first is their ethno-territorial scope. The chief is better understood as a respected resident whose authority is anchored in a local ethno-territorial social order, so it should be strongest over residents who recognize him as speaking from within their own origin community. The second is their social source of enforcement. Given chiefs' standing and embeddedness in the local context, we expect their mobilization capacity to operate through reputational channels.[6] The treatment tests whether chiefly authority can make salongo socially binding, and whether its force declines when the mobilized group falls outside their ethnic scope.

Fragmentation. An initial assessment of Kananga's ethnic landscape may wrongly infer a homogeneous environment. The city is largely Tshiluba-speaking, and most residents are linked to the Lulua umbrella identity. However, this broad classification contains important internal divisions. It consolidated from smaller kin, territorial, and chiefly units in the Kasai during the colonial period. Belgian recognition of the Kalamba lineage as Lulua sovereign led to power struggles between central and peripheral clans that remain unresolved

[4]The relevance of these tasks is visible in the World Bank's Kananga Emergency Urban Resilience Project (World Bank, 2022), which focuses on gully erosion, drainage, and climate-resilient urban planning. Reports emphasize that the constraint is partly organizational since neighborhood erosion-prevention committees have been established across Kananga; yet, community participation remains below target.

[5]The *localité* appeared as a lower subdivision under the 1982 territorial organization (République du Zaïre, 1982), but is absent from the current 2008–2010 framework, which organizes urban communes into quartiers and/or incorporated groupements and lists territoire, quartier, groupement, and village as deconcentrated entities (République Démocratique du Congo, 2008, 2010). In Kananga, the category remains a relevant de facto administrative scale.

[6]Balán et al. (2022) characterize Kananga city chiefs as local notables nominated by neighborhood elders, typically because they are longstanding and respected residents, and then ratified by the state. They report indefinite and often lifelong tenure, rare deposition, some family transmission of the position, and status as its main return. They also show that chiefs possess household-level information and that this information helps explain their effects on tax compliance.

(Ngalamulume, 1995; Stearns, 2018). Because this umbrella identity would flatten most residents into a single group, we define ethno-territorial identity using the five administrative territories of Kasaï-Central (Demba, Dibaya, Dimbelenge, Kazumba, and Luiza), together with Kananga city[7]. The formal administrative units help identify variations in local identity, kin networks, and settlement histories. Given the informal nature of salongo mobilization by local chiefs, whose authority depends on the density of local networks, variations in neighborhood territorial fragmentation should help explain why salongo may have weak support in certain places while succeeding in others.

Descriptive evidence collected in this study corroborates the hypothesis that fragmentation complicates local governance around salongo activities. Using participants' stated territories of origin, we build a fractionalization index for each neighborhood and investigate how it relates to the perceived importance of salongo among local residents. Figure A-1 reveals that fragmentation weakens perceived salongo importance in the neighborhood through two margins: (i) residents from the majority territory in a given neighborhood see salongo as less important the more fragmented the neighborhood is; and (ii) residents from less represented territories see salongo as less important regardless of the neighborhood's fragmentation levels. A key part of our account is that this governance problem is tied to the territorial bounds of *the chef d'avenue's* authority. If traditional leaders indeed rely on an ethno-territorial scope of authority, their standing should be stronger among residents who share these territorial references and weaker among residents from different origins.

Conflict. Beyond historical fragmentation related to the consolidation of several groups under the broader Lulua identity, the Kamuina Nsapu conflict made these internal divisions especially salient. Beginning in 2016 as a dispute over customary authority in Dibaya, the conflict became a regional war after the state's refusal to recognize Jean-Prince Pandi as the successor to the Kamuina Nsapu chiefship. Stearns (2018) interprets this episode as an instance of *dédoublement*, a broader strategy through which Congolese authorities weaken customary leaders viewed as sympathetic to the opposition by withholding recognition and supporting rival claimants, while locating this occurrence within the historical divisions of the Lulua umbrella ethnicity.[8] The war not only hardened those divisions due to clashes occurring in rural areas but also created chaos in the city of Kananga, which became a stage for hostilities. Several killings took place in the city, while schools, health centers, homes, and public buildings were destroyed. It also became a new home for hundreds of thousands of displaced villagers who were unable or unwilling to return to their villages of origin.[9] Therefore, beyond the destruction and tensions left by the war and all the deaths caused by it, Kananga recently absorbed a

[7]Appendix Table A-1 locates the leaders' origins and religious denominations within the population from which experimental participants were recruited. Chiefs and pastors come from broadly the same origin territories as ordinary residents.

[8]The Kamuina Nsapu lineage heads the Bajila Kasanga, the dominant group within the peripheral Lulua clans, which has long contested the Kalamba lineage and the political center around Kananga.

[9]Mercy Corps reports more than 3,300 direct conflict-related deaths and 1.4 million displaced people in 2017; by early 2019, several hundred thousand displaced residents had still not returned because of livelihood collapse, insecurity, and ethnic tensions. The same report notes that more than 400 schools and 200 health centers, along with many government buildings, were damaged or destroyed (Corps, 2019).

violent re-sorting of people from different territories and customary histories (Corps, 2019). Residents who now live on the same street and share a communal task to maintain a drainage channel or to address erosion risk are now more likely to have arrived from different places of origin. The experiment focuses on the resulting mismatch between the chief's administrative neighborhood and the social networks through which his authority becomes relevant.

Religious Environment and Religious Authority. The DRC is overwhelmingly Christian, comprising 95% of the population, and is roughly divided between Catholics and broadly defined Protestants (Pew Research Center, 2011). The religious environment in Kananga roughly corresponds to these numbers, with the caveat that a broad protestant classification hides the rapidly shifting religious landscape where autonomous *Églises du Réveil* are taking over the space previously filled by Catholics and traditional Protestant churches (Kapepula et al., 2022). The gap left by a weak state creates a wide space for churches to mobilize welfare and collective goods. Institutional churches, Catholic and historic Protestant networks, occupy the organizationally dense pole of this field through schools, clinics, development projects, and social assistance.

Entrant denominations perform a more decentralized and entrepreneurial role. They provide support through pastoral counseling, healing, deliverance, mutual aid, and informal insurance against illness, unemployment, and other risks. This decentralized model relies on local mobilization with few large bureaucratic assets.[10] Appendix Table A-1 locates leaders' religious denominations within the population from which experimental participants were recruited. Religious leaders are concentrated in Pentecostal churches, and the experiment also includes pastors from Neo-Apostolic and Kimbanguist denominations, while chiefs and residents are distributed more broadly across the Christian spectrum. The selection is informative in itself. Pastors whose authority depends on building a local following are exactly those most engaged in local mobilization. Catholic priests and other mission churches, by contrast, derive authority from their office within a larger hierarchy and need not build it through the same local involvement.[11]

Importantly, churches are spaces of ethnic mixing, where individuals with different kin networks meet and build a life unrelated to their origins. Baseline data show that more than 90 percent of identifiable congregations include members from multiple origin territories (Appendix Table A-2). Congregants are no more likely to share an origin territory within their churches than would be expected under random allocation within their neighborhood. Within-church pairs are co-territorial in 48.2 percent of cases, compared with 48.8 percent under the no-sorting benchmark. Hence, religious leaders are already leading organizational constituencies that cross the territorial boundaries on which chiefly authority rests.

Therefore, the religious-leader treatment arm focuses on pastors from autonomous churches in a context where religion is an important institution for ethnic mixing. These leaders provide a contrast to avenue

[10] On institutional church provision in Congo, especially health and education under weak state capacity, see Seay (2013); Titeca et al. (2013). On healing churches and everyday social support in urban Congo, see Devisch (1996).

[11] Recruitment was open to leaders from all denominations.

chiefs on the two dimensions central to the design. The first is cross-territorial scope. Pastors address residents through a portable religious frame that cuts across the ethno-territorial identity to which traditional leaders are confined. [12] The second is a different source of enforcement. Pastors can frame participation as conduct before God rather than performance before their neighbors and local authorities. In Kananga's revival churches, concerns with witchcraft, protection, prosperity, and a worldview of hidden supernatural causality are translated into a Christian vocabulary of sin, blessing, and divine judgment.[13] The treatment asks whether this form of religious authority can sustain cooperation where the chief's ethno-territorial audience is weak.

3 Research Design

In this context of post-war recovery, where neighborhoods have recently integrated a massive wave of displaced residents, Kananga's city administration organized a city-led *salongo* campaign to coordinate basic neighborhood maintenance and incentivize citizens to join community activities. The campaign brought residents together to contribute labor and materials to neighborhood upkeeping tasks and used city evaluation and public recognition to reward performance, a common practice in the region.[14] We partnered with the administration in these efforts and embedded an experiment in the campaign.

Neighborhood selection. Before rollout, we identified 50 neighborhoods with active *salongo*, identifiable boundaries, and sufficient scale for team implementation. Fifty neighborhoods met these criteria. We stratified them by commune and by whether the median predicted property value lay above or below the sample median, then randomly assigned one neighborhood in each stratum not to receive the intervention. The remaining 40 neighborhoods entered the intervention rollout. The main estimands compare randomized teams within these intervention neighborhoods.

Recruitment and short baseline. Before each wave, enumerators visited households in participating neighborhoods to introduce the campaign and register adults interested in taking part. This short baseline recorded residents' demographic characteristics, territorial origins, and willingness to contribute, and asked them to refer traditional and religious leaders who were especially committed to making *salongo* succeed. The resulting data defined the resident sampling frame, identified candidate leaders, and supplied the variables

[12] On religious identity as an axis of mobilization that differs from ethno-linguistic identity in Africa, see McCauley (2014). On Pentecostal and charismatic Christianity as a portable religious form, see Robbins (2004). Autonomous revival pastors are especially useful for the argument because their authority is less anchored in inherited institutional infrastructure and more directly tied to local mobilizational capacity.

[13] On Congolese revival Christianity, deliverance, moral diagnosis, and the translation of occult and social anxieties into Christian idioms, see Pype (2012); Tonda (2005).

[14] Comparable civic-work and cleanliness contests elsewhere in the region include Kinshasa's Kin Bopeto, which promised prizes to the cleanest quartier in each commune (Actualite.cd, 2019); Rwanda's Umuganda, which links regular community work to infrastructure, environmental protection, and cleanliness (Rwanda Governance Board, 2017); Lomé's "salubrité challenge des quartiers," which had quartiers submit streets for jury evaluation (Vert Togo, 2019); and Cameroon's "Villes propres" competition, which began in 2019 as a Yaoundé "Quartiers propres" contest (Ministère de l'Habitat et du Développement Urbain, 2022).

used in randomization. The study was planned as ten waves of four neighborhoods. Seven waves were completed under the standard protocol before the Ebola emergency disrupted the eight wave and prevented the remaining rollout (World Health Organization, 2026). Hence, the main analysis covers 28 neighborhoods, 336 teams, and 1,040 non-repeated residents. Appendix B documents the stopping decision and reports robustness estimates incorporating the disrupted eighth wave.

3.1 Salongo Campaign

During each implementation wave, local officials organized daily maintenance activities and recorded residents' contributions for five weeks. In parallel, in the first week of the campaign, we invited volunteers to participate in experimentally composed coordination teams that would help advance salongo in the neighborhood. Each team met twice during the campaign in coordination meetings held separately from the daily work: first in the second week of activities and again in the third week. The meetings gave residents the opportunity to review their team's recent contributions, coordinate future work, and encourage each other to participate.

At the first meeting, participants got to know their assigned teammates, received the information specified by their disclosure condition, and learned that the city would publicly recognize highly engaged teams at the end of the campaign. Because this was the first moment participants encountered their team and disclosure condition, the first meeting marked the onset of the randomized treatments. By the end of the fourth week of activities, the three teams with the highest engagement received public recognition from the city in a neighborhood ceremony attended by city officials. A fifth week of activities was organized after the recognition ceremony. All teams faced the same maintenance activities and recognition rules. The upper panel of Figure 1 summarizes the timeline of activities.

3.2 Randomization

The assignment proceeded in two stages. First, a screening stage selected participants from the registration sampling frame, oversampling those whose territorial origin matched that of the neighborhood's traditional leader to make the construction of homogeneous teams possible. Second, an assignment stage organized the selected residents into twelve teams of four in each neighborhood, composing a factorial design that crossed three leader conditions, two territorial-origin compositions, and two disclosure conditions. The design varies these conditions while keeping the neighborhood, campaign, team task, and recognition rules fixed. Figure 2 summarizes these assignments. Appendix C presents the complete algorithm.

3.2.1 Leader Treatment

Each team counted on a fourth participant who would define the leader condition. Enumerators used the recruitment stage leader nominations to identify leaders who were available to join the campaign. They were instructed to select the leader with the most mentions who was willing to participate. In the traditional

leader condition (T), the same nominated traditional leader joined four teams in the neighborhood: two homogeneous and two mixed, split across disclosure conditions. The nominated religious leader did the same in the religious leader condition (R). In the no-leader condition (C), a placebo participant joined the team. [15] We keep all non-repeated residents in the analysis sample while we exclude repeated leaders and placebo participants.[16] All fourth participants followed the same meeting protocol. Leaders received no script or special instructions.

Traditional leaders. The traditional leaders in the study are chiefs embedded in Kananga's customary system of territorial governance. Residents identified leaders in their neighborhood whom they regarded as especially committed to making *salongo* succeed, and these referrals formed the recruitment pool. Like the *localité* chiefs studied by Balán et al. (2022), they are established local notables whose responsibilities include organizing communal work, mediating disputes, representing residents, and interacting with the state. Appendix Table A-3 shows that, relative to ordinary residents of the same neighborhood, the chiefs recruited into the study are twenty-two years older, have about 1.5 additional years of schooling, and have lived on their avenue for roughly thirteen years longer; they are also overwhelmingly male. Their institutional position is visible given the higher chances of holding a traditional head role, knowing local authorities, planning *salongo* activities, and evaluating higher chiefs' contributions to infrastructure, mediation, and representation favorably. Their personal networks are disproportionately composed of people from the same territory, and they score higher than ordinary residents on an aggregate index of standard leadership traits, particularly conscientiousness. Appendix Table A-4 uses surveys administered before the first meeting to show that chiefs are known and recognized as authorities across ethnic lines, but their broader standing is concentrated among co-ethnics. Residents who share a chief's ethnic origin rate the chief as more motivating, morally exemplary, trustworthy, and hardworking, whereas residents from other origins recognize the chief's formal authority but do not evaluate the chief more favorably on these other dimensions. Coethnic and non-coethnic residents, nevertheless, expect chiefs to participate more in *salongo*, with especially strong expectations of their work effort. Therefore, chiefs enter the experiment as incumbent organizers of salongo whose authority, relationships, and reputations are centered on their ethnic origin.

Religious leaders. The religious leaders are local pastors drawn from Kananga's expanding landscape of Pentecostal, charismatic, and independent churches, institutions whose activities frequently extend beyond worship into community organization and public life (McClendon and Riedl, 2015, 2019; Kapepula et al.,

[15] Beginning in Wave 3, the same randomly selected placebo participant joined all four no-leader teams, making team size and repeated participation symmetric across the three leader conditions. In Waves 1–2, each no-leader team contained a different fourth ordinary resident.

[16] The fixed placebo participant was selected from the traditional leader's territory. Religious leaders were recruited through religious authority and did not always share the traditional leader's territorial origin. Eleven of the 28 assigned religious leaders differed from the traditional leader. We report a robustness check restricted to the 17 neighborhoods in which the two leaders shared an origin.

2022; Ngoma Mavungu et al., 2025). They were recruited through the same resident-referral procedure and come from a demographic base similar to that of chiefs. Appendix Table A-3 shows that, relative to ordinary residents, pastors are thirteen years older, have approximately 1.5 additional years of education, and have lived on their avenue for roughly thirteen years longer; they are even less likely to be female than chiefs. Their distinctive position lies instead in congregational life. Religious leaders are much more likely to head a congregation, attend Sunday services frequently, contribute to their church, and depend on networks centered on fellow church members. They are similar to ordinary residents on standard leadership-personality measures but report more positive and less negative religious coping, with weaker evidence of greater intrinsic religiosity. The institutional boundary is permeable: roughly 40 percent of pastors report holding a traditional role, and 29 percent of chiefs report a religious role. Yet the two groups remain differently situated, with chiefs more firmly embedded in customary government and pastors in congregational organization. Appendix Table A-4 shows that the distinction is reflected in how residents evaluate them. Pastors are known across ethnic lines and are rated as more motivating, morally exemplary, trustworthy, and hardworking, even by residents of a different ethnic origin, with similarly favorable assessments among co-ethnics. Residents also expect pastors to participate in *salongo* and contribute financially, although expectations of their work effort are weaker than for chiefs. Pastors, thus, enter the experiment with less incumbent responsibility for *salongo* but with a favorable standing that extends across the territorial boundaries organizing customary authority.

3.2.2 Cross-randomizations

Team composition. We filled the twelve teams in random order. Each team was first assigned three ordinary residents who participated in only one team. For the six homogeneous teams, all three residents were drawn from the pool that shared the traditional leader's territorial origin. For the six mixed teams, we assigned residents from territories not yet represented in the team, repeating an origin only when no additional territory was available. This procedure ensured that the three ordinary residents in mixed teams came from different territories of origin. Within each leader condition, two teams were homogeneous and two were mixed.

Disclosure. Residents could contribute at different times and work sites; therefore, they could not fully observe their teammates' contributions. Leveraging the incomplete information setup, we varied the information shown at the meeting. Under group disclosure, participants saw only their team's aggregate prior *salongo* contribution (private condition). Under individual disclosure, they also saw each member's identifiable contribution record, increasing external reputational incentives (public condition). Within every leader-by-composition cell, one team was assigned to each disclosure condition.

3.2.3 Rerandomization and Replacements

For each wave, the assignment program generated 48 complete candidate assignments and retained the one that minimized a prespecified imbalance score across 15 pre-assignment characteristics in the cumulative implemented sample, following Banerjee et al. (2020). During the invitation period, some selected residents reported that they would not be able to attend their meeting slot. These residents were replaced before treatment using randomly ordered replacement lists of respondents from the same territory of origin. Replacements inherited the original resident's team and experimental conditions, and all availability decisions occurred before residents met their teammates or the disclosure condition.

3.3 Data

Beyond the short survey used to construct the experimental assignments, the analysis combines administrative records, survey data collected before and after treatment, and recordings of the team meetings. The bottom of Figure 1 shows when each data collection instrument was administered.

Administrative data. The administrative records contain individual daily measures of labor, in-kind contributions such as food or materials, and monetary contributions. Local staff recorded contributions using a digital tracking system created for the campaign. The registry contains the neighborhood, date, and activity site, along with the geolocation of each salongo location; it classifies the type of maintenance activity; and collects photographs of the sites before and after work. Arrival and departure time stamps measured contributions of labor for each participant, along with participant ratings of effort. In-kind and monetary contributions were recorded with quantities and estimated values. The records were merged with registered participants and dates, producing high-frequency, individual-level data on actual contributions to neighborhood maintenance.

Survey data. We collected survey data at baseline, two midlines, and endline. The baseline survey was administered before meeting invitations and measured participants' demographic and socioeconomic background, territorial origin and migration history, religious practices and beliefs, local social embeddedness, perceived salongo norms, trust in local authorities, and expectations of social and divine reciprocity. The first midline was conducted around the first team meeting. Right before the team meeting, it measured participants' proximity to each other, shared attributes such as church affiliation or kinship. Immediately after the meeting, it measured meeting impressions and immediate expectations of future peer participation. The second midline took place around the follow-up meeting and measured interactions during the inter-meeting period, including communication among team members, encouragement to contribute, praise for contributors, and sanctioning of free-riders. The endline questionnaire was administered on the week after the recognition ceremony and repeated the main baseline modules.

Meeting recordings. We recorded team meeting discussions with individual consent and transcribed the content to conduct a textual analysis of the leader's mobilization strategy. Because leaders received no script, these recordings show what they endogenously contributed to the discussion. We specifically investigate whether leaders activated the authority channels specified in the design.

3.4 Implementation Checks

Appendix Table A-5 separates two kinds of balance. The characteristics measured before assignment are similar across experimental arms. Participation during the six days after assignment and before the first meeting is imbalanced. At that point, residents had not learned their assigned leader or reporting rule, so the difference cannot reflect a response to treatment. We nevertheless examine whether it can account for the results in the alternative-explanations subsection. The remaining implementation checks are reassuring. Although 22.2 percent of residents entered through replacement queues, nearly 95 percent of groups still had the territorial composition assigned by the design. Homogeneous groups remained overwhelmingly composed of residents from the chief's territory, while mixed groups drew residents from several territories (Appendix Table A-6). Treatment also reached the intended groups. Residents attended 95.0 percent of first meetings, assigned leaders attended 99.6 percent of treated meetings, and delivery of the meeting instructions and reporting rules was verified in at least 99.4 percent of groups (Appendix Table A-7). Finally, administrative participation is observed for every resident over the same six-day outcome window, and survey follow-up remains high (Appendix Table A-8). Group formation, meeting delivery, and measurement of the main outcome therefore proceeded as planned.

4 Scope of Authority Results

Traditional and religious leaders are notably different from each other and from ordinary citizens, but how do the different profiles translate into the capacity to lead different groups toward a collective goal? We call a leader's scope of authority the range of people and social settings in which the leader can influence behavior toward such a goal. A leaders' scope may depend both on whether individuals recognize the leader's authority and on whether the relationships among group members give relevance to the obligations that authority invokes. Group composition can change both.

For chiefs, sharing the same origin provides one channel for individual recognition of authority, as residents who share the chief's ethnic origin should be more responsive to an ingroup leader (Bhalotra et al., 2023; Roy and Houser, 2024). This account predicts greater effectiveness of chiefs in homogeneous groups, where all participants share the chief's origin, but it does not imply that chiefs lose their influence over coethnics when they are part of mixed groups. Hence, chiefs' scope should follow their relationships with particular residents, even when ethnic identity is not shared by the group as a whole.

For pastors, an open question is whether religious motives can broadly influence believers across and within ethnic divides, or whether the social setting can interfere with how much individuals respond to religious leaders' appeals. If belief in divine judgment is sufficient for religious leaders' speeches to influence behavior, pastors may be effective among believers regardless of group composition. An alternative account builds on the distinction between systems of enforcement that operate through kin and community sanctions versus internalized moral obligations (Enke, 2019). These systems may coexist within individuals, with social context shaping which identities and associated obligations become salient and guide behavior (Akerlof and Kranton, 2000). In homogeneous groups, shared ethnic identity may give customary obligations precedence in organizing collective action. In mixed groups, where ethnicity provides no common basis across all participants, it is when religion becomes the minimum common identity denominator that pastors can leverage as an alternative basis for encouraging cooperation. Under this interpretation, even residents who recognize religious authority may respond differently to a pastor depending on whom they are asked to cooperate with.

We next use our design to examine how group composition shapes each leader's scope of authority.

4.1 Empirical Strategy

Within each neighborhood, we compare groups assigned a traditional or religious leader with groups assigned no leader. Let i index participants, $g(i)$ their accountability group, and $n(i)$ their neighborhood. Variables $T_{g(i)}$ and $R_{g(i)} \in 0, 1$ indicate whether participant i's group was assigned a traditional or religious leader, respectively; no leader is the omitted category. Group composition is $c(i) \in H, M$, where H denotes groups with shared territorial origins and M denotes groups with participants from mixed origins. Finally, $q(i) \in 0, 1$ indicates whether individual participation was disclosed.

The estimation sample contains 1,040 participants in 336 groups across 28 neighborhoods.[17] The outcome Y_i is average *salongo* participation, measured in percentage points, over the six-day administrative window immediately following the first accountability meeting. All specifications control for baseline participation, Y_{i0}, defined as average participation over the six-day window preceding any accountability meeting.[18] For the scope analysis, let $e(i) = c(i)$ and $\mathcal{E} = \{H, M\}$, pooling leader effects across disclosure conditions. We estimate:

$$Y_i = \lambda_{n(i),q(i),c(i)} + \rho Y_{i0} + \sum_{e \in \mathcal{E}} \left(\tau_e^T T_{g(i)} + \tau_e^R R_{g(i)}\right) \mathbf{1}\{e(i) = e\} + X_i'\beta + \varepsilon_i. \tag{1}$$

The vector X_i contains the additional controls used in the robustness specifications. The neighborhood-by-disclosure-by-composition fixed effects $\lambda_{n(i),q(i),c(i)}$ ensure that each leader group is compared with no-leader groups in the same neighborhood and experimental environment. Hence, for each environment e, τ_e^T, and τ_e^R

[17] The sample includes the three ordinary residents assigned to each group and 32 additional ordinary residents who filled the fourth position in no-leader groups during the first two waves. Repeated fourth participants are excluded.

[18] We use a common pre-meeting window to ensure that the baseline measure is not affected by potential spillovers from earlier meetings. As robustness, we also present specifications that control for potential spillover exposure, the construction of which is detailed in Appendix Section D.

are the intention-to-treat effects of assignment to a group with a traditional or religious leader, respectively, relative to no leader under the same conditions.

Within each composition, $\tau_c^T - \tau_c^R$ measures the advantage of traditional over religious leadership. We define the composition gradient, Δ_C, as the change in this relative advantage when moving from mixed to homogeneous groups:

$$\Delta_C = \left(\tau_H^T - \tau_H^R\right) - \left(\tau_M^T - \tau_M^R\right).$$

Positive values of Δ_C indicate that traditional leadership becomes more effective relative to religious leadership in homogeneous groups, or equivalently that religious leadership gains relative effectiveness as groups become mixed. This is our main reduced-form measure of differences in leaders' scope of authority. In the main text, we present the basic specification with the baseline control and fixed effects. Appendix Table A-9 first adds the spillover-exposure control and then fixed indicators for gender and estimated property value. The leader effects and main contrasts are stable across specifications. Standard errors in the main specification are clustered by accountability group, and the main comparisons are robust to alternative clustering.

4.2 Main Results

Figure 3 shows that leader effectiveness depends on the territorial composition of the group. Assigning an urban chief to a group of participants who share the leader's territorial origin increases participation by 4.8 percentage points (SE 2.9; $p = 0.099$), an 8.5 percent increase over the 56.8 percent no-leader mean. In contrast, the chief's effect in mixed groups is 0.7 points (SE 2.9; $p = 0.800$). Pastors display the opposite pattern. The presence of a religious leader increases participation by 6.3 points in mixed groups (SE 2.9; $p = 0.033$), or 10.8 percent relative to the 58.3 percent no-leader mean, and by 0.8 points in homogeneous groups (SE 2.5; $p = 0.742$). The traditional-minus-religious contrast is 4.0 points in homogeneous groups (SE 2.9; $p = 0.176$) and -5.6 points in mixed groups (SE 3.0; $p = 0.063$); the difference between these gaps is $\widehat{\Delta}_C = 9.6$ percentage points (SE 4.2; $p = 0.023$). The lower panel shows similar participation across no-leader groups ($p = 0.562$), suggesting that the pattern is not driven by underlying differences between homogeneous and mixed groups.

We interpret these results as consistent with chiefs and pastors having different scopes of authority. Leaders are effective when participants recognize their right to ask them to contribute. Chiefs derive this standing from kinship, ethnic ties, and their customary responsibility for *salongo*. Thus, when chiefs share origins with participants in the group, their presence strengthens the pressure to meet a demand that participants already recognize as legitimate. In contrast, chiefs may lack the same standing to demand contributions from residents of other origins in mixed teams. Pastors derive their authority from a shared Christian worldview that crosses territorial boundaries. Because mixed teams lack a common customary basis for participation,

pastors can frame community work as an obligation before God and provide a shared reason to contribute. The same appeal has little impact in homogeneous teams, where participation is already recognized as an obligation enforced by custom. God's judgment matters where the chief is not the recognized enforcer.

Composition changes which leader can sustain collective action. A shared territorial origin places *salongo* within the chief's recognized authority, while mixed origins create room for authority that crosses territorial boundaries. This is our *scope of authority* result.

The response is concentrated in labor. Across alternative outcome definitions, the effects primarily appear in labor participation. We estimate Equation (1) using alternative measures of labor participation: total days, total hours in levels, inverse-hyperbolic sine transformation of total hours, and ratings of effort by city staff. Across the same 1,040 participants and main outcome window, the Δ_C pattern corresponds to 0.56 additional days of participation. Total hours contributed and rated effort also move in the same direction. Adjusted p-values across the four labor outcomes are at most 0.047 for Δ_C (Appendix Table A-10), and the asinh-hours specification yields the same conclusion (Appendix Table A-11). Contrastingly, cash and material contributions show no comparable pattern (Appendix Tables A-12–A-13).

Robustness to estimation and inference. The main pattern is stable after accounting for exposure to nearby treated groups and adding baseline controls (Appendix Table A-9 and Appendix D). Conditional randomization inference, which repeats the original group-formation procedure across 5,000 assignments while holding the realized sample fixed, yields $p = 0.039$ for Δ_C (Appendix Table A-14). The main conclusions also hold when allowing errors to be correlated across groups assigned to the same leader. With neighborhood-level clustering, Δ_C is no longer significant at the 5% level (Appendix Table A-15).

Alternative explanation I: pastors' null is not due to ethnic mismatch. Pastors may fail in homogeneous groups because they do not always share the territorial origins that link chiefs to those participants. In this account, pastors would fail to sustain participation in homogeneous groups not because they lack the scope for doing so, but because they are from a different territory. Out of the twenty-eight localities and twenty-eight recruited religious leaders, seventeen share their origins with the chief and the homogeneous groups in the neighborhood, while eleven do not. We leverage these seventeen religious leaders to verify whether our conclusion holds in the restricted sample.

Appendix Table A-16 restricts pastors' comparisons to neighborhoods where the assigned chief and pastor have the same territorial relationship with homogeneous-group participants. Pastors with the relevant territorial connection do not become more effective in homogeneous groups, and their effect remains concentrated among groups composed of diverse origins. The pastor result, therefore, reflects the reach of religious leadership across territorial boundaries rather than a mechanical difference in leader origin.

Alternative explanation II: leadership by example. Leadership by example would predict larger effects under leaders who contributed more before the experiment (Jack and Recalde, 2015). Appendix Table A-17 compares leaders above and below their type-specific median participation in the common pre-meeting week. The higher-minus-lower difference is −5.8 percentage points for chiefs in homogeneous groups (SE 5.2; $p = 0.262$) and 3.2 points for pastors in mixed groups (SE 5.2; $p = 0.542$). These estimates provide little support for differences in leaders' earlier participation as an explanation of the composition pattern.

4.3 Heterogeneous Treatment Effects: Who Can Mobilize Whom?

Our key result suggests that leaders have distinct scopes of authority. It suggests that chiefs are effective when they have the recognized authority to demand contributions from the group. Given the customary basis of their position, this authority is strongest among residents who share their ethnic and territorial networks. Pastors, on the other hand, seem to be effective when residents accept their claim that participation is an obligation enforced by God. But what determines the boundaries of each leader's authority? Do respondent characteristics predict where their effects are strongest in ways consistent with this interpretation?

We examine this question using residents' relationships with their assigned leader, their territorial networks, and their religious orientation. For chiefs, we investigate whether personal recognition can extend authority beyond homogeneous teams. For pastors, we ask whether otherwise similar residents respond differently across group compositions and whether their receptiveness to religious authority predicts participation. We estimate heterogeneity across these characteristics using an extension of the main specification. Let Z_{ig} indicate the higher category of the characteristic under study. Retaining the neighborhood-by-composition-by-disclosure fixed effects from Equation (1), we estimate

$$\begin{aligned} Y_i = \lambda_{n(i),q(i),c(i)} + \rho Y_{i0} + \sum_{c\in\{H,M\}} \eta_c Z_{ig} \mathbf{1}\{c(i) = c\} \\ + \sum_{L\in\{T,R\}} \sum_{c\in\{H,M\}} \left(\tau_c^L + \gamma_c^L Z_{ig}\right) \mathbf{1}\{L_{g(i)} = L, c(i) = c\} + X_i'\beta + \varepsilon_i. \end{aligned} \tag{2}$$

Here L_g denotes the assigned leader type. Both leader effects and the higher-minus-lower differences, γ_c^L, are pooled across disclosure. When Z_{ig} is observed in every assignment arm, the η_c terms allow no-leader participation to differ across categories within each composition. When the characteristic exists only for groups assigned a leader, as with ratings of the assigned leader, we interact it only with the relevant leader-by-composition indicators. For shared territory with the neighborhood chief, we estimate heterogeneity only in mixed groups, where territorial membership varies; homogeneous-group effects remain pooled.

The main figures include the pretreatment outcome and the design fixed effects. Appendix Tables A-18–A-23 also add the 25-meter spillover indicator, gender and wealth strata, and the baseline characteristics selected by the balance checks. Appendix Section A.3.1 describes the variables and missing-value treatment.

Standard errors are clustered by accountability group.

Chiefs reach mixed groups when residents recognize their authority. Ethnic homogeneity may not be a hard boundary for chiefs' authority. We investigate this possibility by comparing leader effects between: (i) residents who viewed the leader as able to sanction and reward them and those who did not (secular authority); (ii) residents who held the leader in high regard and those who did not; and (iii) residents of mixed groups who shared their territorial origin with the neighborhood chief. The first two measures use assessments collected before the first meeting discussion. Missing assessments are filled with the mean reported by other ordinary members of the same group, leaving 1,037 participants with usable data.

The first two exercises suggest that chiefs can promote participation in mixed groups when residents recognize their standing. Figure 4A and Appendix Table A-18 compare residents above and below the secular-authority cutpoint. Each column shows effects in mixed and homogeneous groups, with the same pooled no-leader references below because the assessment is undefined without an assigned leader. In mixed groups, the chief increases contributions by 8.2 percentage points among residents above the cutpoint (SE 4.8), compared with −2.3 points among those below it (SE 3.4). The 10.6-point difference is suggestive (SE 6.0; $p = 0.080$).

Relational evaluations produce a clearer difference. We asked each resident to rate the leader in their team in terms of trustworthiness, hard work, moral stance, and how much the leader inspires them. Figure 4B and Appendix Table A-19 show an 8.4-point chief effect in mixed groups among residents who evaluate him more favorably (SE 4.0), compared with −4.1 points among those who do not (SE 3.6). The difference is 12.4 points (SE 5.2; $p = 0.018$). The corresponding differences for pastors are smaller and imprecisely estimated. These comparisons suggest that a chief can mobilize outside the homogeneous group when residents recognize his standing or view him favorably.

Territorial ties point in the same direction, although the comparison is less precise. Figure 5A and Appendix Table A-20 compare residents of mixed teams with the same or different territorial origin as the neighborhood chief. Both leader types use the chief's origin as the reference. The chief effect is 6.1 points among residents who share his territory (SE 7.3) and −1.0 points among those who do not (SE 3.3). The 7.1-point difference is imprecise (SE 8.6; $p = 0.405$). Because the estimate draws on relatively few residents of mixed groups who share the chief's origin, we treat this result as suggestive of the same boundary, without establishing a territorial difference in effects.

Pastors' reach depends on the social context. The same exercise helps distinguish a limit of pastors' influence among the chief's coethnics from a limit created by homogeneous teams. The right column of Figure 5A shows that pastors raise participation by 14.7 points among the chief's coethnics placed in mixed groups (SE 7.3; $p = 0.044$), while their effect in homogeneous groups is 0.8 points (SE 2.5). The 13.8-point difference is suggestive (SE 7.7; $p = 0.072$). The positive response in mixed groups shows that the

chief's coethnics can respond to pastors. It is consistent with an interpretation in which shared origins give customary obligations precedence over religious appeals in homogeneous teams.

We complement this comparison by examining residents' dependence on territorial ties. At baseline, we asked respondents to identify all friends with whom they would share important plans. After eliciting the list, we identified which of those friends shared the same origin as the respondent (Breza et al., 2020). Figure 5B and Appendix Table A-21 divide groups by the share of their members' confidants who come from the member's own territory. The resulting median share is 80 percent. In homogeneous groups at or below this median, pastors raise participation by 10.0 points (SE 3.9), compared with −4.2 points above it (SE 3.9). The higher-minus-lower difference is −14.3 points (SE 6.2; $p = 0.022$). In mixed groups, the pastor effect also declines as territorial dependence increases, from 8.6 points (SE 3.7) to 2.0 points (SE 5.6), although the −6.6-point difference is imprecise (SE 7.0; $p = 0.349$). The evidence is clearest within homogeneous teams, where stronger territorial networks leave less room for pastors to influence participation. This evidence thus strengthens the interpretation that the more a homogeneous group relies on their ethnic networks, the less influence the group is under religious motivations.

Pastors reach residents through personal faith. Following the Allport and Ross (1967) religious-orientation scale, we distinguish between intrinsic religiosity, where faith is internalized as a guide to life, and extrinsic religiosity, where religion serves other personal or social needs. We further separate personal reliance, such as seeking comfort or support from religion, from social reliance, such as using the church for social contact. If pastors work through a religious frame of obligation that resonates with the audience listening to the appeal, we should expect greater influence among residents for whom religion guides or supports personal conduct. We combine the standardized intrinsic and personal-reliance scores into one equally weighted index and divide residents at its median.

Figure 6A and Appendix Table A-22 present the results. In mixed groups, pastors raise participation by 12.0 points among residents above the median (SE 4.3), compared with −0.1 points among those below it (SE 4.4). The 12.1-point difference is suggestive (SE 6.5; $p = 0.063$) and remains similar with the additional controls (11.3 points; SE 6.7; $p = 0.091$). Chiefs display no comparable gradient. This pattern supports the interpretation that pastors' influence depends on residents' personal receptiveness to religion.

The social component of religiosity provides a useful comparison. Figure 6B and Appendix Table A-23 show pastor effects in mixed groups of 7.1 points among residents below the church-as-social-contact cutpoint (SE 4.4) and 5.4 points among those above it (SE 4.2). The −1.7-point difference is imprecise (SE 6.3; $p = 0.790$). The pooled estimates therefore provide no clear evidence that responsiveness to pastors increases with religion's role as a social activity.

Denominational ties may extend religious authority. Collected data also allow us to establish whether residents share the neighborhood pastor's religious denomination. This matters for whether mobi-

lization needs to occur through a familiar set of dogmas or can hold through a broad religious framing of obligation. Appendix Table A-24 shows that pastors raise participation by 7.6 points among mixed-group residents who do not share their denomination (SE 3.1; $p = 0.015$). Within-denomination links are therefore not necessary for this effect. In homogeneous groups, the pastor effect among residents who share his denomination is positive, at 12.3 points, but imprecise (SE 8.3; $p = 0.142$); its difference from the effect among other residents is also imprecise ($p = 0.155$). Only 42 religious-arm groups contain a resident who shares the pastor's denomination, so the possibility that parochial ties extend pastors' reach into homogeneous groups remains tentative.

Which leaders can exercise their authority? These comparisons suggest that chiefs' scope depends on personally recognized authority, while pastors' scope depends on the social context and residents' receptiveness to religious obligation. Appendix Section A.3.2 complements this evidence with leaders' own characteristics. Chiefs' effects are suggestively stronger among those with deeper local roots in homogeneous groups, while pastors' effects are stronger among those with greater conviction in divine moral enforcement in mixed groups. These comparisons are consistent with chiefs drawing on established local standing and pastors on religious claims over divine judgment. We next ask what makes participation consequential once a leader falls within the audience's recognized scope.

5 Source of Enforcement Results

A question that naturally follows is how authority is enforced. The preceding results suggest that chiefs draw on personally recognized standing, while pastors draw on residents' faith in social contexts receptive to religious obligation. These patterns hint at different sources of enforcement but do not establish what makes participation consequential under each leadership type. Do residents contribute because they expect others to observe and judge their behavior? Or do they comply even when others are not watching because they have internalized the obligation?

For chiefs, the greater responsiveness of residents who recognize their capacity to sanction and reward is consistent with their acting as *external*, social enforcers of behavior (Kosfeld and Rustagi, 2015). If this account explains their influence, increased public accountability should strengthen their capacity to enforce behavior. For pastors, the gradients in personal religiosity and leaders' own conviction in divine moral enforcement point toward internal religious motivations (Shariff and Norenzayan, 2007). Such motivations do not require others to observe participation, so public accountability need not strengthen pastors' influence. Their effect could even decline if disclosure substitutes for internal incentives or shifts attention toward social image concerns that provide weaker incentives in ethnically mixed groups. Yet personal faith does not rule out a religious version of social enforcement: costly behavior may also signal religious commitment and enable future cooperation (Iannaccone, 1992; Levy and Razin, 2012). To the extent that pastors' influence operates

through this social channel, public accountability should instead strengthen their impact.

We next use our experimental variation in public accountability to disentangle these sources of enforcement.

5.1 Empirical Strategy

We now set $e(i) = (q(i), c(i))$ and $\mathcal{E} = \{(0,H),(1,H),(0,M),(1,M)\}$, producing leader effects in each composition-by-disclosure environment while retaining the sample, baseline control, and fixed effects from Equation (1). This specification asks whether the two leaders' effectiveness also depends on the visibility of individual participation. We expected chiefs to be most effective in homogeneous groups with disclosure, where the group falls within their authority and they can evaluate individual behavior. Pastors should rely less on disclosure because their authority can also make participation binding through internal accountability. We therefore define the environment gradient as the difference in the traditional-over-religious advantage between homogeneous groups with disclosure and mixed groups without disclosure:

$$\Delta_E = \left(\tau^T_{1,H} - \tau^R_{1,H}\right) - \left(\tau^T_{0,M} - \tau^R_{0,M}\right)$$

Δ_E measures how much the relative effectiveness of chiefs and pastors changes between homogeneous groups with disclosure and mixed groups without disclosure. These are the two environments in which their scope of authority and sources of enforcement should differ the most.

5.2 Main Results

To distinguish between internal and external sources of enforcement, we randomized whether meetings disclosed individual information about peers' contributions. In teams with disclosure, each member's contribution record was public at every meeting, while teams without disclosure saw only the total group contribution. This variation changed how much participants knew about their peers' participation. Disclosure can strengthen external enforcement by making behavior visible to others, but it should not strengthen self-judgment because individuals already observe their own actions.

Figure 7 and Appendix Table A-28 show how leader effects across homogeneous and mixed teams vary under the two disclosure conditions. In homogeneous groups with disclosure, assigning a traditional leader increases participation by 10.0 percentage points (SE 3.6; $p = 0.006$), while the religious-leader effect is 0.0 points (SE 3.3; $p = 0.997$); the direct traditional-over-religious gap is 9.9 points (SE 4.0; $p = 0.014$). In homogeneous groups without disclosure, neither leader has a statistically significant effect relative to no leader. Pastors show the opposite pattern in mixed groups. In mixed groups with disclosure, neither leader has a statistically significant effect. The pastor's effect in mixed groups is concentrated in teams without disclosure. In mixed groups without disclosure, religious leadership raises participation by 9.2 percentage points (SE 4.0; $p = 0.021$) relative to no leader. The traditional-leader effect in this environment is -0.3

points (SE 3.5; $p = 0.933$). The traditional-minus-religious gap in mixed groups without disclosure is -9.5 points (SE 4.0; $p = 0.018$). The cross-environment contrast summarizes how the relative effectiveness of chiefs and pastors changes between these two environments. It equals $\widehat{\Delta}_E = 9.9 - (-9.5) = 19.4$ percentage points (SE 5.7; $p < 0.001$).

These results reinforce the interpretation that religious leaders are weak mobilizers of homogeneous groups and that chiefs are weak mobilizers of mixed groups due to their mismatched scope. They also provide evidence that chiefs and pastors mobilize through distinct enforcement channels. This double reversal is difficult to reconcile with a common social mechanism that simply operates more strongly for one leader than the other. Chiefs in same-origin groups appear to rely on external enforcement, with residents responding to the chief's judgment of their behavior. In contrast, pastors appear to work through internal religious accountability, using a shared Christian framework that leads residents to evaluate their own behavior before God. This is our *source of enforcement* result.

The response remains concentrated in labor. The environment gradient also appears across the alternative labor measures introduced in Section 4.2. It corresponds to 1.10 additional days of participation, with adjusted p-values below 0.004 across the four labor outcomes (Appendix Table A-30). Total hours and rated effort move in the same direction, and the asinh-hours specification yields the same conclusion. Cash and material contributions show no comparable pattern (Appendix Tables A-32–A-33).

Robustness to estimation and inference. The main pattern is stable after accounting for exposure to nearby treated groups and adding baseline controls (Appendix Table A-28 and Appendix D). Conditional randomization inference, which repeats the original group-formation procedure across 5,000 assignments while holding the realized sample fixed, yields $p = 0.003$ for Δ_E (Appendix Table A-34). The main conclusions also hold when allowing errors to be correlated across groups assigned to the same leader. The environment gradient also remains statistically significant with neighborhood-level clustering (Appendix Table A-35).

Alternative explanation III: Different leader effort does not explain leader effects. A different explanation for the main effects is that leaders exerted more effort in the environments where they succeeded. It could be that public accountability allowed them to have a better targeting in out-of-meeting follow-up effort, and the results are driven mostly by leader effort instead of participant side concerns. We test this possibility using outcomes that record interactions with specific people in the group. During the midline surveys administered right before each meeting, we asked whether each participant had interacted with their peers in the week preceding the meeting. We leverage these outcomes to estimate an augmented version of equation (1) that accommodates directed peer-to-peer outcomes.

Let j index the person involved in the interaction, and let $J_{jg} = 1$ indicate when that person is the assigned leader in group g. We estimate the following:

$$Y_{ij} = \lambda_{n(i),q(i),c(i)} + \rho Y_{ij0} + \sum_{e \in \mathcal{E}} \Big[\left(\tau_e^T + \kappa_e^T J_{jg}\right) T_{g(i)} + \left(\tau_e^R + \kappa_e^R J_{jg}\right) R_{g(i)} \Big] \mathbf{1}\{e(i) = e\} + X_i'\beta + \varepsilon_{ij}. \quad (3)$$

Here Y_{ij} records participant i's reported interaction with person j at the start of the second meeting, and Y_{ij0} records the same interaction before the first meeting, prior to treatment. The coefficient τ_e^L captures the assignment effect for outcomes directed toward an ordinary participant, while $\tau_e^L + \kappa_e^L$ captures the effect for outcomes directed toward the assigned leader. The outreach models estimate this extension separately within each experimental environment and control for respondent pretreatment participation.

Appendix Table A-38 presents results for several types of recorded interactions. Reported contact with leaders is not greater in the environments where they induce more participation. In homogeneous groups with disclosure, reported contact with the assigned chief is only 0.7 percentage points higher (SE 7.8) than the rates of contact between ordinary members of no-leader groups. In mixed groups with disclosure, where the chief's participation effect is small, reported contact with the assigned chief is 20.3 points higher (SE 8.9) than contact between ordinary members of no-leader groups. Pastors show a similar mismatch. In mixed groups without disclosure, where the participation effect is largest, respondents report nearly the same rate of contact with the assigned pastor as between ordinary members of no-leader groups, a difference of 4.8 percentage points (SE 7.6). Requests, references to consequences, and interactions among ordinary participants show no offsetting pattern. If anything, respondents report more contact with chiefs where their effects on participation are weaker. The recorded contacts therefore provide little support for differences in follow-up effort as an explanation of where leaders mobilize participation.

Alternative explanation IV: No evidence of anticipation or mean reversion. The pre-assignment characteristics used to form groups are balanced. However, administrative participation, measured after assignment and before the first accountability meeting, differs across the realized cells (Appendix Table A-5). Participants received their invitation to participate in volunteer teams during this week, but had not yet learned about their teammates or the disclosure condition during this period. Nevertheless, two concerns arise: (i) participants may have learned that a leader was assigned to their team before the first meeting, creating anticipation effects, and (ii) even without anticipation, the estimates could be driven by mean reversion in the imbalanced cells.

We address the first concern in Appendix Table A-36, which examines the six daily pretreatment observations. The daily data show no evidence of anticipation before the first meeting. We address the mean reversion concern in Appendix Table A-37, allowing the relationship between pretreatment and subsequent participation to differ across all 12 assignment cells. All estimated coefficients remain essentially the same. Together, these checks provide little support for the imbalance as an explanation of the leader effects that emerge after the meeting.

We now present complementary evidence on chiefs' and pastors' different sources of enforcement.

5.3 What Leaders Say: Pastors Talk About God, Chiefs About Attendance

The public accountability variation suggests that chiefs and pastors draw on different sources of enforcement. If this is indeed the case, we should observe different leader contributions and approaches to coordination meetings. Do chiefs emphasize attendance and social accountability while pastors invoke obligations before God? Recordings of the first group meetings allow us to study these appeals before the subsequent participation outcomes are realized. This evidence helps assess whether the content of leaders' appeals aligns with the enforcement channels suggested by the experimental results.

Meeting recordings show what leaders and ordinary residents discussed before the treatment changed participation. If leaders sustain collective action through different claims, those differences should appear in their speech. To test this interpretation, we use a standard Latent Dirichlet Allocation model following Blei et al. (2003) to analyze transcripts from the first group meetings (Gentzkow et al., 2019). The model groups words that often appear together into latent topics, and is blind to every other aspect of the intervention, including group structure, treatment condition, and leader role. Because our passages are short, we follow the approach of Quinn et al. (2010) and assign each passage to one of three categories. We name the topics only after estimation. Appendix Table A-39 reports the characteristic words and translated passages that help interpret each topic:

Latent topics. The first passages in Appendix Table A-39 highlight a discourse that creates social pressure around participation. We labeled these as attendance-and-performance speeches. The excerpts display leaders setting clear targets for hours and regular participation, calling out those residents who did not show up and praising groups that meet expectations. This fits the interpretation we developed for the chiefs' source of enforcement, where participation is induced by stronger social accountability over the activity.

The last block of the table establishes a different enforcer. The examples tell volunteers to look to God rather than wages for reward, state that "God rewards each person according to their deeds", and connect collective work to patience, peace, and mutual aid. This language frames participation as a decision subject to divine judgment and calls for solidarity between peers. It is the speech counterpart to the strategy attributed to pastors, whose appeal can bind residents through self-evaluation before God, even when individual contribution records are not disclosed.

The third topic concerns operational constraints. A recurrent topic of discussion was the coordination of who would provide the materials for neighborhood improvement. Some examples concern within-group coordination, where peers would discuss and agree on who would provide the necessary tools for the work to take place. Yet, the passages also show substantial reliance on the centralized provision of these materials, with many examples praising the municipality for providing the tools for the work. Although it may be useful

material for program improvement and future research, we interpret this topic as neutral concerning our main goal. The distinction between mobilization strategies comes from the relative emphasis on attendance and performance or on divine reward and solidarity.

Who talks about what, and when? After assigning each passage to its most likely topic, we sum the passage duration to measure how many minutes each speaker spends on attendance and performance, material support, and divine rewards and solidarity. We aggregate the classified passages to the leader-by-meeting level. Figure 8 shows the average share of speaking time devoted to each topic, pooling the four composition-by-disclosure environments. Blue bars report traditional leaders and gold bars religious leaders. Appendix Figure A-4 reports total speaking time and topic shares separately by environment.

The figure shows that leaders' discourse differs across leader types. Aligned with our interpretation, chiefs devote 44.1 percent of their speech to attendance and performance, compared with 26.0 percent among pastors. In contrast, pastors devote 48.3 percent to divine reward and solidarity, compared with 27.4 percent among chiefs. The differences remain significant after multiple testing adjustments ($q = 0.009$), while material-support shares are similar (28.5 vs. 25.7 percent). Appendix Figure A-4 shows that these differences persist across experimental conditions. Pastors speak for 3.43 minutes on average, compared with 2.55 minutes for chiefs ($p = 0.067$). Within each leader type, speaking time and topic shares are fairly stable across composition and disclosure conditions. All tests of equality in topic shares across environments have $p > 0.30$, while the speaking-time tests have $p = 0.701$ for chiefs and $p = 0.139$ for pastors. Thus, what participants hear differs mainly by leader type, not by the experimental environment.

Although what leaders say is similar across experimental conditions, their influence on the discussion varies. Appendix Figure A-5 shows estimates of Equation (1) using participants' time spent on each topic. Assigning a chief shifts the discussion toward attendance and performance most strongly in homogeneous groups with disclosure, while assigning a pastor shifts the discussion toward divine reward and solidarity most strongly in mixed groups without disclosure. These are also the environments where each leader type most increases participation. The pattern suggests that leaders are most able to steer the discussion toward their characteristic appeals in the same environments where those appeals later change participation. The meetings therefore provide an early sign that these different appeals are working.

5.4 Heterogeneous Treatment Effects: Who Responds To Revealed Records?

The scope results show that chiefs' influence is stronger among residents who recognize their standing, while pastors' influence is stronger among residents for whom religion guides personal conduct. Meeting recordings reveal the different appeals each leader makes. We now examine a behavioral implication of these two sources of enforcement by asking who responds when contribution records are revealed.

Participation histories reveal distinct sources of enforcement. First meetings under the public information condition affect participants in two ways: (i) it establishes what the reporting conditions for future meetings will be, and (ii) it also provides information on who shirks and who contributes to everyone who attended the meeting. The treatment provides a signal that influences the social image of every group participant based on how much they have contributed up to that point. Shirkers will be the ones who suffer the largest damage to their standing since the group will find out they did not contribute. If chiefs operate through social judgment, the chiefs' effect should be strongest on exactly these shirkers, where the social standing of the participant is worse.

Under our interpretation, pastors' enforcement operates differently. Intrinsically religious residents will change their behavior because the pastor will remind them of their internal religious motivations. What that motivation is, whether it is a fear of divine punishment or moral convictions associated with religious practice, matters less than where the audience judging residents' behavior is located. In pastors' enforcement, the relevant audience is the self (or God), who always knows whether the resident cooperated or not, regardless of the disclosure condition. Therefore, there is no reason to believe that the pastor effect will also be concentrated among shirkers; it may very well extend across initial participation histories.

We test these predictions by extending Equation (2) from composition c to the full environment $e = (q, c)$, allowing both the category-specific no-leader levels and each leader effect to vary with residents' pretreatment participation in every composition-by-disclosure environment. Figure 9 and Appendix Table A-40 show the results for the two compositions where each leader is effective: chiefs in homogeneous groups and pastors in mixed groups. The top row plots each leader's effect relative to no leader at lower and higher levels of pretreatment participation. The bottom row shows the corresponding no-leader means.

Exposed shirkers drive the chief effect, pastors influence everyone. The left column of Figure 9 displays what one would expect from external social enforcement. With disclosure, chiefs raised participation by 23.5 percentage points (SE 6.3) among residents who participated one day or fewer in the first week, compared with -4.4 points (SE 3.8) among those with higher participation. The difference is -27.8 points (SE 8.0; $p < 0.001$). Without disclosure, the corresponding effects are -9.3 and 7.9 points, and the 17.2-point difference is only suggestive ($p = 0.064$). Pastors' effects display no comparable difference based on prior participation. The positive chief effect is concentrated among prior nonparticipants and infrequent participants. The weak visible record gives the chief something to judge and the resident something to repair, while residents with stronger records have less room to improve. Appendix Table A-41, which displays robustness specifications, yields the same reversal.

The religious leader effect displays a completely different pattern of results. Pastor effects are positive in both baseline-participation categories when groups are mixed and individual records are not disclosed, at 6.1 points among residents with weaker prior participation (SE 6.3) and 9.9 points among those with

stronger records (SE 5.6). Each subgroup estimate is imprecise, while the pooled pastor effect is positive and statistically significant in the main specification. The 3.8-point higher-minus-lower difference is also imprecise (SE 8.9; $p = 0.666$). This result strengthens the interpretation that they operate through internal incentives, but it does not explain why the pastor effect is weakened by public disclosure.

Unpacking the null: public accountability substitutes for the pastor effect. A possible explanation can be seen in the bottom panel of Figure 9. The aggregate no-leader comparison conceals differences between initial shirkers and stronger contributors. In no-leader groups, the implied participation of higher contributors is 62.4 percent without disclosure and 73.2 percent with disclosure, while the corresponding prediction falls for lower contributors. These are standardized no-leader predictions for the residents supporting each leader effect. The implied no-leader difference for higher contributors is 10.8 points (SE 5.6; $p = 0.054$), while the pastor effect falls by 12.3 points across disclosure conditions (SE 7.6; $p = 0.108$; Appendix Table A-40). These differences suggest that public accountability can substitute for internal accountability among stronger contributors, although the pastor-effect contrast is imprecise. The lower predicted participation among weaker contributors is also consistent with a less favorable response to disclosure.

Peer behavior provides a related version of the same mechanism. Appendix Table A-42 splits residents by the pretreatment hours supplied by their other ordinary group members, estimating heterogeneous effects on residents who encountered highly active vs inactive peers. In mixed groups with disclosure, the pastor effect is 8.8 points when peers were less active and -2.3 points when they were more active, a difference of -11.0 points ($p = 0.235$), while we do not observe the same decrease under no disclosure. Again, the pattern in no-leader groups mimics that of the previous paragraph. Under disclosure, implied participation without a pastor is 50.6 percent among residents with less active peers and 66.0 percent among those with more active peers, a 15.4-point gap. Without disclosure, the corresponding gap is only 4.3 points. Public accountability combined with high participation creates strong social incentives to sustain high contributions. The direction is consistent with social accountability reducing the additional influence of a pastor, although the imprecise difference limits this interpretation.

6 Beyond Participation: What Else Changes?

Our results establish when leaders can promote collective action and develop a theory of why they succeed. What are the consequences of this success? The main behavioral outcomes are observed during a short window of time, but sustained cooperation may reshape the social and political views of participants, as well as their relationships. We turn to investigating these possibilities by examining beliefs about cooperation and accountability, the salience of religious and social identities, views of government and other local institutions, and opinions about other members of the group. For the first set of outcomes measured at the individual level, we use the specification in Equation (1) to assess leader treatment effects in different group environments.

More participation does not change beliefs or political attitudes. The first set of outcomes captures beliefs about social and divine accountability and the identities residents consider most important. The second covers government services and accountability, institutional performance, trust in state and nonstate actors, and evaluations of the locality chief. Across these outcomes, we find no robust patterns. Appendix Tables A-43, A-44, and A-45 report the full results. Additional participation therefore changed behavior without broadly changing residents' beliefs, identities, or political attitudes.

Sustained cooperation precedes greater willingness to help one another. The clearest changes concern relationships within the group. We compare how residents evaluated ordinary peers and assigned leaders before the first discussion and at the start of the second meeting. The outcome is the standardized change in these evaluations, estimated using Equation (3). Because the initial evaluation is already part of the outcome, we omit the separate Meeting 1 control and the additional controls in X_i; the design fixed effects remain unchanged. Appendix Tables A-46 and A-47 report all six outcomes across the four experimental environments. Figure 10 focuses on willingness to help. It shows separately the environment where each leader increased participation and pools the other three environments.

The effects are sizable where leaders sustain participation. In homogeneous groups with disclosure, assigning a chief increases residents' willingness to help an ordinary participant by 0.20 standard deviations. At the average baseline answer of about 4,600 Congolese francs, this effect corresponds to roughly 1,400 francs more willingness to help, or 30 percent of the baseline mean. In mixed groups without disclosure, assigning a pastor increases willingness to help an ordinary participant by 0.35 standard deviations and willingness to help the pastor by 0.34 standard deviations. These effects correspond to about 2,750 and 2,670 francs, or 59 and 58 percent of the baseline means. Other evaluations of peers do not change systematically. Sustained cooperation therefore appears to strengthen willingness to assist others within the group rather than improve opinions of peers more generally.

Pastors gain support when they sustain cooperation. The pattern is different for peers and leaders. Chief assignment mainly increases willingness to help ordinary peers, while evaluations of the chief change little. Pastor assignment increases the willingness to help both ordinary participants and the pastor himself by about the same amount. This difference fits their initial positions. Chiefs already hold established authority among coethnics, while pastors enter mixed groups without the same standing and can gain it when they successfully sustain cooperation. Religious leaders can therefore leave divided groups more cohesive while also gaining greater support from residents.

7 Conclusion

What can unite ethnically divided societies around collective goals when social enforcement breaks down? In Kananga, we have found that religious authorities succeed in promoting collective action in ethnically heterogeneous groups where traditional authorities have failed. In a large-scale field experiment that involved more than a thousand volunteers and hundreds of small neighborhood improvement projects, they not only succeeded in promoting the participation of fragmented teams; they were especially more likely to succeed when information about participation was unavailable and social enforcement was the hardest. Our analysis therefore points to two reasons that explain their success: the authority they invoke can work across ethnic relationships, and enforcement occurs within rather than being dependent on social sanctions.

Perhaps more surprising is the result that religious leaders *fail* to sustain cooperation precisely in settings where traditional leaders succeed. When the audience is ethnically cohesive *or* public accountability is salient, religious leaders are less effective in sustaining cooperation. Complementary evidence suggests that these weak cues of social accountability, whether induced by participants' shared identity or the salience of public records, can prevent self-accountability activated by religious moral appeals from influencing behavior and ultimately lower total contributions. Unlike traditional leaders, who display patterns consistent with an authority that depends on social monitoring and is rooted in ties to each resident that may or may not be ethnic, religious leaders' authority relies on preexisting beliefs and on a setting that allows those beliefs to guide behavior.

These findings expose two dimensions of authority that are rarely studied in tandem and show why future considerations of these dimensions should matter for understanding how collective action operates. Its *scope* determines whom a leader can mobilize; its *source* determines what makes participation binding—evaluation by others, obligations individuals apply to themselves, or both. Collective action depends on the fit between a leader's reach, the population being mobilized, and the conditions under which participation becomes binding. Although extending these considerations to other leaders and institutions requires caution and further research, the implications of this theory of change could apply far beyond religion, customs, and the DRC. More broadly, the same logic could apply where a narrow identity sustained by social accountability coexists with a more encompassing identity whose obligations are internally enforced.

Therefore, beyond the policy implications of the studied framework, which can be applied to any development policy that uses local leaders as intermediaries in program implementation, the paper opens important research avenues by revisiting the idea that fragmentation is a corrosive force for collective action. Indeed, our paper confirms that fragmentation is a corrosive force for the *social enforcement* of collective action. Yet, by corroding social enforcement, something else also happens: internal motives begin to enforce conduct. Fragmentation does not simply corrode collective action; it changes which institutions can govern behavior. Religion is one widespread institution that enables cooperation in divided settings, potentially among many. What else can govern cooperation when external enforcement breaks down remains object of future research.

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Figures

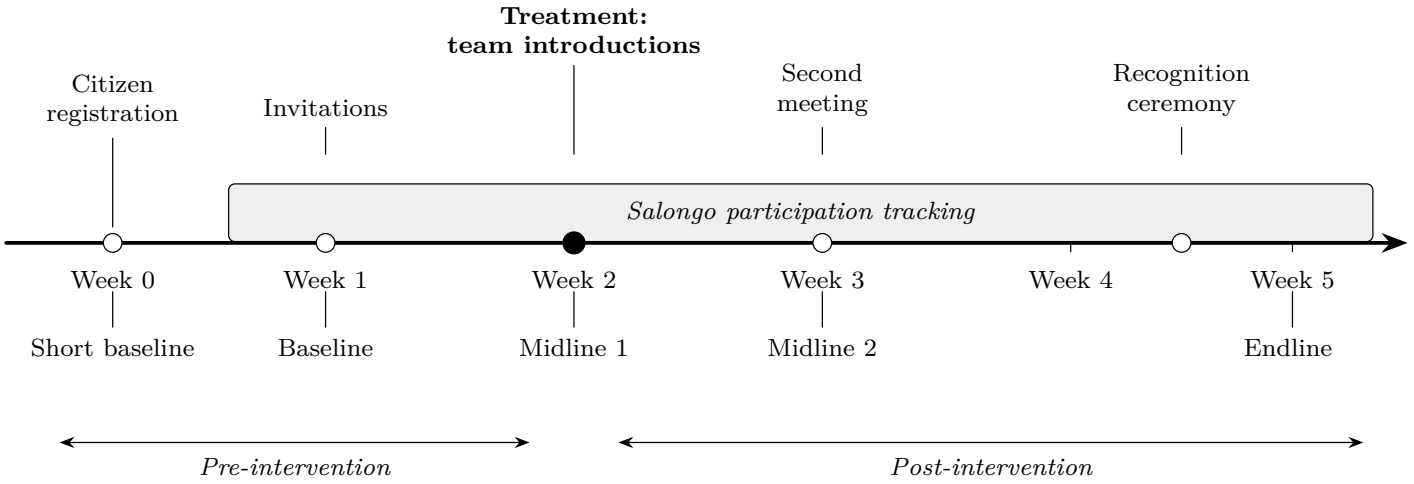
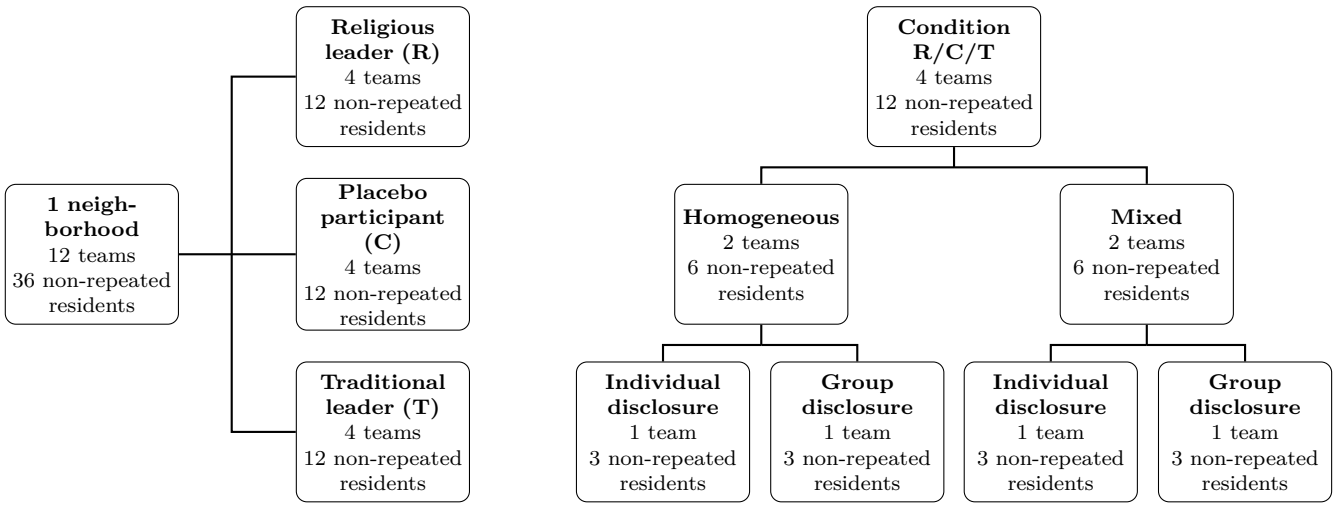


Figure 1: INTERVENTION AND DATA COLLECTION TIMELINE

Notes: The figure follows one five-week campaign wave. Residents registered before the city began tracking daily *salongo* participation. Invitations and the baseline survey came next. At the first meeting, residents met their assigned teammates and received the leader and disclosure conditions, marking the start of the intervention. The same team met again one week later. The city held the recognition ceremony near the end of the tracking period, and the endline followed. Survey timing appears below the timeline. The intervention is described in Section 3; Appendix Figure A-3 shows the outcome windows used in the analysis.



(a) Leader experiment **(b) Within-leader variation**

Figure 2: EXPERIMENTAL DESIGN

Notes: Each neighborhood contains twelve teams. Panel (a) assigns four teams to a traditional leader, four to a religious leader, and four to no leader. Panel (b) divides the four teams in each condition evenly between homogeneous and mixed groups and between individual and group disclosure. The diagram shows the standard four-person team used from Wave 3 onward. The slightly different no-leader roster used in Waves 1–2 is explained in Appendix Tables A-6–A-8. The figure is discussed in Section 3; balance and implementation checks appear in Appendix Tables A-5–A-8.

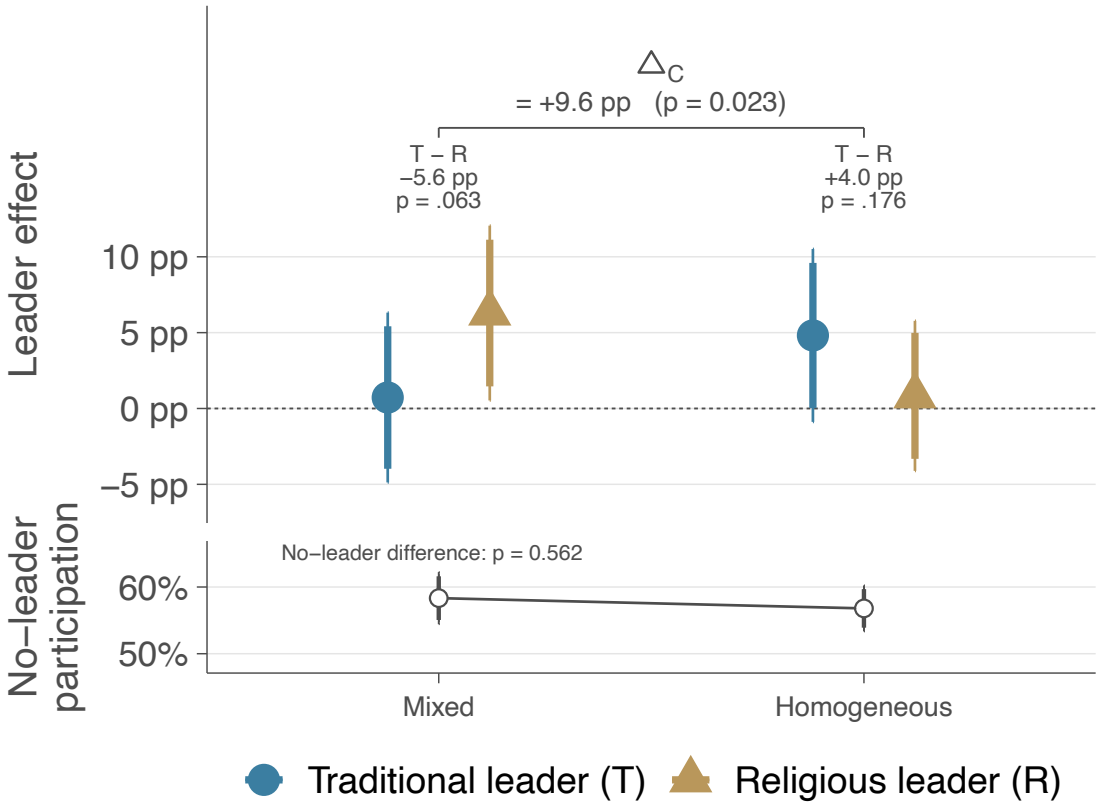


Figure 3: LEADER EFFECTS BY GROUP COMPOSITION

Notes: Blue circles and gold triangles show how assigning a traditional leader or religious leader changes participation relative to no leader, pooling across disclosure conditions. The upper panel compares mixed and homogeneous groups; T–R labels compare the leaders directly, and Δ_C measures the change in this gap from mixed to homogeneous groups. Open circles below show no-leader participation. Thick and thin bars are 90 and 95 percent confidence intervals. Estimates adjust for pretreatment participation and compare groups within the same neighborhood, composition, and disclosure condition. Section 4.2 discusses the figure; Appendix Table A-9 reports the estimates and specification checks.

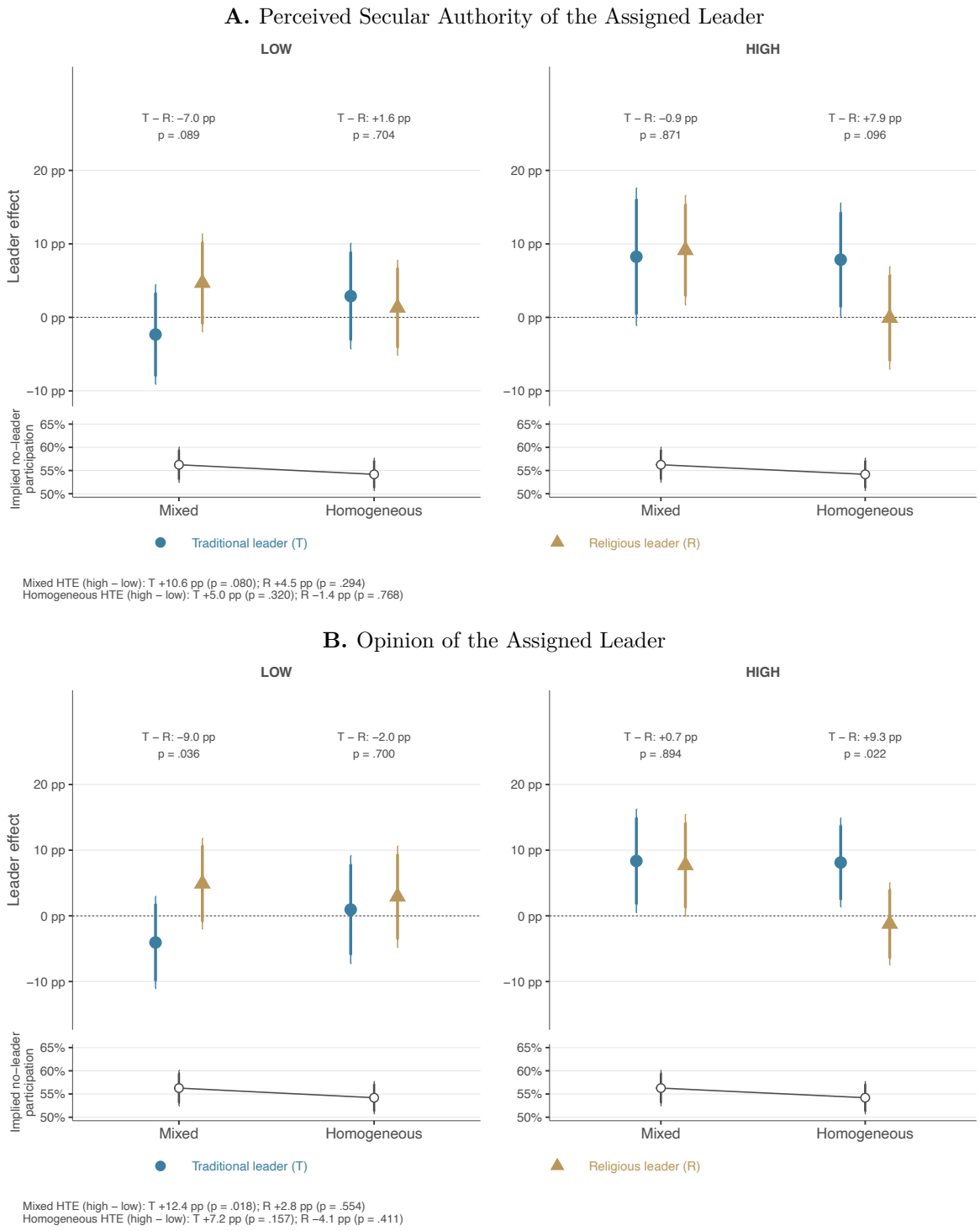


Figure 4: RECOGNIZED AUTHORITY AND LEADER EFFECTIVENESS

Notes: Panel A compares residents at or below the secular-authority cutpoint (4) with those above it. The assessment concerns the assigned leader's capacity to sanction and reward. Panel B splits the opinion index, combining trustworthiness, hard work, morality, and inspiration, at its median. Both assessments precede the first meeting discussion. Missing values for 33 residents are filled with their group's observed mean; three residents in a group without an observed score are excluded. The same pooled no-leader references appear in both columns because the assessments are undefined without an assigned leader. Blue circles and gold triangles report traditional leader and religious leader effects relative to no leader, pooling disclosure. Thick and thin bars are 90 and 95 percent group-clustered confidence intervals. Estimates include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. T–R labels compare leader types; the bottom annotations report higher-minus-lower differences. Section 4.3 discusses the figure; Appendix Tables A-18 and A-19 report both specifications for Panels A and B, respectively.

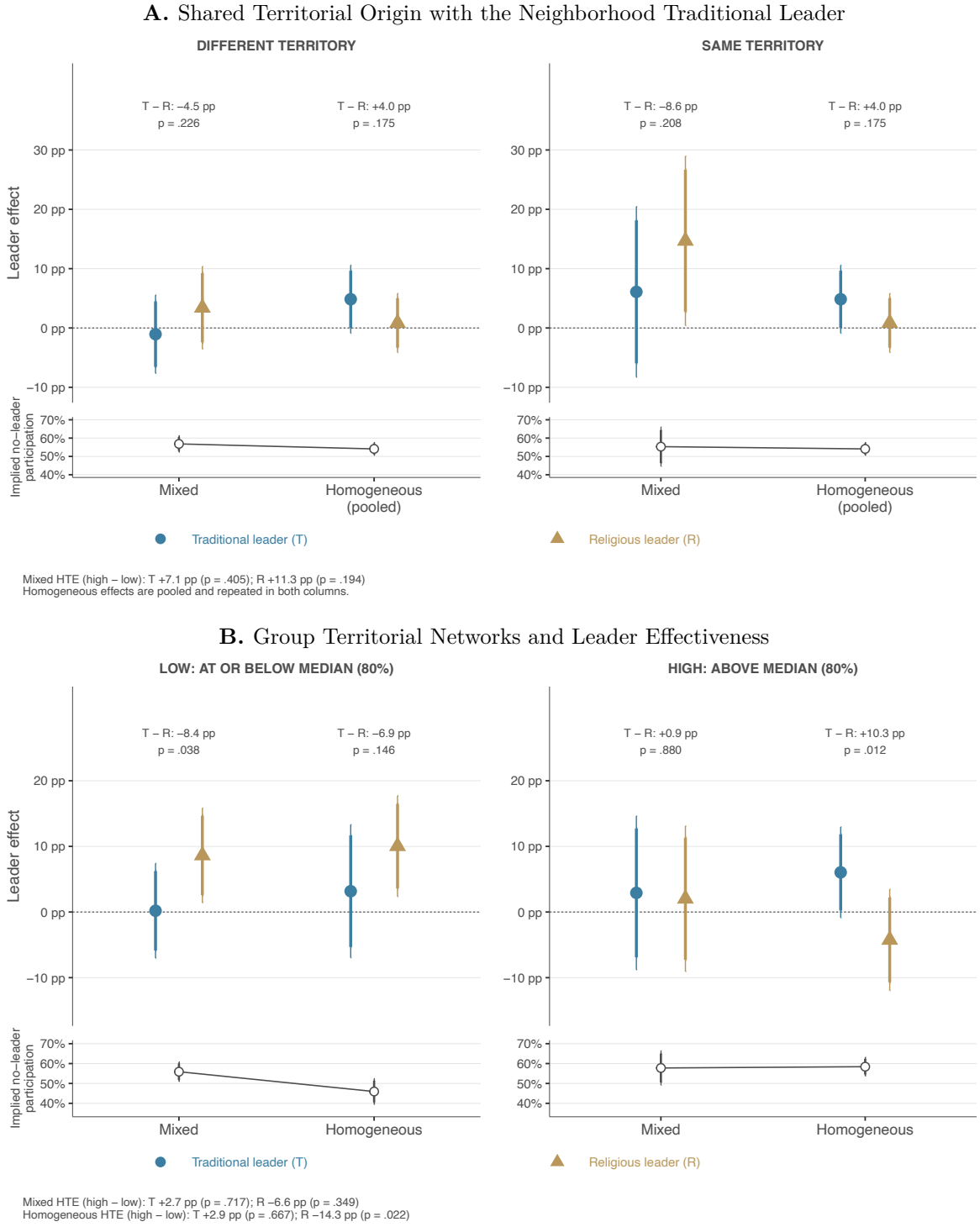


Figure 5: TERRITORIAL TIES AND LEADER EFFECTIVENESS

Notes: Panel A distinguishes mixed-group residents with a different or the same territorial origin as the neighborhood traditional leader, using the traditional leader as the reference in every assignment arm. Homogeneous-group effects are pooled and repeated in both columns. Panel B divides groups at the median same-territory share of members' confidant networks (80 percent; ties enter the lower category). Each ordinary member with valid counts is included in their own network as one same-territory contact. Shares divide summed same-territory contacts by summed total contacts; three members contribute in 330 groups and two in six. The lower plots show common no-leader predictions within each category, with a pooled homogeneous reference in Panel A. Blue circles and gold triangles report traditional leader and religious leader effects relative to no leader, pooling disclosure. Thick and thin bars are 90 and 95 percent group-clustered confidence intervals. Estimates include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. T–R labels compare leader types; the bottom annotations report higher-minus-lower differences. Section 4.3 discusses the figure; Appendix Tables A-20 and A-21 report both specifications for Panels A and B, respectively.

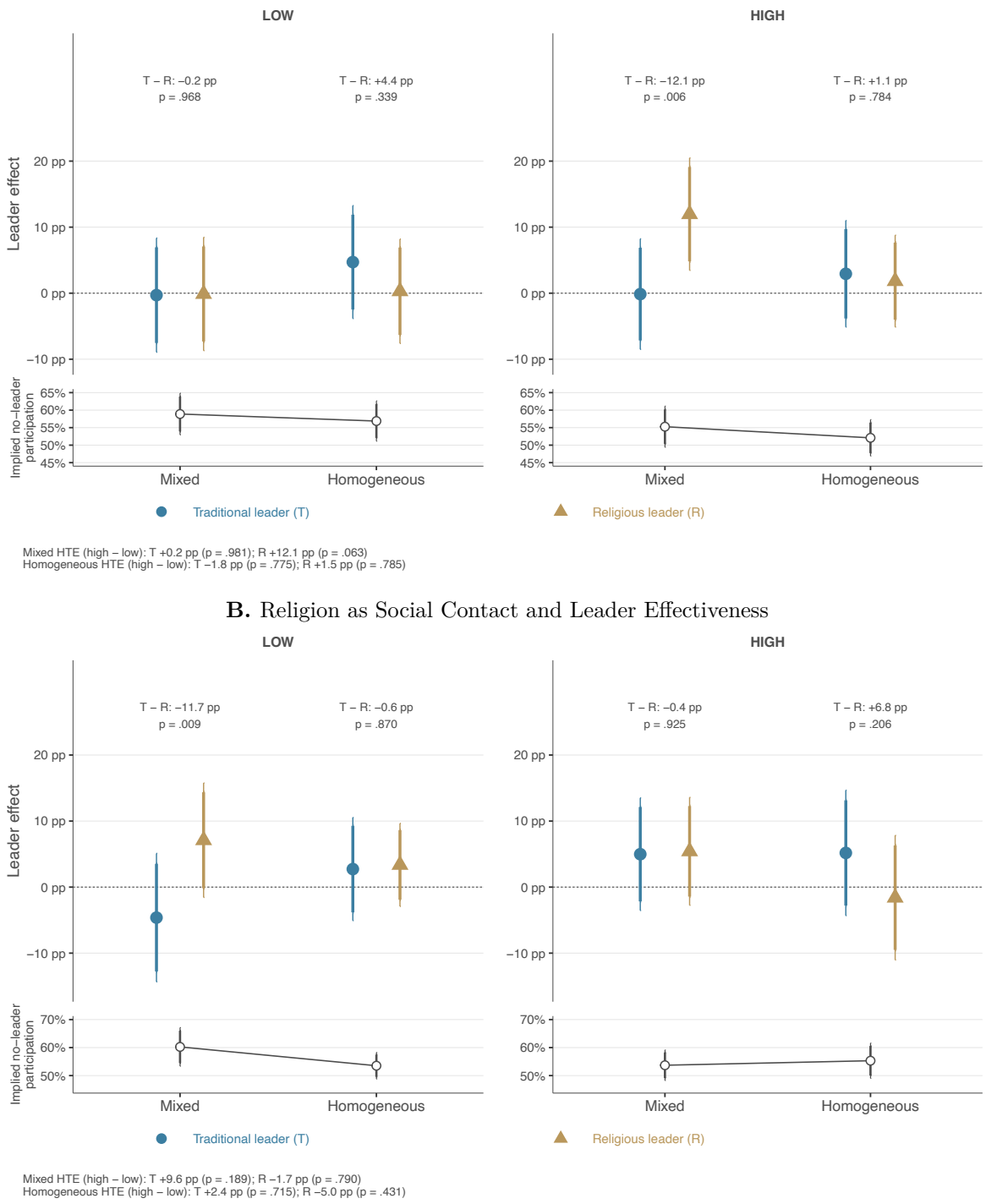


Figure 6: RELIGIOUS ORIENTATION AND LEADER EFFECTIVENESS

Notes: Panel A splits the index averaging standardized intrinsic and extrinsic-personal religiosity at its median. Intrinsic religiosity measures faith as a guide to life; the personal component measures religious comfort and reliance. Both scores are required, leaving 1,032 residents. Panel B distinguishes residents at or below and above the extrinsic-social religiosity cutpoint (approximately 2.33), measuring religion's role in social contact. The lower plots show common no-leader predictions within each category. Blue circles and gold triangles report traditional leader and religious leader effects relative to no leader, pooling disclosure. Thick and thin bars are 90 and 95 percent group-clustered confidence intervals. Estimates include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. T–R labels compare leader types; the bottom annotations report higher-minus-lower differences. Section 4.3 discusses the figure; Appendix Tables A-22 and A-23 report both specifications for Panels A and B, respectively.

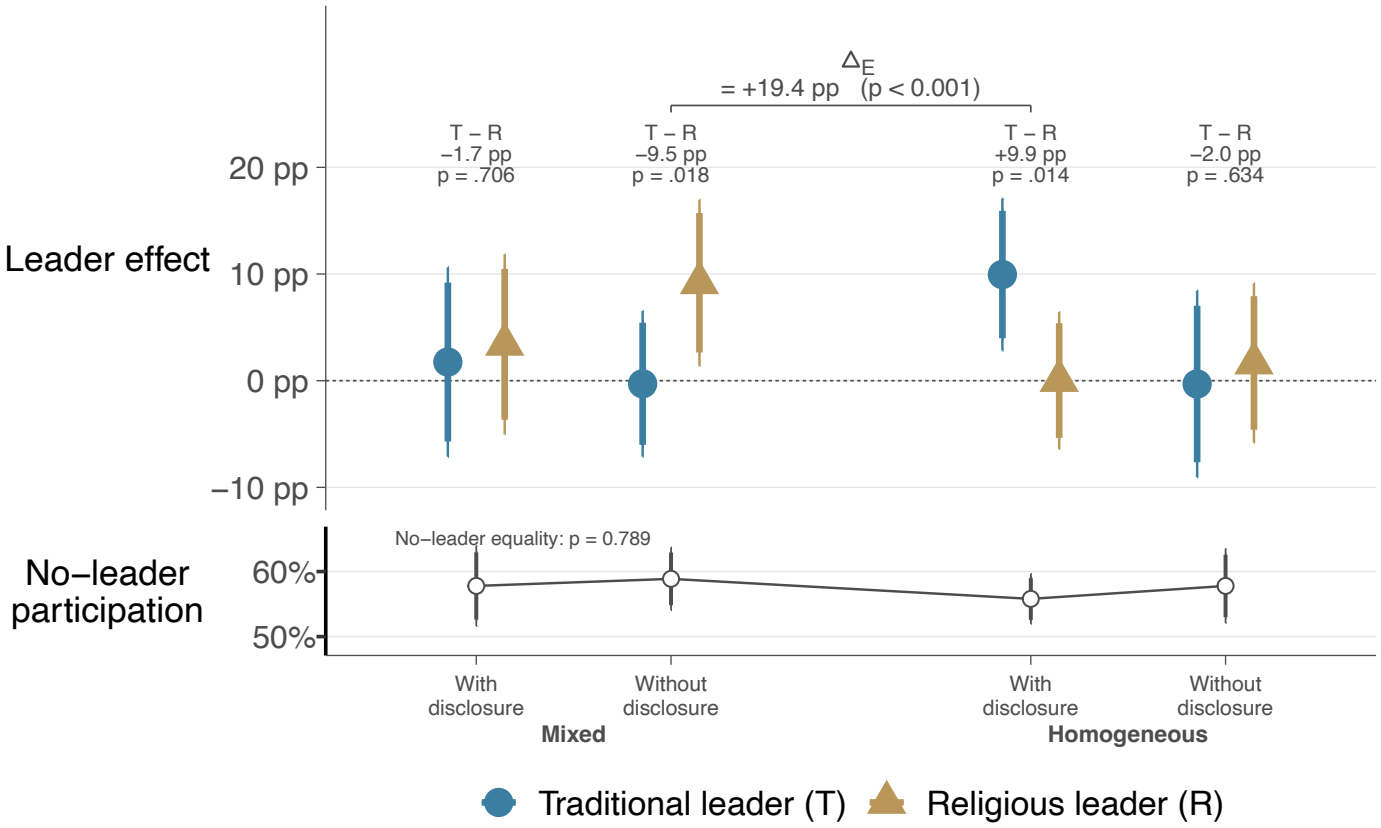


Figure 7: LEADER EFFECTS BY COMPOSITION AND DISCLOSURE

Notes: Blue circles and gold triangles show traditional leader and religious leader effects relative to no leader in each composition-by-disclosure environment. Traditional Leaders raise participation most in homogeneous groups with disclosure, and religious leaders in mixed groups without disclosure. T–R labels compare the leaders directly; Δ_E is the difference between their gaps in these two environments. Open circles below show no-leader participation. Thick and thin bars are 90 and 95 percent confidence intervals. Estimates use the same sample, pretreatment participation control, and neighborhood-by-composition-by-disclosure fixed effects as Figure 3. Section 5 discusses the figure; Appendix Table A-28 reports the estimates and specification checks.

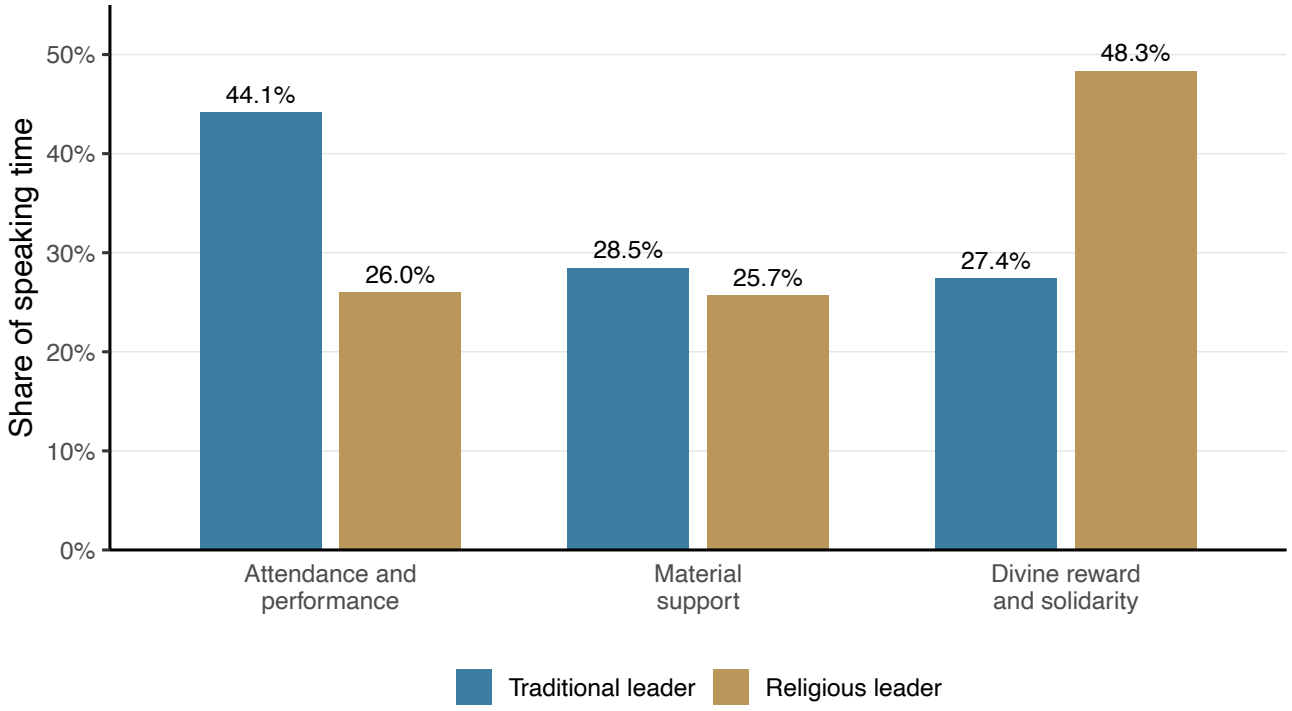


Figure 8: TRADITIONAL LEADERS STRESS PERFORMANCE; RELIGIOUS LEADERS STRESS DIVINE REWARD

Notes: Bars show the mean share of speaking time devoted to each topic during the first recorded meeting, by leader type and pooling the four experimental environments. Each classified passage is assigned to one topic and weighted by its duration. Shares are calculated within each leader-meeting observation and then averaged, giving each observation equal weight. The three shares sum to 100 percent within leader type. Blue bars report traditional leaders and gold bars religious leaders. Section 5.3 discusses the figure. Appendix Figure A-4 reports speaking time and topic shares separately by environment; Appendix Table A-39 gives characteristic words and translated examples.

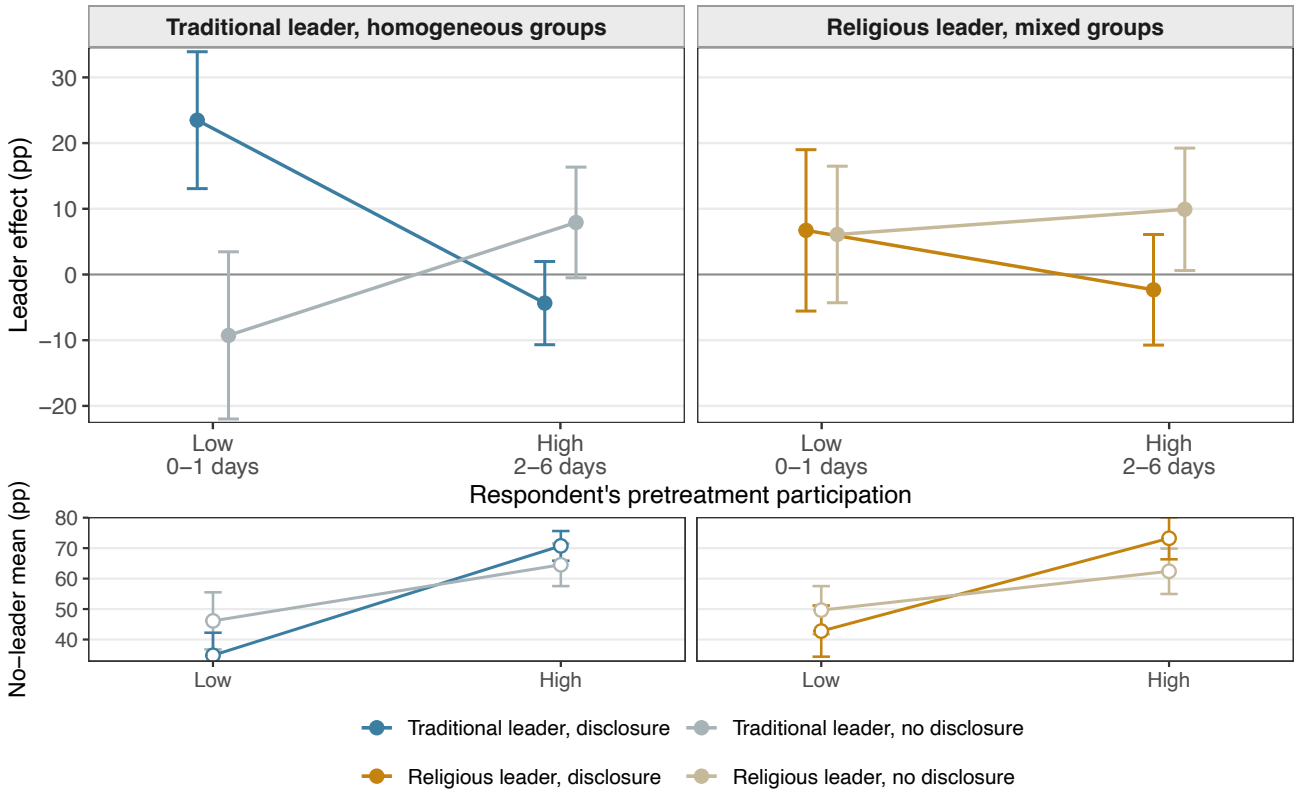


Figure 9: TRADITIONAL LEADERS DRAW IN PRIOR NONPARTICIPANTS UNDER DISCLOSURE; RELIGIOUS LEADER EFFECTS SPAN PARTICIPATION HISTORIES

Notes: Read each column from top to bottom. The upper panels show how assigning the leader changes participation for residents with lower and higher pretreatment participation. The lower panels show how often the same types of residents would participate without a leader. The left column reports traditional leaders in homogeneous groups and the right column religious leaders in mixed groups; the colors distinguish disclosure conditions. Lower participation means zero or one of the six eligible pre-meeting days, and higher participation means two to six days. With disclosure, the traditional leader effect is concentrated among prior nonparticipants and infrequent participants. Without disclosure, the religious leader effect extends across both participation groups. Showing the no-leader means below separates differences in leader influence from differences in residents' usual participation. Lines are 90 percent confidence intervals. The figure is discussed in Section 5.4; Appendix Table A-40 reports the exact plotted specification and direct disclosure contrasts; Table A-41 reports additional-control and fixed-effect checks.

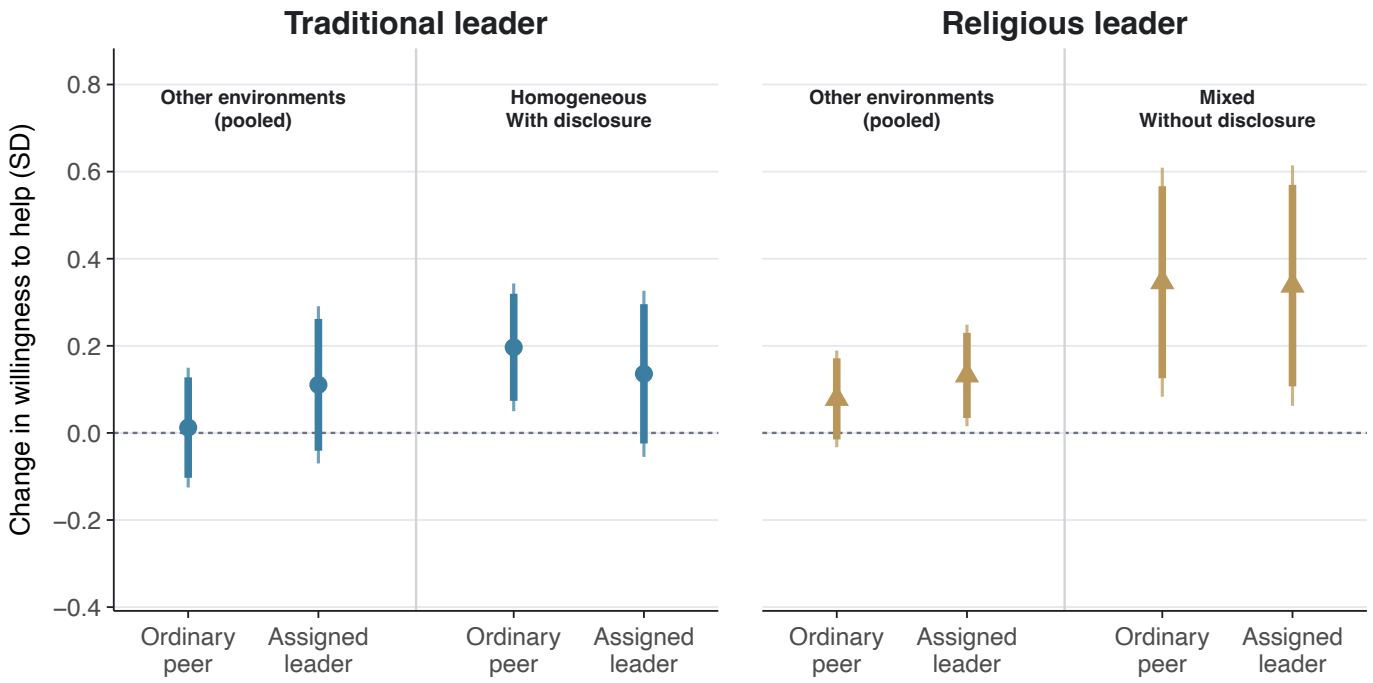


Figure 10: SUCCESSFUL MOBILIZATION STRENGTHENS DIFFERENT RELATIONSHIPS WITHIN THE GROUP

Notes: Before the first discussion and again at the start of Meeting 2, residents reported how much they would help a named group member. The figure shows how leader assignment changes willingness to help over time. The left panel reports traditional leaders and the right panel religious leaders. Within each panel, the right block isolates the environment where that leader raises participation; the left block pools the other three environments. The named person is either an ordinary peer or the assigned leader. Blue circles report traditional leader effects and gold triangles religious leader effects. Effects are measured in baseline standard deviations of the help measure; thick and thin lines are 90 and 95 percent confidence intervals. Section 6 discusses the figure. Appendix Figure A-6 adds expected help; Appendix Tables A-46 and A-47 report all outcomes and environments.

A Supporting Analysis

A.1 Context and Leader Characteristics

Table A-1 – LEADER COUNTS AND RECRUITMENT POPULATION SHARES

	Leader counts		Population
	Religious leaders	Traditional leaders	Share (%)
Panel A. Origin territories			
Dibaya	12 (42.9)	14 (50.0)	36.8
Demba	9 (32.1)	7 (25.0)	25.9
Kazumba	2 (7.1)	5 (17.9)	18.2
Dimbelenge	3 (10.7)	1 (3.6)	5.7
Kananga	1 (3.6)	1 (3.6)	5.3
Other / Not Reported	1 (3.6)	0 (0.0)	8.2
Total	28 (100.0)	28 (100.0)	100.0
Panel B. Main denominations			
Pentecostal, autonomous	5 (17.9)	6 (21.4)	22.8
Pentecostal, Branham	3 (10.7)	3 (10.7)	17.7
Catholic	0 (0.0)	4 (14.3)	11.8
Neo-Apostolic	4 (14.3)	4 (14.3)	10.8
Pentecostal, network	4 (14.3)	0 (0.0)	9.1
Kimbanguist	7 (25.0)	2 (7.1)	6.7
Traditional religion	1 (3.6)	1 (3.6)	5.3
Adventist	0 (0.0)	2 (7.1)	4.1
Protestant/Presbyterian	2 (7.1)	2 (7.1)	4.0
Other / not reported	2 (7.1)	4 (14.3)	7.8
Total	28 (100.0)	28 (100.0)	100.0

Notes: Unit of observation is an individual in the full recruitment sample. The sample is restricted to neighborhoods in final assignment waves 1–7. Religious and traditional leader columns report counts, with within-column percentages in parentheses, for unique person-level leaders assigned in the final assignment files using rows with leader_religious or leader_traditional equal to one. Population shares are percentages of recruitment respondents in these neighborhoods with usable neighborhood information ($N = 2,795$). Territory is the respondent's reported origin territory. Denomination uses the coded religion/denomination item rather than the open-text church name. Rows with less than 4 percent of the recruitment population are pooled in Other / not reported.

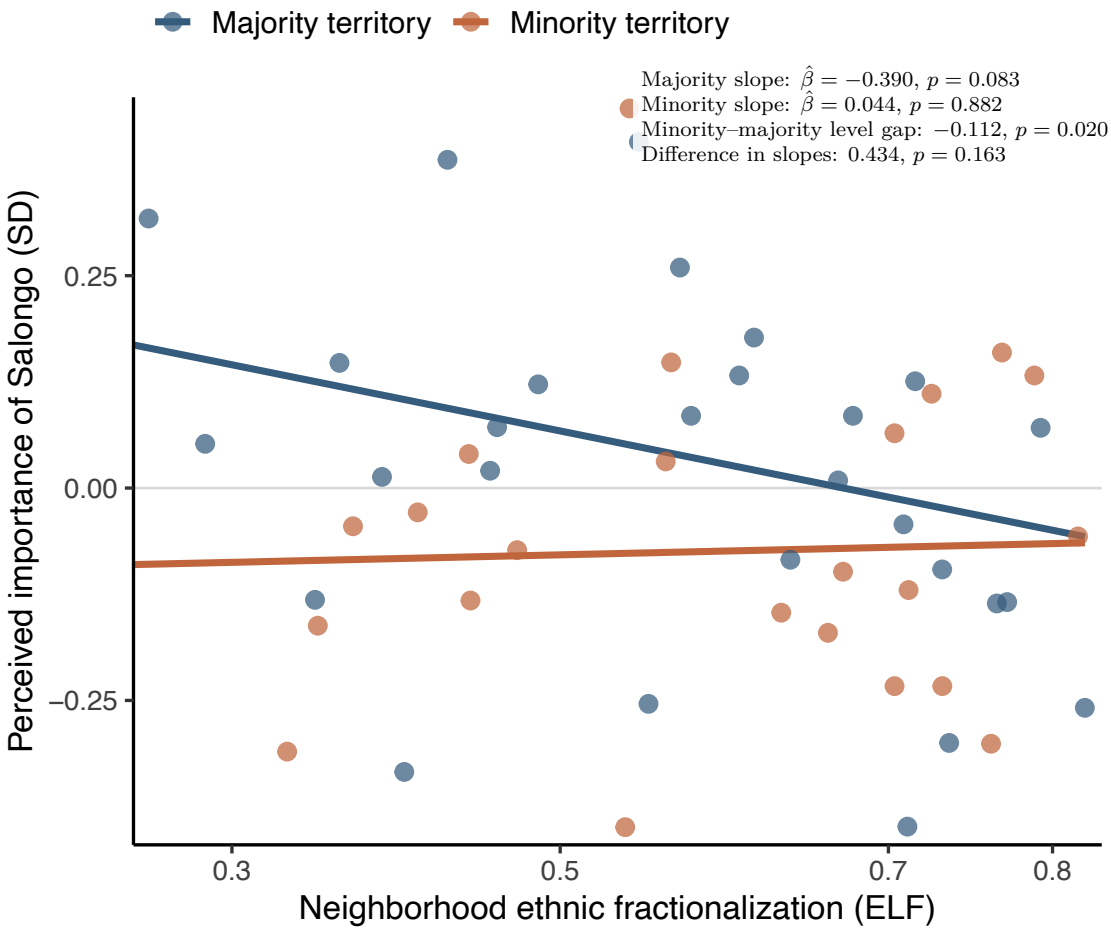


Figure A-1: Neighborhood ethnic fractionalization and perceived importance of *salongo*

Notes: The sample contains 2,791 recruitment-survey respondents in the 28 nonpilot neighborhoods observed through wave 7. Neighborhood ethnic fractionalization is the leave-one-out territory-based ELF, $1 - \sum_g s_g^2$, computed from all respondents in the frozen wave-7 recruitment roster with nonmissing neighborhood and territory. The outcome is the respondent's perceived importance of *salongo*, standardized once over the full estimation sample. The question reads, "How important do people in your community think it is to participate in Salongo?" Responses use a five-point ordered scale anchored at 1, "They think it is not important at all," and 5, "They think it is very important"; values 2–4 are unlabeled intermediate points. Minority status indicates that the respondent's territory is not the modal territory in the neighborhood. The fitted lines come from one individual-level regression of standardized *salongo* importance on mean-centered ELF, minority status, and their interaction, with no additional controls. Standard errors and the displayed tests are clustered by neighborhood. Points are neighborhood-by-status outcome means; the lines are fitted values from the individual-level model. The level gap is evaluated at mean ELF.

Table A-2 – TERRITORY-OF-ORIGIN COMPOSITION OF RECOVERED CHURCH CONGREGATIONS

	Full recruitment roster		Randomized study sample	
	Exact normalized	Strict fuzzy	Exact normalized	Strict fuzzy
Recovered church clusters	66	71	10	12
Observed congregants in recovered clusters	513	555	67	79
Church clusters spanning multiple origin territories	60	64	10	12
Share of church clusters spanning multiple territories	90.9%	90.1%	100.0%	100.0%
Share of congregants in those church clusters	91.6%	91.4%	100.0%	100.0%
Same-territory pairs within church clusters	48.2%	48.4%	53.2%	55.4%
Expected same-territory pairs under no sorting	48.8%	48.5%	53.6%	55.2%
Observed minus expected same-territory pairs	-0.6%	-0.1%	-0.4%	0.2%
Permutation p-value: positive territorial sorting	0.696	0.570	0.566	0.485

Notes: Following the experimental design, ethnic identity is measured using respondents' reported territory of origin; two respondents are coethnic when their normalized territories match exactly. The analysis uses Christian respondents with a valid territory, neighborhood, and free-text church name in the cumulative waves 1–7 recruitment roster. Church names are linked only within neighborhood-by-denomination blocks. The table retains clusters with at least five observed congregants. The no-sorting benchmark reallocates territories within blocks while preserving recovered church sizes; the one-sided permutation test uses 999 draws. Cluster size is the number observed in recruitment, not total congregation size. Neighborhood blocking can split congregations drawing members from multiple neighborhoods.

Table A-3 – LEADER SELECTION AND INSTITUTIONAL EMBEDDEDNESS

	Observations (1)	Citizen mean (2)	Coeff. on religious leader (3)	Coeff. on traditional leader (4)	p-value of diff., columns 3–4 (5)
Panel A. Demographic and economic selection					
Female	2,681	0.603	**-0.590**	**-0.448**	**0.043**
			[**0.000**]	[**0.000**]	
Age	2,681	34.098	**13.289**	**22.253**	**0.035**
			[**0.000**]	[**0.000**]	
Education (yrs)	2,681	9.531	**1.765**	**1.408**	0.662
			[**0.001**]	[**0.020**]	
Non-clergy salaried job	2,681	0.037	0.033	0.105	0.424
			[0.519]	[0.126]	
Government job	2,681	0.006	0.065	0.030	0.573
			[0.198]	[0.416]	
Log income	1,354	11.558	0.106	**0.417**	0.499
			[0.805]	[**0.007**]	
Kananga tenure	1,298	33.312	**13.644**	**21.930**	0.050
			[**0.000**]	[**0.000**]	
Avenue tenure	1,327	15.032	**12.880**	**12.487**	0.937
			[**0.002**]	[**0.002**]	
Panel B. Institutional embeddedness					
Traditional authority and local embeddedness					
Any traditional role	1,362	0.047	**0.346**	**0.953**	**0.000**
			[**0.001**]	[**0.000**]	
Traditional head role	1,362	0.026	**0.260**	**0.617**	**0.005**
			[**0.006**]	[**0.000**]	
Secrets network: same territory	1,362	0.295	0.073	**0.163**	0.459
			[0.360]	[**0.039**]	
Material network: same territory	1,362	0.425	0.037	**0.134**	0.311
			[0.632]	[**0.043**]	
Planned salongo hours	1,350	16.807	**6.837**	**10.051**	0.296
			[**0.036**]	[**0.000**]	
Religious authority and church embeddedness					
Any religious role	1,362	0.139	**0.862**	0.147	**0.000**
			[**0.000**]	[0.090]	
Religious head role	1,362	0.053	**0.662**	**0.197**	**0.001**
			[**0.000**]	[**0.024**]	
Secrets network: same church	1,362	0.177	0.049	-0.004	0.601
			[0.471]	[0.956]	
Material network: same church	1,362	0.223	0.091	**-0.128**	**0.008**
			[0.126]	[**0.006**]	
Sunday attendance	1,362	3.466	**0.319**	0.176	0.381
			[**0.034**]	[0.113]	
Weekday attendance	1,362	1.590	**0.877**	**1.055**	0.742
			[**0.030**]	[**0.002**]	
Log religious contribution	1,317	6.244	**1.482**	0.882	0.296
			[**0.000**]	[0.089]	
Knowledge of local authorities					
Knows authorities	1,362	0.292	**0.315**	**0.434**	0.087
			[**0.000**]	[**0.000**]	
Knows quartier chief	1,362	0.309	**0.298**	**0.477**	0.057
			[**0.001**]	[**0.000**]	
Knows locality chief	1,362	0.352	**0.256**	**0.470**	0.083
			[**0.008**]	[**0.000**]	
Knows chief phone	1,362	0.071	**0.322**	**0.358**	0.802
			[**0.001**]	[**0.001**]	
Chief help index	1,362	1.499	0.170	**0.705**	0.067
			[0.491]	[**0.003**]	
Panel C. Psychological traits					
Standard psychological measures					
Big Five Inventory (main index)	1,362	-0.005	0.019	**0.226**	0.086
			[0.838]	[**0.025**]	
Extraversion	1,358	3.210	-0.013	0.112	0.346
			[0.884]	[0.298]	
Agreeableness	1,361	3.844	0.030	0.120	0.593
			[0.790]	[0.255]	
Conscientiousness	1,360	3.905	0.023	**0.273**	0.085
			[0.846]	[**0.004**]	
Neuroticism	1,362	2.872	-0.104	-0.175	0.659
			[0.464]	[0.116]	
Openness	1,362	3.192	-0.085	0.058	0.476
			[0.497]	[0.733]	
Emotional intelligence	1,362	4.296	-0.014	0.007	0.803
			[0.870]	[0.936]	
Locus of control	1,362	4.069	-0.140	0.324	0.091
			[0.612]	[0.091]	
Social desirability	1,362	0.560	**0.055**	0.025	0.322
			[**0.022**]	[0.278]	
Religious psychological measures					
Positive RCOPE	1,362	3.794	**0.106**	-0.073	0.073
			[**0.012**]	[0.448]	
Negative RCOPE	1,362	3.540	**-0.605**	-0.162	0.115
			[**0.005**]	[0.249]	
Intrinsic religiosity	1,362	3.326	0.109	0.009	0.297
			[0.077]	[0.888]	
Extrinsic religiosity: personal	1,358	4.416	-0.060	-0.024	0.839
			[0.586]	[0.863]	
Extrinsic religiosity: social	1,362	2.678	-0.320	0.133	0.079
			[0.094]	[0.491]	

Notes: Unit of observation is respondent. Each row reports a separate regression of the listed characteristic on religious-leader and traditional-leader indicators with locality fixed effects; ordinary citizens are the omitted group. Locality-clustered p-values are reported in square brackets. Column 5 reports the p-value for equality of the two leader coefficients. Leaders are coded from selected meeting/panel leader IDs. Demographic job rows use the recruitment survey; the remaining rows use the capped final baseline sample. Network shares code missing network counts as zero and code shares as zero for respondents with a zero-size network.

Table A-4 – SOCIAL PROXIMITY AND PERCEIVED AUTHORITY

Outcome	N	Non-leader		Traditional leader		Religious leader	
		Different ethnicity mean	Same-ethnicity premium	Different ethnicity	Same ethnicity	Different ethnicity	Same ethnicity
Panel A. Initial Social Cohesion and Pre-Existing Ties							
Know							
Know other person	2,894	2.938	**0.131**	**0.383**	**0.466**	**0.294**	**0.491**
			[0.042]	[0.000]	[0.000]	[0.000]	[0.000]
Pre-existing ties							
Same church	2,894	0.024	0.019	0.003	0.039	0.036	0.041
			[0.061]	[0.882]	[0.071]	[0.056]	[0.064]
Same groupement	2,894	0.000	**0.057**	-0.003	**0.087**	-0.001	**0.048**
			[0.000]	[0.402]	[0.000]	[0.831]	[0.005]
Same village	2,894	0.000	**0.046**	-0.003	**0.060**	0.001	**0.048**
			[0.000]	[0.321]	[0.002]	[0.760]	[0.005]
Close family	2,894	0.010	0.013	-0.002	0.024	0.002	0.013
			[0.076]	[0.806]	[0.176]	[0.799]	[0.323]
Extended family	2,894	0.017	0.020	0.008	**0.050**	0.001	0.033
			[0.060]	[0.608]	[0.024]	[0.924]	[0.093]
Reciprocity							
Mutual sickness support	2,894	1.157	**0.168**	**0.309**	**0.310**	0.163	**0.242**
			[0.042]	[0.005]	[0.001]	[0.111]	[0.038]
Willing to help them	2,894	1.268	**0.168**	0.179	**0.241**	0.103	0.141
			[0.033]	[0.077]	[0.017]	[0.304]	[0.209]
Expected help from them	2,894	1.056	0.168	**0.438**	**0.379**	0.224	**0.343**
			[0.094]	[0.002]	[0.003]	[0.058]	[0.011]
Panel B. Opinions and Authority							
Motivates respondent	2,894	4.041	0.112	0.173	**0.393**	**0.239**	**0.329**
			[0.057]	[0.051]	[0.000]	[0.001]	[0.000]
Moral example	2,894	4.119	**0.160**	0.167	**0.352**	**0.243**	**0.293**
			[0.005]	[0.056]	[0.000]	[0.000]	[0.000]
Trustworthy	2,894	3.845	0.091	0.118	**0.344**	**0.254**	**0.321**
			[0.138]	[0.158]	[0.000]	[0.000]	[0.000]
Hardworking	2,894	4.043	0.046	0.119	**0.340**	**0.301**	**0.268**
			[0.415]	[0.215]	[0.000]	[0.000]	[0.001]
Seen as authority	2,894	3.586	0.084	**0.212**	**0.253**	0.117	**0.210**
			[0.134]	[0.002]	[0.000]	[0.092]	[0.016]
Panel C. Participation and Material Expectations							
Trust they will join	2,894	3.160	0.098	**0.224**	**0.330**	**0.240**	**0.246**
			[0.067]	[0.004]	[0.000]	[0.000]	[0.004]
Expected money contribution	2,485	0.109	-0.054	**1.056**	**0.698**	**0.939**	**1.209**
			[0.807]	[0.002]	[0.014]	[0.001]	[0.001]
Expected work days	2,691	3.253	0.071	**0.485**	**0.580**	0.271	0.349
			[0.537]	[0.002]	[0.000]	[0.055]	[0.059]

Notes: Unit of observation is a respondent–referenced participant dyad. The omitted group is a non-leader respondent evaluating a non-leader participant from a different ethnicity. The first data column reports that comparison-group mean; all other columns report differences relative to it. p-values are in square brackets below coefficient estimates. Bold coefficients have $p < 0.05$. All regressions include neighborhood fixed effects and selected respondent-side controls.

A.2 Implementation Checks

Table A-5 – ASSIGNMENT BALANCE AND PRETREATMENT PARTICIPATION

	No leader	Leader − no leader, within environment										
Characteristic	$M0$ mean	$M1 - M0$	$H0 - M0$	$H1 - M0$	$T - M0$	$R - M0$	$T - M1$	$R - M1$	$T - H0$	$R - H0$	$T - H1$	$R - H1$	N
Panel A. Characteristics used in rerandomization													
Female	0.565	-0.043	-0.033	-0.022	-0.027	0.021	0.076	-0.007	-0.006	0.017	0.007	-0.005	1,040
	(0.057)	(0.071)	(0.080)	(0.072)	(0.066)	(0.077)	(0.073)	(0.064)	(0.082)	(0.078)	(0.065)	(0.072)	
Predicted property value (USD per cubic meter)	2444.612	-167.307	-75.311	113.918	26.979	-214.419	64.212	624.430	120.720	324.920	-293.164	-290.367	968
	(280.312)	(313.857)	(317.962)	(377.632)	(350.713)	(278.422)	(375.699)	(403.680)	(332.820)	(517.079)	(438.059)	(433.837)	
Community role	0.783	-0.011	0.000	0.054	0.038	0.002	-0.046	-0.058	-0.010	-0.022	-0.040	-0.076	1,040
	(0.049)	(0.058)	(0.055)	(0.053)	(0.056)	(0.059)	(0.062)	(0.066)	(0.052)	(0.051)	(0.046)	(0.054)	
Age	34.554	-1.185	-0.293	-0.815	-0.354	-0.711	1.105	0.081	0.499	2.464	1.581	-0.193	1,040
	(1.770)	(1.877)	(1.956)	(2.006)	(2.334)	(2.238)	(2.100)	(1.794)	(2.069)	(1.760)	(2.163)	(1.771)	
Education (years)	9.511	0.130	-0.130	0.402	0.638	0.007	-0.135	0.532	0.352	0.507	-0.157	-0.538	1,040
	(0.353)	(0.480)	(0.494)	(0.514)	(0.556)	(0.506)	(0.491)	(0.445)	(0.457)	(0.462)	(0.482)	(0.455)	
Government job	0.000	0.011	0.011	-0.000	0.012	-0.000	-0.011	-0.011	-0.011	0.001	0.012	-0.000	1,040
	(0.000)	(0.010)	(0.010)	(0.003)	(0.011)	(0.003)	(0.011)	(0.011)	(0.010)	(0.015)	(0.011)	(0.003)	
Salaried job	0.043	-0.000	-0.000	-0.000	-0.006	-0.018	-0.006	0.030	0.006	0.053	0.006	0.030	1,040
	(0.021)	(0.032)	(0.030)	(0.028)	(0.028)	(0.025)	(0.031)	(0.034)	(0.030)	(0.040)	(0.030)	(0.034)	
Mission church	0.174	-0.011	0.022	0.054	-0.005	-0.005	-0.018	-0.006	0.057	0.009	-0.059	0.024	1,040
	(0.050)	(0.060)	(0.064)	(0.064)	(0.064)	(0.070)	(0.054)	(0.048)	(0.064)	(0.062)	(0.057)	(0.064)	
Revival church	0.489	0.033	0.098	-0.033	-0.024	0.107	0.003	-0.021	-0.075	-0.098	-0.016	0.008	1,040
	(0.056)	(0.077)	(0.076)	(0.076)	(0.079)	(0.085)	(0.064)	(0.074)	(0.069)	(0.073)	(0.063)	(0.062)	
Reported prior salongo participation	0.034	-0.134	-0.098	0.048	0.113	-0.165	0.019	0.217	0.161	-0.031	-0.247**	0.099	1,040
	(0.067)	(0.124)	(0.117)	(0.125)	(0.106)	(0.124)	(0.150)	(0.145)	(0.155)	(0.126)	(0.119)	(0.172)	
Salongo contribution index	0.145	-0.224	-0.319**	-0.032	-0.053	-0.112	0.174	0.088	0.233	0.252*	-0.225*	-0.226	1,040
	(0.123)	(0.147)	(0.136)	(0.164)	(0.164)	(0.171)	(0.131)	(0.130)	(0.142)	(0.143)	(0.135)	(0.141)	
Biometric support index	0.040	-0.075	0.033	0.111	0.028	0.112	0.120	0.121	-0.107	0.047	-0.268*	-0.040	1,040
	(0.120)	(0.146)	(0.139)	(0.136)	(0.152)	(0.152)	(0.142)	(0.132)	(0.122)	(0.152)	(0.146)	(0.144)	
Household size	8.380	-0.815	-1.130*	-0.228	-0.200	-1.212**	0.020	1.305**	0.394	0.835	1.028*	0.349	1,040
	(0.447)	(0.552)	(0.575)	(0.546)	(0.857)	(0.574)	(0.551)	(0.521)	(0.528)	(0.632)	(0.557)	(0.634)	
Adult share	0.426	0.041	0.027	0.002	0.039	0.025	-0.056	-0.033	0.028	0.018	0.003	0.032	1,039
	(0.017)	(0.035)	(0.032)	(0.026)	(0.029)	(0.029)	(0.037)	(0.037)	(0.034)	(0.035)	(0.032)	(0.028)	
Distance to centroid (m)	233.250	-1.463	1.625	-7.836	-4.202	5.058	-5.825	-18.727	-15.452	-1.795	5.550	7.423	1,033
	(31.416)	(15.191)	(14.124)	(13.777)	(15.463)	(15.604)	(14.104)	(14.445)	(12.589)	(12.635)	(12.018)	(13.108)	
Panel B. Additional baseline-survey characteristics													
Demographic and economic characteristics													
Log income	11.474	-0.162	0.243	-0.020	0.008	0.183	0.277	0.209	-0.209	-0.366*	0.411	0.370	1,024
	(0.207)	(0.262)	(0.181)	(0.258)	(0.244)	(0.186)	(0.283)	(0.272)	(0.209)	(0.194)	(0.250)	(0.248)	
Kananga tenure	34.174	-2.673	-0.957	-1.068	-0.658	-1.348	1.909	1.003	0.641	2.859	2.097	-0.995	1,032
	(1.747)	(1.991)	(2.033)	(2.149)	(2.410)	(2.187)	(2.192)	(1.939)	(2.286)	(1.778)	(2.320)	(1.978)	
Avenue tenure	17.272	-3.351*	1.168	-1.778	-0.400	-2.469	2.079	1.358	-5.445***	-3.081	1.596	-0.589	1,012
	(1.690)	(1.791)	(2.177)	(2.224)	(2.131)	(1.785)	(1.733)	(1.873)	(2.064)	(2.242)	(2.029)	(2.172)	

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Assignment Balance and Pretreatment Participation (continued)

	No leader				Leader − no leader, within environment								
Characteristic	$M0$ mean	$M1$ $-M0$	$H0$ $-M0$	$H1$ $-M0$	T $M0$	R $M0$	T $M1$	R $M1$	T $H0$	R $H0$	T $H1$	R $H1$	N
Traditional authority and local embeddedness													
Any traditional role	0.098	-0.042	-0.043	-0.053	-0.037	-0.050	-0.043	0.052	0.019	0.030	-0.008	-0.019	1,032
	(0.031)	(0.037)	(0.036)	(0.035)	(0.039)	(0.037)	(0.026)	(0.041)	(0.033)	(0.037)	(0.028)	(0.026)	
Traditional head role	0.065	-0.043	-0.043	-0.043	-0.016	-0.064**	-0.009	0.038	-0.009	0.027	-0.009	-0.021	1,032
	(0.029)	(0.030)	(0.030)	(0.031)	(0.034)	(0.027)	(0.019)	(0.028)	(0.019)	(0.031)	(0.019)	(0.016)	
Secrets network: same territory	0.286	-0.065	0.075	0.079	-0.069	-0.053	-0.014	-0.003	0.053	-0.028	0.020	-0.033	1,032
	(0.054)	(0.060)	(0.070)	(0.062)	(0.064)	(0.062)	(0.056)	(0.053)	(0.080)	(0.070)	(0.064)	(0.062)	
Material network: same territory	0.383	-0.101	0.148***	0.176***	-0.074	-0.014	0.077	0.036	0.097*	-0.034	-0.058	0.012	1,032
	(0.051)	(0.064)	(0.056)	(0.064)	(0.057)	(0.065)	(0.065)	(0.061)	(0.055)	(0.060)	(0.060)	(0.070)	
Planned salongo hours	17.293	0.224	-1.174	-0.203	-0.554	-0.812	1.282	-0.365	0.697	0.184	1.279	-1.201	1,022
	(1.704)	(1.706)	(1.391)	(1.588)	(1.873)	(1.688)	(2.358)	(1.987)	(1.623)	(1.403)	(1.632)	(1.651)	
Religious authority and church embeddedness													
Any religious role	0.391	-0.007	0.033	-0.062	-0.042	-0.022	-0.120*	-0.015	0.001	-0.078	0.059	0.081	1,032
	(0.057)	(0.072)	(0.066)	(0.065)	(0.073)	(0.077)	(0.069)	(0.075)	(0.062)	(0.062)	(0.064)	(0.071)	
Religious head role	0.098	-0.043	-0.043	-0.043	-0.025	-0.062*	-0.055**	-0.007	0.019	0.017	-0.018	0.005	1,032
	(0.027)	(0.034)	(0.034)	(0.034)	(0.040)	(0.032)	(0.024)	(0.030)	(0.034)	(0.033)	(0.030)	(0.037)	
Secrets network: same church	0.202	-0.021	-0.042	-0.010	-0.067	-0.031	-0.047	0.036	0.012	0.030	0.030	-0.035	1,032
	(0.042)	(0.049)	(0.049)	(0.051)	(0.047)	(0.057)	(0.048)	(0.054)	(0.056)	(0.044)	(0.055)	(0.049)	
Material network: same church	0.213	0.012	-0.019	0.006	-0.043	0.006	0.009	0.031	0.069	0.038	-0.011	0.033	1,032
	(0.037)	(0.050)	(0.044)	(0.044)	(0.048)	(0.050)	(0.056)	(0.053)	(0.052)	(0.050)	(0.046)	(0.045)	
Sunday attendance	3.467	-0.031	-0.152	-0.097	-0.079	0.178	-0.061	0.209	0.003	0.235	0.231	0.282**	1,032
	(0.087)	(0.144)	(0.148)	(0.136)	(0.127)	(0.124)	(0.162)	(0.145)	(0.162)	(0.147)	(0.151)	(0.135)	
Weekday attendance	1.967	-0.286	-0.413**	-0.198	-0.450*	-0.222	-0.183	0.040	-0.067	0.060	-0.210	-0.079	1,032
	(0.187)	(0.211)	(0.171)	(0.224)	(0.240)	(0.218)	(0.236)	(0.215)	(0.211)	(0.167)	(0.271)	(0.237)	
Log religious contribution	6.365	-0.283	0.023	0.000	-0.046	-0.056	-0.225	0.165	0.132	-0.190	-0.006	0.319	999
	(0.307)	(0.417)	(0.370)	(0.350)	(0.437)	(0.347)	(0.495)	(0.456)	(0.354)	(0.393)	(0.415)	(0.348)	
Knowledge of local authorities													
Knows authorities	0.312	0.019	-0.004	-0.044	-0.015	-0.028	-0.048	0.005	0.016	-0.017	0.035	0.000	1,032
	(0.034)	(0.040)	(0.035)	(0.037)	(0.040)	(0.039)	(0.041)	(0.038)	(0.036)	(0.035)	(0.038)	(0.038)	
Knows quartier chief	0.304	0.015	0.043	0.004	0.001	-0.033	0.005	-0.001	-0.016	-0.017	0.042	-0.022	1,032
	(0.057)	(0.051)	(0.047)	(0.047)	(0.054)	(0.058)	(0.060)	(0.054)	(0.053)	(0.050)	(0.056)	(0.052)	
Knows locality chief	0.359	0.026	0.011	-0.084	-0.041	0.040	-0.033	0.026	-0.023	-0.019	0.053	0.059	1,032
	(0.055)	(0.065)	(0.058)	(0.055)	(0.063)	(0.057)	(0.072)	(0.068)	(0.058)	(0.057)	(0.053)	(0.057)	
Knows chief phone	0.076	-0.021	0.011	-0.032	-0.014	-0.038	0.044	0.091**	-0.012	-0.037	0.019	0.067*	1,032
	(0.025)	(0.035)	(0.037)	(0.033)	(0.034)	(0.031)	(0.036)	(0.042)	(0.040)	(0.034)	(0.028)	(0.039)	
Chief help index	1.446	0.264	-0.029	0.018	0.064	0.103	-0.090	-0.327*	0.317*	0.029	-0.138	0.089	1,032
	(0.152)	(0.174)	(0.172)	(0.174)	(0.182)	(0.161)	(0.194)	(0.172)	(0.170)	(0.160)	(0.174)	(0.190)	

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Assignment Balance and Pretreatment Participation (continued)

	No leader				Leader − no leader, within environment								
Characteristic	$M0$ mean	$M1$ $-M0$	$H0$ $-M0$	$H1$ $-M0$	T $M0$	R $M0$	T $M1$	R $M1$	T $H0$	R $H0$	T $H1$	R $H1$	N
Standard psychological measures													
Big Five Inventory (main index)	0.068	-0.040	-0.109	-0.114	-0.115*	-0.051	-0.047	0.009	0.031	0.114	0.037	0.027	1,032
	(0.048)	(0.067)	(0.071)	(0.074)	(0.065)	(0.072)	(0.070)	(0.076)	(0.075)	(0.072)	(0.079)	(0.073)	
Extraversion	3.261	-0.063	-0.053	-0.047	-0.004	-0.047	0.005	-0.014	0.033	0.006	-0.052	-0.035	1,031
	(0.057)	(0.075)	(0.082)	(0.081)	(0.081)	(0.078)	(0.080)	(0.070)	(0.081)	(0.079)	(0.080)	(0.088)	
Agreeableness	3.957	-0.066	-0.114	-0.176*	-0.263***	-0.012	-0.073	0.000	-0.085	0.054	0.145	0.125	1,032
	(0.064)	(0.089)	(0.092)	(0.099)	(0.091)	(0.091)	(0.092)	(0.101)	(0.097)	(0.096)	(0.116)	(0.094)	
Conscientiousness	3.957	0.039	-0.076	-0.099	-0.079	-0.203*	-0.128	0.014	0.014	0.040	0.165*	0.014	1,030
	(0.069)	(0.100)	(0.091)	(0.092)	(0.088)	(0.104)	(0.105)	(0.097)	(0.084)	(0.099)	(0.095)	(0.096)	
Neuroticism	2.924	0.005	-0.027	-0.039	-0.033	-0.138	-0.176*	-0.220**	-0.068	-0.135	0.139	-0.004	1,032
	(0.062)	(0.091)	(0.107)	(0.100)	(0.087)	(0.096)	(0.093)	(0.098)	(0.115)	(0.114)	(0.102)	(0.110)	
Openness	3.266	-0.027	-0.152	-0.099	-0.084	-0.030	-0.145	-0.199*	0.077	0.170	0.013	-0.007	1,032
	(0.078)	(0.105)	(0.112)	(0.098)	(0.104)	(0.098)	(0.109)	(0.116)	(0.119)	(0.126)	(0.090)	(0.095)	
Emotional intelligence	4.353	-0.064	-0.071	0.028	-0.129*	-0.076	-0.017	0.001	-0.013	0.066	-0.116*	-0.044	1,032
	(0.057)	(0.066)	(0.066)	(0.062)	(0.071)	(0.080)	(0.073)	(0.062)	(0.070)	(0.067)	(0.060)	(0.066)	
Locus of control	4.065	-0.083	-0.163	0.191	0.008	0.149	0.238	-0.125	0.082	0.217	-0.246	0.045	1,032
	(0.144)	(0.171)	(0.171)	(0.175)	(0.174)	(0.173)	(0.186)	(0.182)	(0.179)	(0.204)	(0.182)	(0.185)	
Social desirability	0.574	-0.009	-0.028	-0.031	-0.015	-0.043*	-0.000	0.020	0.016	0.031*	0.046**	0.020	1,032
	(0.019)	(0.019)	(0.019)	(0.019)	(0.020)	(0.022)	(0.021)	(0.021)	(0.020)	(0.017)	(0.021)	(0.019)	
Religious psychological measures													
Positive RCOPE	3.850	-0.025	-0.087	-0.080	-0.087	-0.032	-0.000	0.012	0.049	-0.071	0.032	0.089	1,032
	(0.043)	(0.053)	(0.064)	(0.067)	(0.063)	(0.062)	(0.051)	(0.049)	(0.068)	(0.078)	(0.071)	(0.065)	
Negative RCOPE	3.391	0.203**	0.220***	0.140	0.121	0.203**	-0.090	-0.012	-0.093	-0.140	-0.083	0.030	1,032
	(0.070)	(0.083)	(0.084)	(0.089)	(0.101)	(0.088)	(0.081)	(0.088)	(0.099)	(0.088)	(0.100)	(0.095)	
Intrinsic religiosity	3.376	-0.046	-0.036	-0.046	-0.034	-0.002	0.005	0.002	-0.078*	-0.023	0.027	-0.053	1,032
	(0.022)	(0.037)	(0.036)	(0.038)	(0.043)	(0.033)	(0.038)	(0.039)	(0.042)	(0.045)	(0.048)	(0.045)	
Extrinsic religiosity: personal	4.446	0.026	-0.043	0.136*	-0.188**	-0.019	-0.040	-0.092	-0.036	-0.035	-0.101	-0.179**	1,030
	(0.060)	(0.081)	(0.079)	(0.074)	(0.091)	(0.092)	(0.090)	(0.080)	(0.083)	(0.094)	(0.084)	(0.086)	
Extrinsic religiosity: social	2.743	-0.139	0.022	-0.157	-0.193	-0.069	0.056	-0.081	-0.111	-0.206	0.166	0.075	1,032
	(0.118)	(0.125)	(0.126)	(0.136)	(0.127)	(0.134)	(0.136)	(0.137)	(0.121)	(0.126)	(0.148)	(0.150)	
Panel C. Post-assignment, pre-intervention participation													
Participation rate over six common pre-treatment days	0.404	-0.069*	-0.029	-0.009	-0.126***	-0.073**	0.002	0.040	-0.022	0.018	-0.085**	-0.068*	1,040
	(0.057)	(0.038)	(0.042)	(0.034)	(0.040)	(0.036)	(0.043)	(0.046)	(0.040)	(0.043)	(0.040)	(0.040)	

Notes: This table checks whether the randomized groups contained similar residents before the first meeting. Panel A covers the characteristics used to form the groups, Panel B adds measures from the baseline survey, and Panel C reports administrative participation measured after assignment but before residents learned their treatment. The first column is the average for no-leader residents in mixed groups without disclosure ($M0$). The next three columns compare the other no-leader environments with that group. The final eight columns compare chief and pastor assignments with no leader in the same environment. M and H mean mixed and homogeneous groups; 0 and 1 mean group and individual disclosure. Comparisons are made within neighborhood, and standard errors in parentheses allow for residents assigned to the same group. The table is discussed in Section 3; Figure 2 summarizes the design. Stars use unadjusted two-sided p-values. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A-6 – ROSTER FORMATION, REPLACEMENTS, AND COMPOSITION FIDELITY

Leader	Composition	Disclosure	Assigned residents	Replacement share	Mean territories	Final chief-territory share	Composition fidelity
No leader	Mixed	Without disclosure	92	21.7	3.04	22.8	96.4
		With disclosure	92	18.5	3.07	22.8	100.0
	Homogeneous	Without disclosure	92	17.4	1.07	97.8	92.9
		With disclosure	92	22.8	1.07	97.8	92.6
Traditional	Mixed	Without disclosure	84	26.2	2.61	23.8	96.4
		With disclosure	84	27.4	2.82	28.6	100.0
	Homogeneous	Without disclosure	84	21.4	1.07	96.4	92.9
		With disclosure	84	25.0	1.11	95.2	89.3
Religious	Mixed	Without disclosure	84	29.8	2.79	26.2	96.4
		With disclosure	84	21.4	2.82	26.2	100.0
	Homogeneous	Without disclosure	84	15.5	1.07	97.6	92.9
		With disclosure	84	20.2	1.18	94.0	85.7
Total			1,040	22.2	1.98	60.7	94.6

Notes: This table checks whether the planned groups were formed as intended. "Replacement share" is the percentage of residents whose final roster entry differs from the original assignment. "Mean territories" counts the territorial origins represented among ordinary residents. "Final chief-territory share" is the percentage from the chief's territory. Composition is correct when every observed resident in a homogeneous group comes from that territory and a mixed group contains at least two territories. Territory is observed for 335 of 336 groups; all shares are percentages. No-leader cells contain 92 residents rather than 84 because Waves 1–2 used a different fourth ordinary resident in each no-leader team, and those residents remain in the analysis sample. From Wave 3 onward, one person filled that role in all four no-leader teams and was excluded from the analysis, just like the repeated chief and pastor. The table is discussed in Section 3; Figure 2 shows where these groups enter the experiment.

Table A-7 – TREATMENT DELIVERY AT THE ACCOUNTABILITY MEETINGS

Leader	Composition	Disclosure	Assigned residents	Resident attendance	Realized group size	Full-group share	Leader attendance	M1 announcement verified	M2 record verified
No leader	Mixed	Without disclosure	92	95.7	3.82	82.1	–	96.4	100.0
		With disclosure	92	95.7	3.75	75.0	–	100.0	100.0
	Homogeneous	Without disclosure	92	97.8	3.82	85.7	–	100.0	100.0
		With disclosure	92	91.3	3.64	64.3	–	100.0	100.0
Traditional	Mixed	Without disclosure	84	94.0	3.82	85.7	100.0	100.0	100.0
		With disclosure	84	96.4	3.86	85.7	96.4	96.4	100.0
	Homogeneous	Without disclosure	84	92.9	3.79	78.6	100.0	100.0	100.0
		With disclosure	84	94.0	3.82	82.1	100.0	100.0	100.0
Religious	Mixed	Without disclosure	84	95.2	3.86	89.3	100.0	100.0	100.0
		With disclosure	84	96.4	3.89	89.3	100.0	100.0	100.0
	Homogeneous	Without disclosure	84	96.4	3.89	89.3	100.0	100.0	100.0
		With disclosure	84	94.0	3.82	82.1	100.0	100.0	100.0
Total			1,040	95.0	3.82	82.4	99.6	99.4	100.0

Notes: This table checks whether residents, leaders, and information arrived at the meetings as planned. Resident attendance refers to ordinary residents on the analysis roster. Realized group size counts everyone scheduled for the team, including the leader or no-leader participant; "full group" means that all four attended. Leader attendance verifies that the assigned chief or pastor was present. The last two columns verify that the assigned information was announced at Meeting 1 and that the team's performance record was shown at Meeting 2. Assignment alone does not count as delivery. Attendance and verification are percentages; group size is a count. No-leader cells contain 92 residents rather than 84 because Waves 1–2 used a different fourth ordinary resident in each no-leader team, and those residents remain in the analysis sample. From Wave 3 onward, the same person filled that role in all four teams and was excluded, like the repeated leaders. The table is discussed in Section 3; Figure 2 shows the experimental cells.

Table A-8 – ADMINISTRATIVE, SURVEY, AND AUDIO COVERAGE

Leader	Composition	Disclosure	Assigned residents	Baseline survey	Meeting 1 usable	Meeting 2 usable	Endline survey	Audio recorded	Administrative outcome
No leader	Mixed	Without disclosure	92	100.0	95.7	93.5	88.0	89.3	100.0
		With disclosure	92	98.9	95.7	90.2	95.7	57.1	100.0
	Homogeneous	Without disclosure	92	100.0	97.8	94.6	93.5	78.6	100.0
		With disclosure	92	98.9	91.3	92.4	97.8	64.3	100.0
Traditional	Mixed	Without disclosure	84	100.0	94.0	92.9	94.0	92.9	100.0
		With disclosure	84	98.8	96.4	90.5	92.9	60.7	100.0
	Homogeneous	Without disclosure	84	98.8	92.9	85.7	94.0	89.3	100.0
		With disclosure	84	98.8	94.0	92.9	97.6	60.7	100.0
Religious	Mixed	Without disclosure	84	100.0	95.2	89.3	85.7	89.3	100.0
		With disclosure	84	100.0	96.4	94.0	95.2	64.3	100.0
	Homogeneous	Without disclosure	84	100.0	96.4	90.5	96.4	100.0	100.0
		With disclosure	84	98.8	94.0	86.9	86.9	50.0	100.0
Total			1,040	99.4	95.0	91.2	93.2	74.7	100.0

Notes: This table shows the share of residents or groups with usable data from each source. The survey columns count usable resident interviews; Meeting 1 and Meeting 2 exclude forms that recorded the resident as unavailable. Audio is the share of randomized groups with a verified recording. Administrative coverage means that the resident has the participation outcome used in the main results. Every covered resident contributes the same six-day window after Meeting 1 through the day of Meeting 2. All entries are percentages. No-leader cells contain 92 residents rather than 84 because Waves 1–2 used a different fourth ordinary resident in each no-leader team, and those residents remain in the analysis sample. From Wave 3 onward, the same person filled that role in all four teams and was excluded, like the repeated leaders. The table is discussed in Section 3; Figure 2 shows the experimental cells.

A.3 Scope of Authority

Table A-9 – LEADER EFFECTS BY GROUP COMPOSITION

	Disclosure pooled		
	Baseline	+ Spillover	+ Gender/wealth
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed	0.72	0.68	0.55
	(2.86)	(2.86)	(2.72)
Homogeneous	4.82*	4.88*	4.93*
	(2.91)	(2.90)	(2.91)
Religious leader			
Mixed	6.29**	6.28**	6.33**
	(2.94)	(2.95)	(2.85)
Homogeneous	0.83	0.83	0.83
	(2.52)	(2.52)	(2.53)
Panel B. Treatment-effect contrasts			
Mixed: T − R	-5.57*	-5.60*	-5.77**
	(2.98)	(3.00)	(2.89)
Homogeneous: T − R	3.98	4.05	4.10
	(2.94)	(2.92)	(2.98)
Δ_C: composition gradient	9.55**	9.65**	9.87**
	(4.20)	(4.20)	(4.16)
Panel C. No-leader reference means and diagnostics			
Mixed	58.33	58.33	58.33
	(1.99)	(1.99)	(1.89)
Homogeneous	56.79	56.79	56.79
	(1.76)	(1.76)	(1.75)
Wald: all no-leader cells equal	p = 0.562	p = 0.562	p = 0.549
Observations	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336
Neighborhoods	28	28	28
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes
25m spillover control	No	Yes	Yes
Gender and wealth-stratum controls	No	No	Yes

Notes: Panel A reports leader effects relative to no leader; Panel B reports direct leader contrasts; Panel C reports no-leader reference means. Leader effects are pooled across disclosure; Δ_C measures the change in the traditional-minus-religious gap from mixed to homogeneous composition. The outcome is average participation on six eligible days after Meeting 1 through the Meeting 2 day, in percentage points. Every column controls for pretreatment participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds the 25-meter spillover indicator; the third also adds gender and wealth strata. Group-clustered standard errors appear in parentheses. Section 4.2 and Figure 3 discuss these estimates. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-10 – LABOR CONTRIBUTION OUTCOMES BY GROUP COMPOSITION

	Leader by composition			
	Participation rate	Attendance days	Total hours	Total effort
Panel A. Leader effects relative to no leader				
Traditional leader				
Mixed	0.007	0.060	-0.284	-0.080
	(0.029)	(0.172)	(0.651)	(0.562)
Homogeneous	0.048*	0.250	1.171*	0.975*
	(0.029)	(0.170)	(0.652)	(0.567)
Religious leader				
Mixed	0.063**	0.394**	0.911	1.108*
	(0.029)	(0.178)	(0.697)	(0.578)
Homogeneous	0.008	0.026	-0.075	0.028
	(0.025)	(0.149)	(0.599)	(0.520)
Panel B. Treatment-effect contrasts				
Mixed: T − R	-0.056*	-0.334*	-1.195*	-1.188**
	(0.030)	(0.180)	(0.689)	(0.588)
	[Holm p = 0.188]	[Holm p = 0.188]	[Holm p = 0.188]	[Holm p = 0.177]
Homogeneous: T − R	0.040	0.224	1.246*	0.946
	(0.029)	(0.170)	(0.645)	(0.580)
	[Holm p = 0.352]	[Holm p = 0.352]	[Holm p = 0.216]	[Holm p = 0.312]
Δ_C	0.096**	0.558**	2.441**	2.134**
	(0.042)	(0.249)	(0.946)	(0.827)
	[Holm p = 0.047]	[Holm p = 0.047]	[Holm p = 0.041]	[Holm p = 0.041]
Panel C. No-leader reference means and diagnostics				
Mixed	0.583	3.446	13.210	11.334
	(0.020)	(0.120)	(0.465)	(0.390)
Homogeneous	0.568	3.364	12.703	10.872
	(0.018)	(0.105)	(0.431)	(0.357)
Wald: homogeneous = mixed	p = 0.562	p = 0.610	p = 0.423	p = 0.383
Observations	1,040	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336	336
Neighborhoods	28	28	28	28
Matched pretreatment outcome control	Yes	Yes	Yes	Yes
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_C. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Participation is the daily rate; attendance is the number of labor days; hours and effort are window totals, with nonattendance coded zero. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-11 – LABOR CONTRIBUTIONS: ASINH HOURS BY GROUP COMPOSITION

	Leader by composition
	Asinh total hours
Panel A. Leader effects relative to no leader	
Traditional leader	
Mixed	-0.002
	(0.119)
Homogeneous	0.192*
	(0.108)
Religious leader	
Mixed	0.179
	(0.116)
Homogeneous	0.043
	(0.095)
Panel B. Treatment-effect contrasts	
Mixed: T − R	-0.181
	(0.122)
	[Holm p = 0.140]
Homogeneous: T − R	0.149
	(0.109)
	[Holm p = 0.174]
Δ_C	0.330**
	(0.164)
	[Holm p = 0.046]
Panel C. No-leader reference means and diagnostics	
Mixed	2.669
	(0.080)
Homogeneous	2.594
	(0.065)
Wald: homogeneous = mixed	p = 0.468
Observations	1,040
Group-neighborhood clusters	336
Neighborhoods	28
Matched pretreatment outcome control	Yes
Neighborhood × disclosure × composition FEs	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_C. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Asinh hours retains zero-valued windows, preserving the full frozen analysis sample. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-12 – CASH AND IN-KIND CONTRIBUTIONS BY GROUP COMPOSITION

	Leader by composition			
	Any cash contribution	Cash total	Any in-kind contribution	In-kind value
Panel A. Leader effects relative to no leader				
Traditional leader				
Mixed	0.030*	14.971	-0.005	-53.318
	(0.017)	(27.229)	(0.032)	(101.388)
Homogeneous	0.013	-1.271	0.007	-1131.884*
	(0.014)	(14.186)	(0.040)	(677.600)
Religious leader				
Mixed	0.006	-7.053	0.053	-6.317
	(0.016)	(20.885)	(0.034)	(99.060)
Homogeneous	0.001	-13.396	0.001	-957.190
	(0.013)	(13.200)	(0.041)	(684.773)
Panel B. Treatment-effect contrasts				
Mixed: T − R	0.024	22.024	-0.058	-47.001
	(0.020)	(27.104)	(0.037)	(112.707)
	[Holm p = 0.702]	[Holm p = 0.834]	[Holm p = 0.481]	[Holm p = 0.834]
Homogeneous: T − R	0.012	12.125	0.007	-174.694
	(0.013)	(11.360)	(0.043)	(486.568)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]
Δ_C	-0.012	-9.898	0.064	-127.693
	(0.024)	(29.392)	(0.057)	(499.382)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]
Panel C. No-leader reference means and diagnostics				
Mixed	0.022	38.315	0.266	543.478
	(0.009)	(15.616)	(0.021)	(61.742)
Homogeneous	0.016	21.739	0.288	1619.022
	(0.010)	(11.417)	(0.026)	(611.892)
Wald: homogeneous = mixed	p = 0.679	p = 0.392	p = 0.513	p = 0.080
Observations	1,040	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336	336
Neighborhoods	28	28	28	28
Matched pretreatment outcome control	Yes	Yes	Yes	Yes
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_C. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Cash and in-kind extensive margins are window indicators; amounts are window totals in Congolese francs (CDF). The source-level identity combined material value = cash + in-kind is validated before estimation. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-13 – CASH AND IN-KIND CONTRIBUTIONS: ASINH VALUES BY GROUP COMPOSITION

	Leader by composition	
	Asinh cash total	Asinh in-kind value
Panel A. Leader effects relative to no leader		
Traditional leader		
Mixed	0.221*	-0.067
	(0.125)	(0.248)
Homogeneous	0.086	-0.059
	(0.097)	(0.316)
Religious leader		
Mixed	0.046	0.419
	(0.115)	(0.266)
Homogeneous	-0.005	-0.051
	(0.094)	(0.329)
Panel B. Treatment-effect contrasts		
Mixed: T − R	0.175	-0.486*
	(0.147)	(0.288)
	[Holm p = 0.235]	[Holm p = 0.185]
Homogeneous: T − R	0.090	-0.009
	(0.091)	(0.339)
	[Holm p = 0.641]	[Holm p = 0.980]
Δ_C	-0.084	0.477
	(0.173)	(0.445)
	[Holm p = 0.626]	[Holm p = 0.567]
Panel C. No-leader reference means and diagnostics		
Mixed	0.161	2.122
	(0.063)	(0.160)
Homogeneous	0.122	2.327
	(0.069)	(0.211)
Wald: homogeneous = mixed	p = 0.682	p = 0.437
Observations	1,040	1,040
Group-neighborhood clusters	336	336
Neighborhoods	28	28
Matched pretreatment outcome control	Yes	Yes
Neighborhood × disclosure × composition FEs	Yes	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_C. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Inverse-hyperbolic-sine specifications retain zero-valued windows. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-14 – RANDOMIZATION INFERENCE BY GROUP COMPOSITION

	Leader by composition		
	Estimate	Clustered SE	RI p
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed	0.72	(2.86)	0.817
Homogeneous	4.82*	(2.91)	0.118
Religious leader			
Mixed	6.29**	(2.94)	0.046
Homogeneous	0.83	(2.52)	0.788
Panel B. Treatment-effect contrasts			
Mixed: T − R	-5.57*	(2.98)	0.089
Homogeneous: T − R	3.98	(2.94)	0.227
Δ_C: composition gradient	9.55**	(4.20)	0.039
Panel C. No-leader reference means and diagnostics			
Mixed	58.33	(1.99)	
Homogeneous	56.79	(1.76)	
Wald: all no-leader cells equal	p = 0.562		
Observations	1,040		
Group-neighborhood clusters	336		
Neighborhoods	28		
NQC fixed effects	Yes		
Pretreatment participation control	Yes		
Formation-stage permutations	5,000		

Notes: The table follows the specification and row structure of the paper's main results table. The outcome is average post-first-meeting salongo participation, in percentage points. All models include the six-day pretreatment participation control and neighborhood × disclosure × composition fixed effects. Panel A compares each leader assignment with no leader in the same fixed-effect cell. Panel B reports the same leader contrasts as the main table; Δ_C is the difference between the Traditional and Religious composition gradients. Panel C reports model-implied no-leader reference means and clustered delta-method tests that the displayed no-leader means are equal. Standard errors are clustered one-way at the group–neighborhood level. RI p-values are two-sided and come from 5,000 conditional replays of the realized participants through the group-formation stage. The replay preserves wave-specific group sizes, randomized filling order, composition and fallback rules, gender × wealth pattern targets, mixed-group territory diversity, and treatment coding; it does not replay respondent sampling or the original balance-selection stage. The replay seed is 20260731. Stars use clustered asymptotic inference: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-15 – ALTERNATIVE CLUSTERING BY GROUP COMPOSITION

	Leader by composition		
	Group	Leader cluster	Neighborhood
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed	0.72	0.72	0.72
	(2.86)	(3.15)	(3.74)
Homogeneous	4.82*	4.82	4.82
	(2.91)	(2.99)	(3.37)
Religious leader			
Mixed	6.29**	6.29**	6.29*
	(2.94)	(3.06)	(3.62)
Homogeneous	0.83	0.83	0.83
	(2.52)	(2.78)	(2.92)
Panel B. Treatment-effect contrasts			
Mixed: T − R	-5.57*	-5.57*	-5.57
	(2.98)	(3.11)	(3.50)
Homogeneous: T − R	3.98	3.98	3.98
	(2.94)	(3.36)	(4.21)
Δ_C: composition gradient	9.55**	9.55**	9.55
	(4.20)	(4.63)	(5.67)
Panel C. No-leader reference means and diagnostics			
Mixed	58.33	58.33	58.33
	(1.99)	(2.19)	(2.21)
Homogeneous	56.79	56.79	56.79
	(1.76)	(1.61)	(1.63)
Wald: all no-leader cells equal	p = 0.562	p = 0.546	p = 0.551
Observations	1,040	1,040	1,040
Clusters	336	84	28
Neighborhoods	28	28	28
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes

Notes: The table reproduces the main-table sample, outcomes, neighborhood × disclosure × composition fixed effects, pretreatment participation control, leader-effect definitions, contrasts, and no-leader diagnostics. Columns differ only in the one-way clustering level. Group clusters are the 336 experimental group–neighborhood cells. Leader-group clusters are the 84 neighborhood-wave × leader-arm cells, each containing the four groups assigned to traditional, religious, or no leader. Neighborhood clustering uses 28 neighborhood-wave assignments. Panel B and Panel C inference uses the clustered delta method. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-16 – LEADER EFFECTS UNDER THE RELIGIOUS-LEADER TERRITORY RESTRICTION

	Leader by composition		
	Baseline	+ Spillover	+ Gender/wealth
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed	0.65	0.60	0.44
	(2.71)	(2.70)	(2.58)
Homogeneous	4.79*	4.89*	4.92*
	(2.88)	(2.87)	(2.88)
Religious leader			
Mixed	10.05***	10.12***	9.39***
	(3.52)	(3.51)	(3.52)
Homogeneous	-1.43	-1.50	-1.27
	(3.33)	(3.31)	(3.34)
Panel B. Treatment-effect contrasts			
Mixed: T − R	-9.40***	-9.52***	-8.96***
	(3.43)	(3.44)	(3.42)
Homogeneous: T − R	6.22*	6.38*	6.19
	(3.74)	(3.71)	(3.75)
Δ_C: composition gradient	15.62***	15.91***	15.14***
	(5.08)	(5.07)	(5.07)
Panel C. No-leader reference means and diagnostics			
Mixed	58.33	58.33	58.33
	(1.92)	(1.90)	(1.83)
Homogeneous	56.79	56.79	56.79
	(1.76)	(1.76)	(1.77)
Wald: homogeneous = mixed	p = 0.555	p = 0.553	p = 0.545
Observations	908	908	908
Group-neighborhood clusters	292	292	292
Neighborhoods	28	28	28
Design fixed effects	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes
25m spillover control	No	Yes	Yes
Gender and wealth-stratum controls	No	No	Yes

Notes: The table repeats the pooled scope specification after excluding the 132 pastor-arm residents in the 11 neighborhoods where the assigned pastor and chief come from different territories. All chief and no-leader observations remain. Panel A reports effects pooled across disclosure, Panel B compares the two leaders, and Panel C reports model-implied no-leader means. Every column controls for pretreatment participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds nearby spillover exposure; the third also adds gender and wealth strata. Group-clustered standard errors are in parentheses. Section 4.2 discusses the territorial comparison. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-17 – LEADERSHIP BY EXAMPLE: LEADER PRETREATMENT PARTICIPATION

	Group-clustered SE			Neighborhood-clustered SE		
	Participation at/below leader-type median	Participation above leader-type median	Difference	Participation at/below leader-type median	Participation above leader-type median	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	0.91	0.51	-0.40	0.91	0.51	-0.40
	(3.59)	(4.00)	(5.05)	(4.26)	(5.50)	(6.50)
Homogeneous	7.96**	2.12	-5.85	7.96	2.12	-5.85
	(3.93)	(3.84)	(5.21)	(5.79)	(3.78)	(6.99)
Religious leader						
Mixed	4.91	8.09**	3.18	4.91	8.09*	3.18
	(3.84)	(4.00)	(5.21)	(4.75)	(4.68)	(6.16)
Homogeneous	3.65	-2.87	-6.52	3.65	-2.87	-6.52
	(3.40)	(3.35)	(4.53)	(4.19)	(4.45)	(6.23)
Panel B. Implied no-leader counterfactuals						
No leader, mixed		56.52			56.52	
		(1.99)			(2.10)	
No leader, homogeneous		54.09			54.09	
		(1.82)			(1.59)	
Observations			1,040			
Group clusters			336			

Notes: Difference is the effect above the median minus the effect at or below it. Panel B reports model-implied no-leader participation in percentage points, averaging over common chief and pastor participant support within each composition with baseline participation at its estimation-sample mean. The characteristic is undefined without a leader, so each block reports one pooled reference rather than separate participation categories among no-leader groups. Entries compare leader effects above and below the type-specific median of leaders' participation during the common week before any meeting. The cutpoints are 0.80 for chiefs and 0.83 for pastors; ties enter the at-or-below-median category. Sunday is excluded and each leader rate uses four to six observed eligible days. The two blocks report the same composition-pooled regression with standard errors clustered by 336 groups or by 28 neighborhoods. The latter allows dependence across groups assigned to the same leader. All 1,040 participants are retained. The regression includes baseline participation, neighborhood-by-composition-by-disclosure fixed effects, and leader-by-composition interactions with the leader participation category. The characteristic is undefined without a leader and has no standalone level term. Section 4.2 discusses the interpretation. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.3.1 Scope of Authority: Resident Characteristics

Tables A-18–A-23 estimate Equation (2), pooling both leader effects and heterogeneity across disclosure within each composition. Resident and group characteristics observed in every assignment arm enter separately by composition. Ratings of the assigned leader enter only through leader-treatment interactions, because the rating is undefined without a leader. Shared territory with the neighborhood chief varies only within mixed groups; homogeneous effects are not split. The group network measure includes each ordinary member in their own confidant network, aggregates valid counts, and places ties at the 80-percent median in the lower category.

Authority and opinion assessments are matched to the assigned leader using the meeting membership records. Missing values for 33 residents are filled with the observed mean among ordinary members of the same group. Three residents in one group lack an observed group mean and are excluded. This gives 1,037 observations in 335 groups for both assessments. Standard errors condition on the filled categories. For these two measures, the lower figure panels report one pooled no-leader reference per composition, repeated across assessment categories; they do not represent low- and high-assessment no-leader subgroups. For characteristics observed in every arm, the reference levels use common chief and pastor participant support within each composition and category.

The first specification includes baseline participation and the design fixed effects. The second adds gender and wealth strata, the corresponding 25-meter spillover measure, and a fixed set of 25 baseline characteristics with at least one unadjusted balance-test p-value below 0.10 in the baseline balance battery. This selection uses pretreatment covariates, not the heterogeneity estimates. The covariates cover Sunday and weekday religious attendance; avenue tenure; log income; the Big Five index, agreeableness, conscientiousness, neuroticism, and openness; support for biometric registration; the chief-help index and knowledge of the chief's telephone number; emotional intelligence; household size; intrinsic and extrinsic-personal religiosity; same-territory shares of confidant and material-support networks; negative religious coping; religious head and other religious roles; the salongo contribution index and prior participation; social desirability; and traditional head roles. Missing covariate values are filled at their estimation-sample means and accompanied by missingness indicators, retaining the same sample across specifications. Tests in this exploratory heterogeneity family are unadjusted for multiple comparisons.

Table A-18 – PERCEIVED SECULAR AUTHORITY OF THE ASSIGNED LEADER

	Baseline participation control			+ Baseline characteristics		
	Score ≤ 4	Score > 4	Difference	Score ≤ 4	Score > 4	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	-2.33	8.25*	10.58*	-1.83	8.67*	10.49*
	(3.44)	(4.75)	(6.02)	(3.36)	(4.46)	(5.57)
Homogeneous	2.90	7.85**	4.95	4.31	6.84*	2.53
	(3.65)	(3.92)	(4.98)	(3.79)	(3.86)	(5.07)
Religious leader						
Mixed	4.68	9.14**	4.45	5.26	8.63**	3.36
	(3.38)	(3.79)	(4.24)	(3.40)	(3.94)	(4.37)
Homogeneous	1.31	-0.08	-1.39	1.61	-0.59	-2.20
	(3.29)	(3.54)	(4.69)	(3.50)	(3.90)	(5.26)
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed, pooled		56.24			56.09	
		(1.93)			(1.93)	
Homogeneous, pooled		54.18			54.01	
		(1.77)			(1.88)	
Observations		1,037			1,037	
Group clusters		335			335	
Groups containing respondents in each category (first / second)						
Chief, mixed		47 / 34			47 / 34	
Chief, homogeneous		53 / 43			53 / 43	
Pastor, mixed		52 / 38			52 / 38	
Pastor, homogeneous		51 / 39			51 / 39	

Notes: Difference subtracts the first category from the second in each block. The columns distinguish pre-discussion secular-authority scores at or below 4 from scores above 4. Assigned-leader targets are verified against meeting membership. Missing scores for 33 residents are filled with their group's observed mean; three residents without a group mean are excluded. No-leader references are pooled by composition because the assessment is undefined without a leader. Leader effects, interactions, and applicable category levels are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover, gender and wealth strata, and 25 baseline characteristics; Appendix Section A.3.1 details their selection and missing-value treatment. Group-clustered standard errors in parentheses condition on the constructed categories. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 4.3 and Figure 4A discuss the estimates.

Table A-19 – OPINION OF THE ASSIGNED LEADER

	Baseline participation control			+ Baseline characteristics		
	Index at/below median	Index above median	Difference	Index at/below median	Index above median	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	-4.05	8.36**	12.41**	-3.83	9.04**	12.87***
	(3.56)	(4.00)	(5.23)	(3.45)	(3.85)	(4.91)
Homogeneous	0.95	8.12**	7.16	3.04	7.32**	4.28
	(4.18)	(3.44)	(5.05)	(4.43)	(3.43)	(5.28)
Religious leader						
Mixed	4.90	7.70*	2.80	5.05	7.80*	2.75
	(3.51)	(3.94)	(4.73)	(3.65)	(3.97)	(4.89)
Homogeneous	2.92	-1.23	-4.14	2.21	-0.49	-2.70
	(3.92)	(3.18)	(5.04)	(4.31)	(3.42)	(5.69)
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed, pooled		56.28			56.14	
		(1.94)			(1.94)	
Homogeneous, pooled		54.20			54.02	
		(1.78)			(1.89)	
Observations		1,037			1,037	
Group clusters		335			335	
Groups containing respondents in each category (first / second)						
Chief, mixed		45 / 39			45 / 39	
Chief, homogeneous		46 / 48			46 / 48	
Pastor, mixed		48 / 46			48 / 46	
Pastor, homogeneous		44 / 47			44 / 47	

Notes: Difference subtracts the first category from the second in each block. The columns distinguish opinion-index scores at or below the median from scores above it. The four-item pre-discussion opinion index measures trustworthiness, hard work, morality, and inspiration. The median opinion-index score is 0.20195; ties enter the first category. Assigned-leader targets are verified against meeting membership. Missing indices for 33 residents are filled with their group's observed mean; three residents without a group mean are excluded. No-leader references are pooled by composition because the assessment is undefined without a leader. Leader effects, interactions, and applicable category levels are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover, gender and wealth strata, and 25 baseline characteristics; Appendix Section A.3.1 details their selection and missing-value treatment. Group-clustered standard errors in parentheses condition on the constructed categories. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 4.3 and Figure 4B discuss the estimates.

Table A-20 – SHARED TERRITORIAL ORIGIN WITH THE NEIGHBORHOOD CHIEF

	Baseline participation control			+ Baseline characteristics		
	Different territory	Same territory	Difference	Different territory	Same territory	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	-1.05	6.09	7.13	0.01	5.07	5.06
	(3.35)	(7.31)	(8.56)	(3.35)	(7.43)	(8.79)
Homogeneous, pooled		4.85*			5.45*	
		(2.91)			(2.93)	
Religious leader						
Mixed	3.41	14.69**	11.27	3.62	14.26*	10.65
	(3.54)	(7.26)	(8.66)	(3.50)	(7.53)	(8.84)
Homogeneous, pooled		0.85			0.80	
		(2.53)			(2.63)	
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed	56.83	55.33	-1.50	55.97	56.82	0.86
	(2.29)	(5.45)	(6.32)	(2.30)	(5.71)	(6.65)
Homogeneous, pooled		54.09			54.05	
		(1.78)			(1.89)	
Observations		1,040			1,040	
Group clusters		336			336	
Groups containing respondents in each category (first / second)						
Chief, mixed		56 / 34			56 / 34	
Chief homogeneous groups (pooled)		56			56	
Pastor, mixed		55 / 36			55 / 36	
Pastor homogeneous groups (pooled)		56			56	

Notes: Difference subtracts the first category from the second in each block. Both categories compare the respondent's territorial origin with the neighborhood chief's origin. The territorial-origin comparison is defined in every assignment arm. Heterogeneity is estimated only in mixed groups; homogeneous effects are pooled. Panel B reports common no-leader reference levels on combined chief and pastor participant support. Leader effects, interactions, and applicable category levels are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover, gender and wealth strata, and 25 baseline characteristics; Appendix Section A.3.1 details their selection and missing-value treatment. Group-clustered standard errors in parentheses condition on the constructed categories. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 4.3 and Figure 5A discuss the estimates.

Table A-21 – GROUP TERRITORIAL NETWORKS AND LEADER EFFECTIVENESS

	Baseline participation control			+ Baseline characteristics		
	Share $\leq$ 80%	Share $>$ 80%	Difference	Share $\leq$ 80%	Share $>$ 80%	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	0.19	2.91	2.72	2.09	0.56	-1.52
	(3.67)	(5.95)	(7.50)	(3.51)	(5.80)	(7.26)
Homogeneous	3.17	6.04*	2.87	2.76	7.13**	4.37
	(5.15)	(3.51)	(6.66)	(5.56)	(3.62)	(7.20)
Religious leader						
Mixed	8.62**	2.02	-6.60	9.84***	0.15	-9.68
	(3.66)	(5.63)	(7.03)	(3.61)	(5.56)	(6.83)
Homogeneous	10.03**	-4.24	-14.28**	9.88**	-4.78	-14.66**
	(3.91)	(3.92)	(6.20)	(4.50)	(4.09)	(6.91)
Panel B. Implied no-leader counterfactuals at each share						
Common no-leader comparison						
Mixed	55.90	57.78	1.88	54.95	58.69	3.74
	(2.49)	(4.38)	(5.42)	(2.39)	(4.39)	(5.37)
Homogeneous	45.95	58.43	12.48***	46.98	57.65	10.67**
	(3.30)	(2.38)	(4.58)	(3.80)	(2.59)	(5.26)
Observations	1,040			1,040		
Group clusters	336			336		
Groups containing respondents in each category (first / second)						
Chief, mixed	43 / 13			43 / 13		
Chief, homogeneous	21 / 35			21 / 35		
Pastor, mixed	36 / 20			36 / 20		
Pastor, homogeneous	30 / 26			30 / 26		

Notes: Difference subtracts the first category from the second in each block. The columns distinguish same-territory network shares at or below 80 percent from shares above 80 percent. The group measure is summed same-territory confidants divided by summed total confidants, adding each ordinary member to their own network. Three members contribute valid counts in 330 groups and two in six groups. Groups without reported confidants have a share of one. The median share is 80 percent; ties enter the first category. Panel B reports common no-leader predictions within each category. Leader effects, interactions, and applicable category levels are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover, gender and wealth strata, and 25 baseline characteristics; Appendix Section A.3.1 details their selection and missing-value treatment. Group-clustered standard errors in parentheses condition on the constructed categories. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 4.3 and Figure 5B discuss the estimates.

Table A-22 – PERSONAL RELIGIOUS ORIENTATION AND LEADER EFFECTIVENESS

	Baseline participation control			+ Baseline characteristics		
	Index at/below median	Index above median	Difference	Index at/below median	Index above median	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	-0.30	-0.14	0.15	0.58	0.59	0.01
	(4.39)	(4.25)	(6.43)	(4.46)	(4.26)	(6.60)
Homogeneous	4.71	2.93	-1.77	5.67	3.33	-2.35
	(4.35)	(4.10)	(6.20)	(4.44)	(4.01)	(6.15)
Religious leader						
Mixed	-0.12	11.96***	12.08*	0.32	11.64***	11.32*
	(4.36)	(4.33)	(6.48)	(4.53)	(4.35)	(6.69)
Homogeneous	0.29	1.83	1.53	1.16	1.45	0.29
	(4.02)	(3.54)	(5.63)	(4.24)	(3.59)	(5.84)
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed	58.89	55.27	-3.62	62.13	51.41	-10.71**
	(3.02)	(2.99)	(4.50)	(3.45)	(3.20)	(5.36)
Homogeneous	56.88	52.09	-4.80	60.17	48.08	-12.09**
	(2.93)	(2.64)	(4.25)	(3.54)	(3.24)	(5.65)
Observations	1,032			1,032		
Group clusters	336			336		
Groups containing respondents in each category (first / second)						
Chief, mixed	49 / 49			49 / 49		
Chief, homogeneous	49 / 45			49 / 45		
Pastor, mixed	49 / 49			49 / 49		
Pastor, homogeneous	51 / 50			51 / 50		

Notes: Difference subtracts the first category from the second in each block. The columns distinguish personal-religiosity index scores at or below the median from scores above it. The index averages standardized intrinsic and extrinsic-personal religiosity. Standardization uses the 1,032 residents with both components observed. The median is -0.10492, with 524 residents below or at it and 508 above. Panel B reports common no-leader predictions within each category. Leader effects, interactions, and applicable category levels are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover, gender and wealth strata, and 25 baseline characteristics; Appendix Section A.3.1 details their selection and missing-value treatment. Group-clustered standard errors in parentheses condition on the constructed categories. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 4.3 and Figure 6A discuss the estimates.

Table A-23 – RELIGION AS SOCIAL CONTACT AND LEADER EFFECTIVENESS

	Baseline participation control			+ Baseline characteristics		
	Score $\leq 2\frac{1}{3}$	Score $> 2\frac{1}{3}$	Difference	Score $\leq 2\frac{1}{3}$	Score $> 2\frac{1}{3}$	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	-4.62	4.98	9.60	-3.02	4.60	7.62
	(4.94)	(4.33)	(7.30)	(4.78)	(4.19)	(6.98)
Homogeneous	2.73	5.18	2.45	3.31	5.45	2.14
	(3.97)	(4.83)	(6.69)	(4.05)	(4.68)	(6.59)
Religious leader						
Mixed	7.10	5.42	-1.68	8.34*	4.57	-3.76
	(4.40)	(4.15)	(6.31)	(4.34)	(4.33)	(6.42)
Homogeneous	3.36	-1.60	-4.96	3.71	-2.21	-5.92
	(3.19)	(4.79)	(6.29)	(3.40)	(4.83)	(6.42)
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed	60.25	53.69	-6.55	59.22	54.22	-5.00
	(3.50)	(2.77)	(4.86)	(3.58)	(2.81)	(5.04)
Homogeneous	53.52	55.31	1.79	52.92	55.79	2.87
	(2.40)	(3.20)	(4.34)	(2.64)	(3.22)	(4.51)
Observations	1,034			1,034		
Group clusters	336			336		
Groups containing respondents in each category (first / second)						
Chief, mixed	46 / 45			46 / 45		
Chief, homogeneous	46 / 46			46 / 46		
Pastor, mixed	43 / 46			43 / 46		
Pastor, homogeneous	47 / 43			47 / 43		

Notes: Difference subtracts the first category from the second in each block. The columns distinguish scores on religious participation for social contact at or below $2\frac{1}{3}$ from scores above it. Panel B reports common no-leader predictions within each category. Leader effects, interactions, and applicable category levels are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover, gender and wealth strata, and 25 baseline characteristics; Appendix Section A.3.1 details their selection and missing-value treatment. Group-clustered standard errors in parentheses condition on the constructed categories. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Section 4.3 and Figure 6B discuss the estimates.

Table A-24 – DENOMINATIONAL TIES AND LEADER EFFECTIVENESS

	Denomination relative to the neighborhood pastor		
	Different denomination	Same denomination	Difference
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed	1.39	-6.70	-8.09
	(3.31)	(9.46)	(10.66)
Homogeneous	3.14	11.99	8.85
	(3.34)	(9.15)	(10.20)
Religious leader			
Mixed	7.61**	-3.69	-11.30
	(3.11)	(10.14)	(10.85)
Homogeneous	-0.96	12.26	13.23
	(2.90)	(8.33)	(9.29)
Panel B. Implied no-leader counterfactuals			
No leader, mixed	56.93	55.66	-1.26
	(2.22)	(5.87)	(6.55)
No leader, homogeneous	54.89	50.26	-4.64
	(1.95)	(5.51)	(5.95)
Observations		1,032	
Group clusters		336	

Notes: Difference subtracts the first category from the second in each block. Panel B reports model-implied no-leader participation in percentage points. Predictions set leader terms to zero and baseline participation to its estimation-sample mean, averaging the fixed effects and category levels over the combined chief and pastor participants within each composition and category. Both categories compare the respondent's denomination with the neighborhood pastor's denomination. The comparison is defined for residents in every assignment arm, including no leader. Effects and interactions are pooled across disclosure within each composition; the model also includes separate composition-specific category levels, baseline participation, and neighborhood-by-composition-by-disclosure fixed effects. The sample includes 1,032 residents in 336 groups. Forty-two pastor-arm groups contain at least one resident who shares the pastor's denomination. Standard errors in parentheses are clustered by accountability group. Section 4.3 discusses these estimates. Stars use unadjusted tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.3.2 Which Leaders Can Exercise Their Authority?

We ask whether the assigned leader is equipped to exercise the relevant form of authority. Figure A-2 and Tables A-25–A-27 estimate Equation (2), pooling both leader effects and their interactions with leader characteristics across disclosure within each composition. The first specification controls for baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds spillover exposure, gender, and wealth strata. Leader attributes enter only through treatment interactions because they are undefined without an assigned leader. Standard errors are clustered by randomized group; the tables also report neighborhood-clustered standard errors and tests of the higher-minus-lower contrasts. Each split uses only 28 leaders of each type, and the exploratory tests are unadjusted.

Chiefs benefit from local roots; pastors from religious conviction. If chiefs' authority comes from being embedded in local ethnic networks, chiefs who have lived in the neighborhood longer should be better able to sustain participation. A longer time living in the neighborhood gives them more knowledge of residents, stronger local ties, and greater standing in the community. These are exactly the types of relationships through which chiefs would be able to enforce participation in local collective action. Pastors, on the other hand, should depend less on local tenure because their authority should not rely as strongly on neighborhood networks or local standing. Instead, their effectiveness should depend more on how strongly they themselves believe that God's oversight rewards cooperation. Thus, we expect pastor effects to be stronger among pastors with stronger beliefs in divine enforcement.

Figure A-2 presents these results. The upper panel shows how chief effects vary with avenue tenure. The left column reports effects for chiefs who have lived on the avenue for 26 years or fewer, while the right column reports effects for chiefs with longer tenure. In homogeneous groups, chiefs with longer tenure raise participation by 10.3 percentage points (SE 4.5), compared with 0.1 points for chiefs with shorter tenure (SE 3.2). The 10.2-point difference is suggestive (SE 5.2; $p = 0.051$); it remains similar with additional controls, although neighborhood-clustered inference is less precise (Table A-25). The chief-tenure difference in mixed groups is close to zero. Pastor effects do not increase with their own avenue tenure: in homogeneous groups, their effect falls from 7.1 to -5.4 points, a -12.6-point difference (SE 4.4; $p = 0.005$). Local rootedness therefore has different associations with the two leaders' effectiveness.

The lower panel turns to pastors' conviction in divine moral enforcement. We combine questions from Lang et al. (2019) on whether God rewards good people and punishes bad people into a single measure and use the existing ordered split within each leader type. In mixed groups, pastors with stronger convictions raise participation by 12.2 percentage points (SE 3.7), compared with 0.6 points for pastors with weaker convictions (SE 4.1). The 11.6-point difference is statistically significant (SE 5.1; $p = 0.024$). The adjusted contrast remains positive with neighborhood-clustered inference ($p = 0.046$). In homogeneous groups, the corresponding 7.4-point difference is less precise (SE 4.6; $p = 0.107$). Chief effects do not significantly vary with chiefs' conviction (Table A-26). These comparisons support the interpretation that pastors' reach depends on their own conviction in the religious obligation they invoke.

Leadership roles across institutions. Some leaders who participated in the campaign were primarily recognized for one role but also held a position in the other institution. Thus, some chiefs also reported serving as pastors, and some pastors reported serving as chiefs. Table A-27 compares leaders who head both institutions with those who do not. The cross-institution sample is small: five chiefs also head a religious institution, and seven pastors also head a traditional institution. In mixed groups, the chief effect is 7.4 points among those who head both institutions (SE 5.4), compared with -0.7 points among the other chiefs (SE 3.2). The 8.1-point difference is imprecise (SE 6.0; $p = 0.178$). Pastors who also head a traditional institution do not have larger effects in homogeneous groups; their higher-minus-lower difference is -4.8 points (SE 4.4; $p = 0.276$). The directions are consistent with religious roles extending chiefs' reach across territorial boundaries, but these pooled estimates do not establish an asymmetry between the two types of leader.

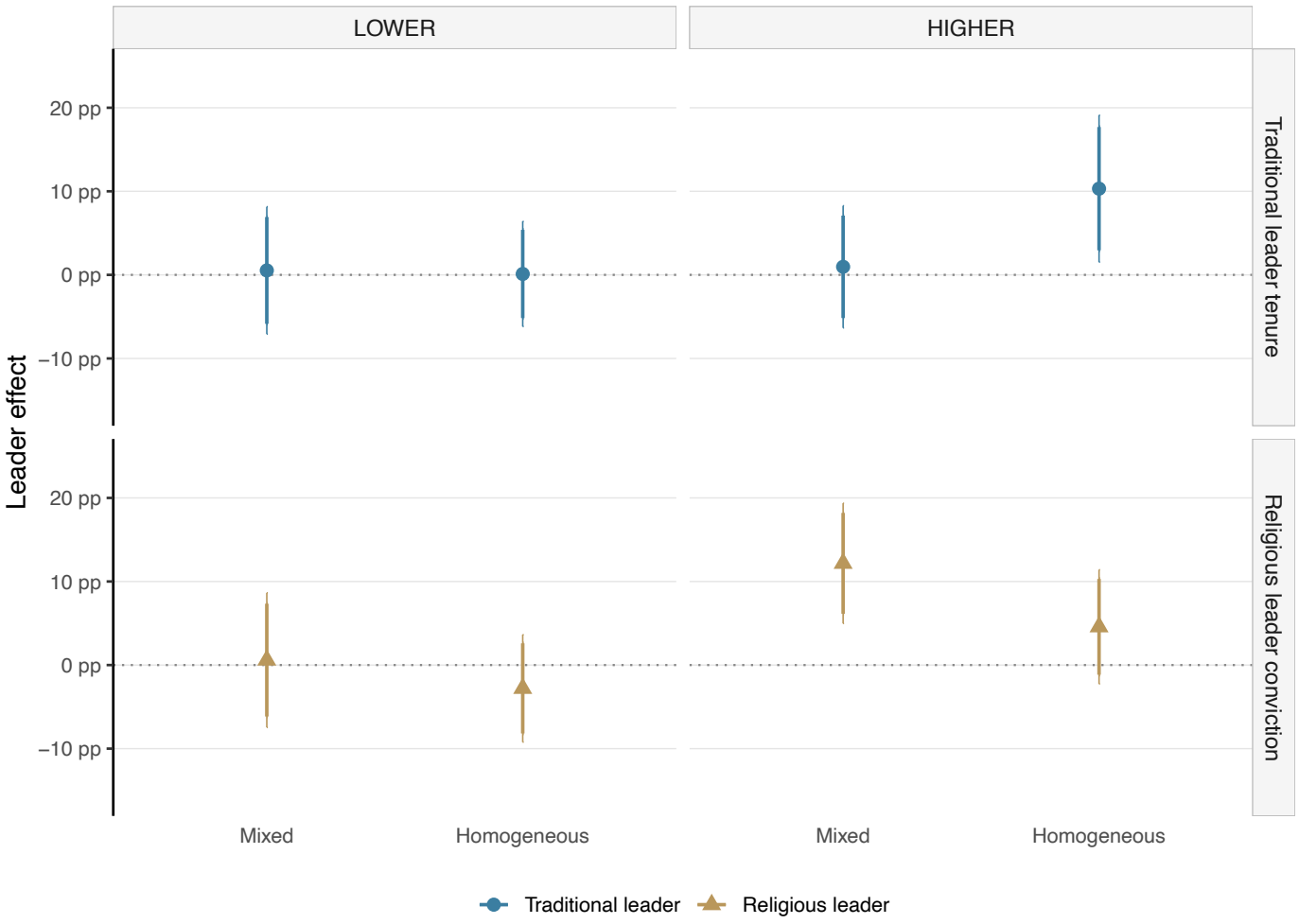


Figure A-2: TRADITIONAL LEADER ROOTEDNESS AND RELIGIOUS LEADER CONVICTION

Notes: Columns compare lower- and higher-scoring leaders. The upper row reports traditional leader effects by avenue tenure, split at 26 years; the lower row reports religious leader effects by the baseline divine moral-enforcement index. Both leader effects and their interactions are pooled across disclosure within composition. Blue circles and gold triangles denote traditional leaders and religious leaders. Thick and thin bars are 90 and 95 percent confidence intervals with group-clustered standard errors. Estimates include baseline participation and neighborhood-by-composition-by-disclosure fixed effects. The traditional leader-tenure higher-minus-lower difference in homogeneous groups is 10.2 points (SE 5.2; $p = 0.051$); the religious leader-conviction difference in mixed groups is 11.6 points (SE 5.1; $p = 0.024$). Section A.3.2 discusses these estimates. Tables A-25 and A-26 report both leader types, additional controls, alternative fixed effects, and neighborhood-clustered sensitivity.

Table A-25 – LEADER EFFECTS BY YEARS OF RESIDENCE ON THE AVENUE

	Baseline participation control			+ Spillover, gender, and wealth		
	Chief: $\leq$ 26 years Pastor: $\leq$ 27 years	Chief: $>$ 26 years Pastor: $>$ 27 years	Difference	Chief: $\leq$ 26 years Pastor: $\leq$ 27 years	Chief: $>$ 26 years Pastor: $>$ 27 years	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	0.53	0.97	0.43	0.60	0.54	-0.05
	(3.87)	(3.71)	(5.02)	(3.69)	(3.53)	(4.78)
	[5.06]	[4.71]	[6.35]	[4.77]	[4.43]	[6.00]
Homogeneous	0.11	10.32**	10.21*	0.37	10.25**	9.88*
	(3.20)	(4.47)	(5.21)	(3.20)	(4.55)	(5.31)
	[3.40]	[5.82]	[6.76]	[3.48]	[6.06]	[7.07]
Religious leader						
Mixed	5.21	7.38*	2.18	5.64	7.03*	1.40
	(4.01)	(3.84)	(5.21)	(3.93)	(3.71)	(5.09)
	[4.96]	[4.53]	[6.13]	[5.01]	[4.15]	[6.09]
Homogeneous	7.14**	-5.42	-12.56***	6.56**	-4.84	-11.41**
	(3.32)	(3.30)	(4.44)	(3.23)	(3.51)	(4.56)
	[3.92]	[4.34]	[5.95]	[3.76]	[4.58]	[6.12]
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed	56.50	56.50	–	56.50	56.50	–
	(2.00)	(2.00)		(1.90)	(1.90)	
Homogeneous	54.08	54.08	–	54.11	54.11	–
	(1.80)	(1.80)		(1.78)	(1.78)	
Observations	1,040			1,040		
Group clusters	336			336		
Neighborhood clusters	28			28		
Pretreatment participation control	Yes			Yes		
Spillover, gender, and wealth strata	No			Yes		
Traditional, mixed: neighborhood p	0.946			0.993		
Traditional, homogeneous: neighborhood p	0.143			0.174		
Religious, mixed: neighborhood p	0.726			0.820		
Religious, homogeneous: neighborhood p	0.044			0.073		

Notes: Difference subtracts the first category from the second in each block. Residence is measured in years living on the avenue. The first category includes chiefs with at most 26 years and pastors with at most 27 years. There are 15 chiefs in the first category and 13 in the second, and 14 pastors in each category. Both leader effects and characteristic interactions are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects; the second adds spillover exposure, gender, and wealth strata. The characteristic enters only through leader-treatment interactions. Parentheses report group-clustered standard errors; square brackets report neighborhood-clustered standard errors for the same estimates. The footer gives tests of the difference between the two categories, clustering by neighborhood. Panel B averages the fixed-effect component over common chief and pastor participant support within composition, setting centered controls to zero. Because the characteristic is undefined without an assigned leader, the same pooled no-leader reference is repeated across categories; the categories do not define separate observed no-leader groups. Effects are percentage points. Section A.3.2 discusses the results. Stars use unadjusted group-clustered tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-26 – LEADER EFFECTS BY BELIEF IN DIVINE REWARDS AND PUNISHMENTS

	Baseline participation control			+ Spillover, gender, and wealth		
	Chief: score ≤ 3.5 Pastor: score ≤ 4.5	Chief: score > 3.5 Pastor: score > 4.5	Difference	Chief: score ≤ 3.5 Pastor: score ≤ 4.5	Chief: score > 3.5 Pastor: score > 4.5	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	3.00	-1.38	-4.38	3.28	-2.02	-5.30
	(3.60)	(3.86)	(4.93)	(3.49)	(3.62)	(4.73)
	[4.11]	[5.10]	[5.71]	[3.77]	[4.82]	[5.42]
Homogeneous	6.64**	3.10	-3.53	6.85**	3.10	-3.76
	(3.30)	(4.44)	(5.27)	(3.28)	(4.50)	(5.32)
	[3.54]	[5.91]	[7.06]	[3.72]	[6.00]	[7.26]
Religious leader						
Mixed	0.59	12.17***	11.58**	0.65	12.17***	11.52**
	(4.09)	(3.66)	(5.12)	(3.94)	(3.58)	(4.99)
	[5.06]	[4.18]	[5.73]	[4.72]	[4.07]	[5.51]
Homogeneous	-2.79	4.57	7.37	-1.97	3.72	5.69
	(3.27)	(3.47)	(4.56)	(3.23)	(3.61)	(4.67)
	[3.54]	[4.67]	[6.13]	[3.39]	[4.88]	[6.30]
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed	56.41	56.41	–	56.41	56.41	–
	(2.02)	(2.02)		(1.90)	(1.90)	
Homogeneous	54.05	54.05	–	54.10	54.10	–
	(1.75)	(1.75)		(1.73)	(1.73)	
Observations		1,040			1,040	
Group clusters		336			336	
Neighborhood clusters		28			28	
Pretreatment participation control		Yes			Yes	
Spillover, gender, and wealth strata		No			Yes	
Traditional, mixed: neighborhood p		0.450			0.337	
Traditional, homogeneous: neighborhood p		0.621			0.609	
Religious, mixed: neighborhood p		0.053			0.046	
Religious, homogeneous: neighborhood p		0.240			0.374	

Notes: Difference subtracts the first category from the second in each block. The index combines baseline beliefs that God rewards good behavior and punishes improper behavior. The score threshold is 3.5 for chiefs and 4.5 for pastors, with 14 leaders of each type on each side of the threshold. Scores equal to the threshold enter the first category. Both leader effects and characteristic interactions are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects; the second adds spillover exposure, gender, and wealth strata. The characteristic enters only through leader-treatment interactions. Parentheses report group-clustered standard errors; square brackets report neighborhood-clustered standard errors for the same estimates. The footer gives tests of the difference between the two categories, clustering by neighborhood. Panel B averages the fixed-effect component over common chief and pastor participant support within composition, setting centered controls to zero. Because the characteristic is undefined without an assigned leader, the same pooled no-leader reference is repeated across categories; the categories do not define separate observed no-leader groups. Effects are percentage points. Section A.3.2 discusses the results. Stars use unadjusted group-clustered tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-27 – LEADER EFFECTS BY JOINT CHIEF AND PASTOR ROLES

	Baseline participation control			+ Spillover, gender, and wealth		
	Does not hold both roles	Chief and pastor roles	Difference	Does not hold both roles	Chief and pastor roles	Difference
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed	-0.74	7.37	8.11	-1.13	8.22	9.35
	(3.17)	(5.41)	(6.01)	(3.02)	(5.34)	(5.95)
	[4.18]	[5.96]	[6.63]	[4.04]	[5.10]	[6.08]
Homogeneous	6.23*	-1.58	-7.81	6.64**	-2.81	-9.46*
	(3.32)	(3.99)	(4.84)	(3.31)	(4.39)	(5.20)
	[3.88]	[5.25]	[6.24]	[3.90]	[6.23]	[7.07]
Religious leader						
Mixed	5.43*	8.82	3.39	5.44*	8.92	3.49
	(3.23)	(5.64)	(6.18)	(3.10)	(5.59)	(6.10)
	[4.51]	[2.84]	[4.73]	[4.29]	[3.12]	[4.86]
Homogeneous	2.05	-2.76	-4.81	2.34	-3.64	-5.98
	(2.94)	(3.73)	(4.41)	(2.96)	(3.71)	(4.40)
	[3.59]	[3.83]	[5.06]	[3.56]	[4.13]	[5.41]
Panel B. Implied no-leader counterfactuals						
Common no-leader comparison						
Mixed	56.52	56.52	–	56.52	56.52	–
	(2.01)	(2.01)		(1.92)	(1.92)	
Homogeneous	54.09	54.09	–	54.12	54.12	–
	(1.81)	(1.81)		(1.79)	(1.79)	
Observations		1,040			1,040	
Group clusters		336			336	
Neighborhood clusters		28			28	
Pretreatment participation control		Yes			Yes	
Spillover, gender, and wealth strata		No			Yes	
Traditional, mixed: neighborhood p		0.232			0.136	
Traditional, homogeneous: neighborhood p		0.222			0.192	
Religious, mixed: neighborhood p		0.480			0.479	
Religious, homogeneous: neighborhood p		0.350			0.279	

Notes: Difference subtracts the first category from the second in each block. The second category comprises leaders recorded as both an avenue/customary chief and a pastor, cure, or reverend. There are 5 such chiefs and 7 such pastors. The first category contains the remaining 23 chiefs and 21 pastors. Both leader effects and characteristic interactions are pooled across disclosure within composition. Both blocks include baseline participation and neighborhood-by-composition-by-disclosure fixed effects; the second adds spillover exposure, gender, and wealth strata. The characteristic enters only through leader-treatment interactions. Parentheses report group-clustered standard errors; square brackets report neighborhood-clustered standard errors for the same estimates. The footer gives tests of the difference between the two categories, clustering by neighborhood. Panel B averages the fixed-effect component over common chief and pastor participant support within composition, setting centered controls to zero. Because the characteristic is undefined without an assigned leader, the same pooled no-leader reference is repeated across categories; the categories do not define separate observed no-leader groups. Effects are percentage points. Section A.3.2 discusses the results. Stars use unadjusted group-clustered tests: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Source of Enforcement

Table A-28 – LEADER EFFECTS BY COMPOSITION AND DISCLOSURE

	Composition and disclosure		
	Baseline	+ Spillover	+ Gender/wealth
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed, without disclosure	-0.29	-0.31	0.18
	(3.48)	(3.48)	(3.39)
With disclosure	1.75	1.67	0.94
	(4.53)	(4.52)	(4.24)
Homogeneous, without disclosure	-0.31	-0.34	-0.18
	(4.46)	(4.45)	(4.47)
With disclosure	9.95***	10.13***	10.06***
	(3.63)	(3.63)	(3.63)
Religious leader			
Mixed, without disclosure	9.18**	9.16**	9.10**
	(3.97)	(3.98)	(3.93)
With disclosure	3.41	3.40	3.56
	(4.30)	(4.32)	(4.09)
Homogeneous, without disclosure	1.66	1.67	1.59
	(3.81)	(3.80)	(3.80)
With disclosure	0.01	-0.01	0.08
	(3.27)	(3.26)	(3.32)
Panel B. Treatment-effect contrasts			
Mixed, with: T − R	-1.66	-1.73	-2.61
	(4.40)	(4.41)	(4.06)
Mixed, without: T − R	-9.47**	-9.47**	-8.92**
	(3.98)	(4.01)	(4.08)
Homogeneous, with: T − R	9.94**	10.13**	9.98**
	(4.03)	(4.00)	(4.05)
Homogeneous, without: T − R	-1.97	-2.01	-1.77
	(4.13)	(4.12)	(4.26)
Δ_E: H, with − M, without	19.41***	19.60***	18.90***
	(5.67)	(5.69)	(5.78)
Panel C. No-leader reference means and diagnostics			
Mixed, without disclosure	58.88	58.88	58.88
	(2.46)	(2.44)	(2.27)
With disclosure	57.79	57.79	57.79
	(3.14)	(3.14)	(3.01)
Homogeneous, without disclosure	57.79	57.79	57.79
	(2.91)	(2.91)	(2.85)
With disclosure	55.80	55.80	55.80
	(1.96)	(1.98)	(2.00)
Wald: all no-leader cells equal	p = 0.789	p = 0.790	p = 0.777
Observations	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336
Neighborhoods	28	28	28
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes
25m spillover control	No	Yes	Yes
Gender and wealth-stratum controls	No	No	Yes

Notes: Panel A reports leader effects relative to no leader; Panel B reports direct leader contrasts; Panel C reports no-leader reference means. Leader effects vary by composition and disclosure; Δ_E compares the traditional-minus-religious gap in homogeneous groups with disclosure with the gap in mixed groups without disclosure. The outcome is average participation on six eligible days after Meeting 1 through the Meeting 2 day, in percentage points. Every column controls for pretreatment participation and neighborhood-by-composition-by-disclosure fixed effects. The second adds the 25-meter spillover indicator; the third also adds gender and wealth strata. Group-clustered standard errors appear in parentheses. Section 5 and Figure 7 discuss these estimates. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4.1 Alternative Post-Outcome Windows

The main outcome averages participation over six administrative days strictly after the first meeting through and including the second-meeting day. Figure A-3 maps this window and the timing alternatives onto the implementation timeline. Table A-29 reports estimates for the main window, the intervening middle period, the final week of each wave, and all eligible post-treatment periods. Every specification uses the same respondent sample, fixed effects, pretreatment participation control, and clustering level.

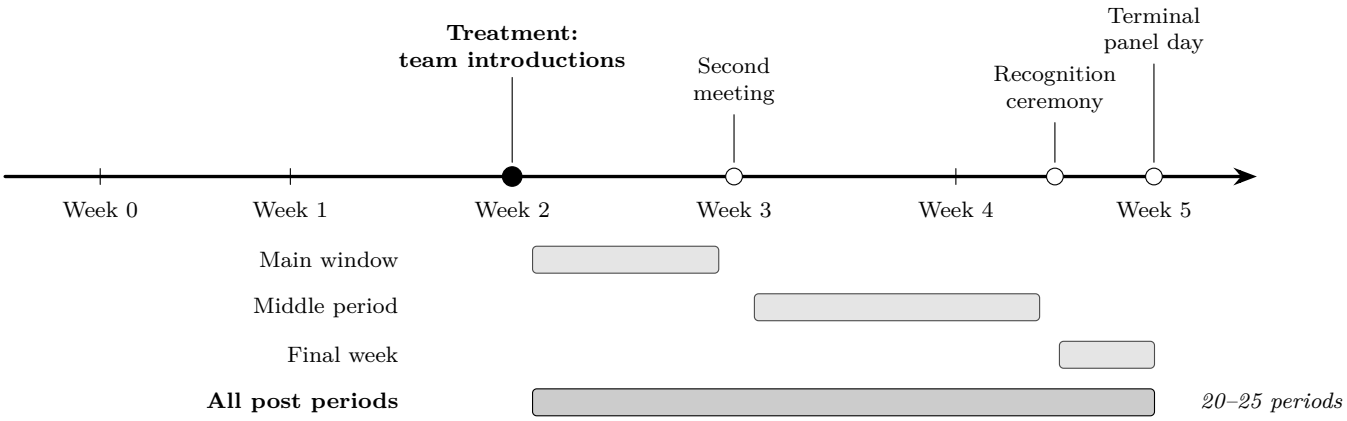


Figure A-3: Outcome windows used in the timing robustness analysis.

Table A-29 – ROBUSTNESS TO ALTERNATIVE POST-OUTCOME WINDOWS

	Leader by composition					Environment specific			
	Main window	Middle period	Final week	All post		Main window	Middle period	Final week	All post
Panel A. Leader effects relative to no leader									
Traditional leader									
Mixed	0.72	-0.59	-0.45	-0.29	Without disclosure	-0.29	0.82	-0.35	0.08
	(2.86)	(2.51)	(2.44)	(2.04)		(3.48)	(3.60)	(3.58)	(2.99)
					With disclosure	1.75	-1.98	-0.51	-0.64
						(4.53)	(3.50)	(3.35)	(2.80)
Homogeneous	4.82*	0.90	-0.40	1.27	Without disclosure	-0.31	-4.60	-7.89*	-4.80
	(2.91)	(3.03)	(2.66)	(2.39)		(4.46)	(4.44)	(4.17)	(3.69)
					With disclosure	9.95***	6.43	7.13**	7.36***
						(3.63)	(3.96)	(2.91)	(2.80)
Religious leader									
Mixed	6.29**	5.93**	4.41*	5.21**	Without disclosure	9.18**	9.61**	5.59	7.76**
	(2.94)	(2.67)	(2.54)	(2.19)		(3.97)	(3.81)	(3.97)	(3.20)
					With disclosure	3.41	2.25	3.22	2.66
						(4.30)	(3.69)	(3.20)	(2.96)
Homogeneous	0.83	-0.27	-3.05	-1.20	Without disclosure	1.66	0.89	-5.18	-1.20
	(2.52)	(2.59)	(2.37)	(2.03)		(3.81)	(3.61)	(3.59)	(3.00)
					With disclosure	0.01	-1.41	-0.90	-1.18
						(3.27)	(3.63)	(2.92)	(2.60)
Panel B. Treatment-effect contrasts									
Mixed: T − R	-5.57*	-6.52**	-4.85*	-5.50**	Mixed, with: T − R	-1.66	-4.24	-3.74	-3.30
	(2.98)	(2.72)	(2.54)	(2.19)		(4.40)	(3.98)	(3.28)	(3.01)
					Mixed, without: T − R	-9.47**	-8.78**	-5.94	-7.69**
						(3.98)	(3.68)	(3.88)	(3.15)
Homogeneous: T − R	3.98	1.17	2.66	2.47	Homogeneous, with: T − R	9.94**	7.84**	8.03***	8.54***
	(2.94)	(2.82)	(2.42)	(2.22)		(4.03)	(3.58)	(2.96)	(2.75)
					Homogeneous, without: T − R	-1.97	-5.48	-2.71	-3.60
						(4.13)	(4.15)	(3.65)	(3.25)
Δ_C: composition gradient	9.55**	7.69*	7.51**	7.97**	Δ_E: H, with − M, without	19.41***	16.62***	13.97***	16.23***
	(4.20)	(3.92)	(3.52)	(3.12)		(5.67)	(5.14)	(4.88)	(4.18)
Panel C. No-leader reference means and diagnostics									
Mixed	58.33	42.81	26.63	39.65	Without disclosure	58.88	42.38	27.64	39.90
	(1.99)	(1.73)	(1.70)	(1.43)		(2.46)	(2.60)	(2.49)	(2.11)
					With disclosure	57.79	43.24	25.62	39.40
						(3.14)	(2.23)	(2.30)	(1.93)
Homogeneous	56.79	43.86	27.95	40.30	Without disclosure	57.79	45.53	32.45	42.75
	(1.76)	(1.96)	(1.81)	(1.55)		(2.91)	(2.71)	(2.89)	(2.40)
					With disclosure	55.80	42.18	23.45	37.85
						(1.96)	(2.82)	(2.01)	(1.89)
Wald: all no-leader cells equal	p = 0.562	p = 0.689	p = 0.594	p = 0.757		p = 0.789	p = 0.812	p = 0.075	p = 0.457
Observations	1,040	1,040	1,040	1,040		1,040	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336	336		336	336	336	336
Neighborhoods	28	28	28	28		28	28	28	28
Post periods: mean	6.0	9.5	7.0	22.5		6.0	9.5	7.0	22.5
Post periods: range	6–6	7–12	7–7	20–25		6–6	7–12	7–7	20–25
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes	Yes		Yes	Yes	Yes	Yes
25m spillover control	No	No	No	No		No	No	No	No
Gender and wealth-stratum controls	No	No	No	No		No	No	No	No

Notes: All columns control for pretreatment participation and include neighborhood × disclosure × composition fixed effects. The main window averages participation strictly after the assigned first meeting through and including the assigned second-meeting day. The middle period runs from the assigned second-meeting cutoff through the day before the final week; the final week covers $T-6$ through T, where T is the terminal panel day. The all-post window uses every non-treatment day after the assigned first meeting. Panel A compares each leader assignment to no leader within the relevant fixed-effect cell. Panel B reports traditional-minus-religious contrasts and the composition and environment gradients. Panel C reports model-implied no-leader reference means and equality tests. Panel B and Panel C inference uses the clustered delta method. Standard errors are clustered one-way at the group-neighborhood level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4.2 Alternative Outcomes and Inference

Table A-30 – LABOR CONTRIBUTION OUTCOMES BY COMPOSITION AND DISCLOSURE

	Environment specific			
	Participation rate	Attendance days	Total hours	Total effort
Panel A. Leader effects relative to no leader				
Traditional leader				
Mixed, without disclosure	-0.003	0.021	-0.544	-0.146
	(0.035)	(0.215)	(0.876)	(0.706)
Mixed, with disclosure	0.017	0.100	-0.023	-0.012
	(0.045)	(0.270)	(0.962)	(0.881)
Homogeneous, without disclosure	-0.003	-0.051	0.298	0.080
	(0.045)	(0.256)	(0.921)	(0.850)
Homogeneous, with disclosure	0.100***	0.552**	2.044**	1.872**
	(0.036)	(0.219)	(0.913)	(0.734)
Religious leader				
Mixed, without disclosure	0.092**	0.585**	1.729*	1.506**
	(0.040)	(0.242)	(0.961)	(0.763)
Mixed, with disclosure	0.034	0.203	0.094	0.710
	(0.043)	(0.259)	(1.000)	(0.869)
Homogeneous, without disclosure	0.017	0.032	0.009	-0.148
	(0.038)	(0.222)	(0.850)	(0.754)
Homogeneous, with disclosure	0.000	0.020	-0.159	0.205
	(0.033)	(0.197)	(0.841)	(0.712)
Panel B. Treatment-effect contrasts				
Mixed, without: T − R	-0.095**	-0.564**	-2.273**	-1.652**
	(0.040)	(0.244)	(0.940)	(0.802)
	[Holm p = 0.064]	[Holm p = 0.064]	[Holm p = 0.064]	[Holm p = 0.064]
Mixed, with: T − R	-0.017	-0.103	-0.117	-0.723
	(0.044)	(0.262)	(0.989)	(0.860)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]
Homogeneous, without: T − R	-0.020	-0.082	0.289	0.227
	(0.041)	(0.239)	(0.853)	(0.829)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]
Homogeneous, with: T − R	0.099**	0.532**	2.203**	1.666**
	(0.040)	(0.237)	(0.953)	(0.804)
	[Holm p = 0.056]	[Holm p = 0.064]	[Holm p = 0.064]	[Holm p = 0.064]
Δ_E	0.194***	1.096***	4.476***	3.318***
	(0.057)	(0.341)	(1.342)	(1.137)
	[Holm p = 0.003]	[Holm p = 0.003]	[Holm p = 0.003]	[Holm p = 0.004]
Panel C. No-leader reference means and diagnostics				
Mixed, without disclosure	0.589	3.478	13.438	11.571
	(0.025)	(0.150)	(0.624)	(0.470)
Mixed, with disclosure	0.578	3.413	12.983	11.098
	(0.031)	(0.188)	(0.689)	(0.625)
Homogeneous, without disclosure	0.578	3.457	12.877	11.272
	(0.029)	(0.170)	(0.650)	(0.549)
Homogeneous, with disclosure	0.558	3.272	12.529	10.473
	(0.020)	(0.123)	(0.565)	(0.452)
Wald: four cells equal	p = 0.789	p = 0.699	p = 0.758	p = 0.389
Observations	1,040	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336	336
Neighborhoods	28	28	28	28
Matched pretreatment outcome control	Yes	Yes	Yes	Yes
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_E. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Participation is the daily rate; attendance is the number of labor days; hours and effort are window totals, with nonattendance coded zero. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-31 – LABOR CONTRIBUTIONS: ASINH HOURS BY COMPOSITION AND DISCLOSURE

	Environment specific
	Asinh total hours
Panel A. Leader effects relative to no leader	
Traditional leader	
Mixed, without disclosure	-0.018
	(0.164)
Mixed, with disclosure	0.014
	(0.172)
Homogeneous, without disclosure	-0.032
	(0.157)
Homogeneous, with disclosure	0.416***
	(0.145)
Religious leader	
Mixed, without disclosure	0.313**
	(0.159)
Mixed, with disclosure	0.044
	(0.169)
Homogeneous, without disclosure	0.031
	(0.140)
Homogeneous, with disclosure	0.055
	(0.125)
Panel B. Treatment-effect contrasts	
Mixed, without: T − R	-0.331*
	(0.172)
	[Holm p = 0.055]
Mixed, with: T − R	-0.030
	(0.171)
	[Holm p = 0.862]
Homogeneous, without: T − R	-0.062
	(0.152)
	[Holm p = 0.683]
Homogeneous, with: T − R	0.361**
	(0.152)
	[Holm p = 0.018]
Δ_E	0.692***
	(0.230)
	[Holm p = 0.003]
Panel C. No-leader reference means and diagnostics	
Mixed, without disclosure	2.683
	(0.106)
Mixed, with disclosure	2.654
	(0.120)
Homogeneous, without disclosure	2.622
	(0.102)
Homogeneous, with disclosure	2.565
	(0.079)
Wald: four cells equal	p = 0.822
Observations	1,040
Group-neighborhood clusters	336
Neighborhoods	28
Matched pretreatment outcome control	Yes
Neighborhood × disclosure × composition FEs	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_E. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Asinh hours retains zero-valued windows, preserving the full frozen analysis sample. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-32 – CASH AND IN-KIND CONTRIBUTIONS BY COMPOSITION AND DISCLOSURE

	Environment specific			
	Any cash contribution	Cash total	Any in-kind contribution	In-kind value
Panel A. Leader effects relative to no leader				
Traditional leader				
Mixed, without disclosure	0.014	31.786	-0.074*	-258.738**
	(0.022)	(44.980)	(0.041)	(127.396)
Mixed, with disclosure	0.046*	-1.842	0.065	149.692
	(0.025)	(30.774)	(0.047)	(154.389)
Homogeneous, without disclosure	0.012	6.736	-0.001	-404.699*
	(0.018)	(8.137)	(0.058)	(216.039)
Homogeneous, with disclosure	0.013	-9.239	0.015	-1861.438
	(0.020)	(27.015)	(0.054)	(1332.055)
Religious leader				
Mixed, without disclosure	0.002	5.595	-0.011	-96.757
	(0.026)	(29.524)	(0.039)	(138.257)
Mixed, with disclosure	0.010	-19.699	0.117**	81.611
	(0.020)	(29.577)	(0.054)	(140.508)
Homogeneous, without disclosure	0.012	3.165	0.067	-0.268
	(0.020)	(8.133)	(0.059)	(237.941)
Homogeneous, with disclosure	-0.010	-29.877	-0.065	-1919.618
	(0.018)	(24.873)	(0.055)	(1336.608)
Panel B. Treatment-effect contrasts				
Mixed, without: T − R	0.012	26.190	-0.064	-161.981
	(0.031)	(48.356)	(0.046)	(122.633)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 0.666]	[Holm p = 0.666]
Mixed, with: T − R	0.036	17.857	-0.052	68.082
	(0.025)	(24.618)	(0.057)	(187.360)
	[Holm p = 0.647]	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 1.000]
Homogeneous, without: T − R	-0.000	3.571	-0.067	-404.431*
	(0.017)	(6.854)	(0.062)	(208.376)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 0.830]	[Holm p = 0.212]
Homogeneous, with: T − R	0.023	20.638	0.081	58.181
	(0.020)	(21.559)	(0.058)	(940.972)
	[Holm p = 0.735]	[Holm p = 0.735]	[Holm p = 0.647]	[Holm p = 0.951]
Δ_E	0.011	-5.552	0.144*	220.161
	(0.037)	(52.956)	(0.074)	(948.659)
	[Holm p = 1.000]	[Holm p = 1.000]	[Holm p = 0.203]	[Holm p = 1.000]
Panel C. No-leader reference means and diagnostics				
Mixed, without disclosure	0.033	38.587	0.293	572.826
	(0.010)	(17.678)	(0.025)	(100.143)
Mixed, with disclosure	0.011	38.043	0.239	514.130
	(0.014)	(25.631)	(0.031)	(67.275)
Homogeneous, without disclosure	0.011	5.435	0.261	747.826
	(0.014)	(6.318)	(0.037)	(172.380)
Homogeneous, with disclosure	0.022	38.043	0.315	2490.217
	(0.013)	(21.843)	(0.037)	(1201.373)
Wald: four cells equal	p = 0.526	p = 0.122	p = 0.354	p = 0.236
Observations	1,040	1,040	1,040	1,040
Group-neighborhood clusters	336	336	336	336
Neighborhoods	28	28	28	28
Matched pretreatment outcome control	Yes	Yes	Yes	Yes
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_E. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Cash and in-kind extensive margins are window indicators; amounts are window totals in Congolese francs (CDF). The source-level identity combined material value = cash + in-kind is validated before estimation. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-33 – CASH AND IN-KIND CONTRIBUTIONS: ASINH VALUES BY COMPOSITION AND DISCLOSURE

	Environment specific	
	Asinh cash total	Asinh in-kind value
Panel A. Leader effects relative to no leader		
Traditional leader		
Mixed, without disclosure	0.137	-0.653**
	(0.173)	(0.318)
Mixed, with disclosure	0.305*	0.518
	(0.182)	(0.366)
Homogeneous, without disclosure	0.087	-0.180
	(0.119)	(0.459)
Homogeneous, with disclosure	0.084	0.061
	(0.150)	(0.428)
Religious leader		
Mixed, without disclosure	0.034	-0.112
	(0.178)	(0.300)
Mixed, with disclosure	0.059	0.950**
	(0.148)	(0.427)
Homogeneous, without disclosure	0.076	0.486
	(0.133)	(0.472)
Homogeneous, with disclosure	-0.086	-0.588
	(0.131)	(0.441)
Panel B. Treatment-effect contrasts		
Mixed, without: T − R	0.103	-0.540
	(0.232)	(0.347)
	[Holm p = 0.658]	[Holm p = 0.241]
Mixed, with: T − R	0.247	-0.432
	(0.182)	(0.455)
	[Holm p = 0.350]	[Holm p = 0.350]
Homogeneous, without: T − R	0.011	-0.666
	(0.111)	(0.492)
	[Holm p = 0.922]	[Holm p = 0.353]
Homogeneous, with: T − R	0.170	0.649
	(0.144)	(0.449)
	[Holm p = 0.299]	[Holm p = 0.299]
Δ_E	0.067	1.189**
	(0.273)	(0.568)
	[Holm p = 0.805]	[Holm p = 0.074]
Panel C. No-leader reference means and diagnostics		
Mixed, without disclosure	0.225	2.334
	(0.068)	(0.194)
Mixed, with disclosure	0.096	1.909
	(0.108)	(0.235)
Homogeneous, without disclosure	0.075	2.123
	(0.096)	(0.296)
Homogeneous, with disclosure	0.170	2.531
	(0.099)	(0.299)
Wald: four cells equal	p = 0.558	p = 0.349
Observations	1,040	1,040
Group-neighborhood clusters	336	336
Neighborhoods	28	28
Matched pretreatment outcome control	Yes	Yes
Neighborhood × disclosure × composition FEs	Yes	Yes

Notes: Outcomes aggregate the frozen six-day main-outcome window, strictly after Meeting 1 through and including the Meeting 2 day; the Meeting 1 day is excluded. Panel A compares each leader assignment with no leader. Panel B reports direct traditional-minus-religious contrasts and Δ_E. Parentheses contain group-neighborhood-clustered standard errors; bracketed values are Holm-adjusted p-values across the displayed outcome family for the same contrast. Panel C reports model-implied no-leader means and equality tests. Models include the matched pre-treatment outcome and neighborhood × disclosure × composition fixed effects. Inverse-hyperbolic-sine specifications retain zero-valued windows. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-34 – RANDOMIZATION INFERENCE BY COMPOSITION AND DISCLOSURE

	Environment specific		
	Estimate	Clustered SE	RI p
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed, without disclosure	-0.29	(3.48)	0.951
Mixed, with disclosure	1.75	(4.53)	0.696
Homogeneous, without disclosure	-0.31	(4.46)	0.943
Homogeneous, with disclosure	9.95***	(3.63)	0.028
Religious leader			
Mixed, without disclosure	9.18**	(3.97)	0.038
Mixed, with disclosure	3.41	(4.30)	0.447
Homogeneous, without disclosure	1.66	(3.81)	0.703
Homogeneous, with disclosure	0.01	(3.27)	0.998
Panel B. Treatment-effect contrasts			
Mixed, with: T − R	-1.66	(4.40)	0.724
Mixed, without: T − R	-9.47**	(3.98)	0.043
Homogeneous, with: T − R	9.94**	(4.03)	0.033
Homogeneous, without: T − R	-1.97	(4.13)	0.654
Δ_E: H, with − M, without	19.41***	(5.67)	0.003
Panel C. No-leader reference means and diagnostics			
Mixed, without disclosure	58.88	(2.46)	
Mixed, with disclosure	57.79	(3.14)	
Homogeneous, without disclosure	57.79	(2.91)	
Homogeneous, with disclosure	55.80	(1.96)	
Wald: all no-leader cells equal	p = 0.789		
Observations	1,040		
Group-neighborhood clusters	336		
Neighborhoods	28		
NQC fixed effects	Yes		
Pretreatment participation control	Yes		
Formation-stage permutations	5,000		

Notes: The table follows the specification and row structure of the paper's main results table. The outcome is average post-first-meeting salongo participation, in percentage points. All models include the six-day pretreatment participation control and neighborhood × disclosure × composition fixed effects. Panel A compares each leader assignment with no leader in the same fixed-effect cell. Panel B reports the same leader contrasts as the main table; Δ_E is the Traditional-minus-Religious contrast in homogeneous groups with disclosure minus the same contrast in mixed groups without disclosure. Panel C reports model-implied no-leader reference means and clustered delta-method tests that the displayed no-leader means are equal. Standard errors are clustered one-way at the group–neighborhood level. RI p-values are two-sided and come from 5,000 conditional replays of the realized participants through the group-formation stage. The replay preserves wave-specific group sizes, randomized filling order, composition and fallback rules, gender × wealth pattern targets, mixed-group territory diversity, and treatment coding; it does not replay respondent sampling or the original balance-selection stage. The replay seed is 20260731. Stars use clustered asymptotic inference: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-35 – ALTERNATIVE CLUSTERING BY COMPOSITION AND DISCLOSURE

	Environment specific		
	Group	Leader cluster	Neighborhood
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed, without disclosure	-0.29	-0.29	-0.29
	(3.48)	(3.46)	(3.65)
Mixed, with disclosure	1.75	1.75	1.75
	(4.53)	(4.55)	(5.48)
Homogeneous, without disclosure	-0.31	-0.31	-0.31
	(4.46)	(4.48)	(5.60)
Homogeneous, with disclosure	9.95***	9.95***	9.95**
	(3.63)	(3.65)	(4.22)
Religious leader			
Mixed, without disclosure	9.18**	9.18**	9.18*
	(3.97)	(3.97)	(4.82)
Mixed, with disclosure	3.41	3.41	3.41
	(4.30)	(4.31)	(5.09)
Homogeneous, without disclosure	1.66	1.66	1.66
	(3.81)	(3.82)	(4.15)
Homogeneous, with disclosure	0.01	0.01	0.01
	(3.27)	(3.31)	(3.48)
Panel B. Treatment-effect contrasts			
Mixed, with: T − R	-1.66	-1.66	-1.66
	(4.40)	(4.42)	(4.95)
Mixed, without: T − R	-9.47**	-9.47**	-9.47*
	(3.98)	(4.00)	(4.76)
Homogeneous, with: T − R	9.94**	9.94**	9.94*
	(4.03)	(4.04)	(5.06)
Homogeneous, without: T − R	-1.97	-1.97	-1.97
	(4.13)	(4.14)	(4.67)
Δ_E: H, with − M, without	19.41***	19.41***	19.41**
	(5.67)	(6.00)	(7.56)
Panel C. No-leader reference means and diagnostics			
Mixed, without disclosure	58.88	58.88	58.88
	(2.46)	(2.47)	(2.50)
Mixed, with disclosure	57.79	57.79	57.79
	(3.14)	(3.15)	(3.19)
Homogeneous, without disclosure	57.79	57.79	57.79
	(2.91)	(2.92)	(2.96)
Homogeneous, with disclosure	55.80	55.80	55.80
	(1.96)	(1.97)	(1.99)
Wald: all no-leader cells equal	p = 0.789	p = 0.733	p = 0.740
Observations	1,040	1,040	1,040
Clusters	336	84	28
Neighborhoods	28	28	28
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes

Notes: The table reproduces the main-table sample, outcomes, neighborhood × disclosure × composition fixed effects, pretreatment participation control, leader-effect definitions, contrasts, and no-leader diagnostics. Columns differ only in the one-way clustering level. Group clusters are the 336 experimental group–neighborhood cells. Leader-group clusters are the 84 neighborhood-wave × leader-arm cells, each containing the four groups assigned to traditional, religious, or no leader. Neighborhood clustering uses 28 neighborhood-wave assignments. Panel B and Panel C inference uses the clustered delta method. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-36 – PRETREATMENT DYNAMICS

	All six pretreatment days			Excluding day 1		
Panel A. Linear trends						
	Estimate	Standard error	p-value	Estimate	Standard error	p-value
Traditional leader						
Mixed, without disclosure	-1.83	(1.32)	0.165	-1.29	(1.60)	0.421
Mixed, with disclosure	0.83	(1.26)	0.510	0.10	(1.40)	0.945
Homogeneous, without disclosure	-0.84	(1.35)	0.533	-0.00	(1.91)	0.999
Homogeneous, with disclosure	-2.12	(1.06)	0.047	-1.73	(1.27)	0.175
Religious leader						
Mixed, without disclosure	1.20	(1.21)	0.321	1.33	(1.58)	0.400
Mixed, with disclosure	1.00	(1.15)	0.385	0.69	(1.30)	0.596
Homogeneous, without disclosure	-2.75	(1.41)	0.052	-2.03	(1.81)	0.263
Homogeneous, with disclosure	-1.68	(1.10)	0.126	-1.49	(1.59)	0.349
Panel B. Early versus late pretreatment						
	Estimate	Standard error	p-value	Estimate	Standard error	p-value
Traditional leader						
Mixed, without disclosure	-3.13	(4.19)	0.456	-0.72	(4.31)	0.868
Mixed, with disclosure	2.47	(4.06)	0.543	0.83	(4.09)	0.839
Homogeneous, without disclosure	-2.20	(4.79)	0.647	-0.29	(5.52)	0.958
Homogeneous, with disclosure	-6.25	(3.53)	0.078	-4.51	(3.99)	0.260
Religious leader						
Mixed, without disclosure	2.83	(3.79)	0.457	2.26	(4.11)	0.583
Mixed, with disclosure	1.28	(3.80)	0.736	-0.16	(3.77)	0.967
Homogeneous, without disclosure	-6.96	(4.71)	0.141	-4.06	(5.00)	0.418
Homogeneous, with disclosure	-2.68	(4.25)	0.529	-0.94	(5.08)	0.854
Panel C. Saturated day-specific paths						
	F	Restrictions	p-value	F	Restrictions	p-value
Traditional leader						
Mixed, without disclosure	2.97	5	0.012	3.39	4	0.010
Mixed, with disclosure	0.51	5	0.768	0.50	4	0.737
Homogeneous, without disclosure	0.47	5	0.801	0.20	4	0.936
Homogeneous, with disclosure	1.83	5	0.106	1.76	4	0.137
Religious leader						
Mixed, without disclosure	1.56	5	0.170	1.94	4	0.104
Mixed, with disclosure	0.42	5	0.832	0.34	4	0.850
Homogeneous, without disclosure	2.72	5	0.020	2.17	4	0.072
Homogeneous, with disclosure	1.29	5	0.270	1.35	4	0.253
Respondents		1,040			1,040	
Group–neighborhood clusters		336			336	
Neighborhoods		28			28	

Notes: Panels A and B report percentage-point estimates, clustered standard errors, and p-values for each of the eight leader-by-information-by-composition paths. Panel A estimates the change in the leader-minus-no-leader gap per campaign day. Panel B compares days 1–3 with days 4–6 in the full window and days 2–3 with days 4–6 after excluding day 1. Panel C is a joint multi-coefficient test and therefore reports the F statistic, number of restrictions, and p-value rather than a single estimate and standard error. It tests the unrestricted day-specific coefficients for each path, using campaign day 6 as the omitted normalization period. All regressions include respondent fixed effects and neighborhood × information × composition × campaign-day fixed effects. Standard errors are clustered one-way by assigned group within neighborhood.

Table A-37 – BASELINE IMBALANCE AND TREATMENT-SPECIFIC BASELINE ADJUSTMENT

	Leader by composition				Environment specific		
	Baseline adjustment	+ Spillover	+ Gender/wealth		Baseline adjustment	+ Spillover	+ Gender/wealth
Panel A. Leader effects relative to no leader							
Traditional leader							
Mixed	0.73	0.68	0.47	Without disclosure	-0.20	-0.23	0.09
	(2.82)	(2.83)	(2.69)		(3.47)	(3.47)	(3.42)
				With disclosure	1.56	1.49	0.78
					(4.40)	(4.39)	(4.11)
Homogeneous	4.13	4.20	4.38	Without disclosure	-0.50	-0.53	-0.34
	(2.83)	(2.83)	(2.85)		(4.50)	(4.49)	(4.53)
				With disclosure	8.87***	9.06***	9.22***
					(3.26)	(3.27)	(3.32)
Religious leader							
Mixed	6.01**	6.00**	6.00**	Without disclosure	8.74**	8.72**	8.58**
	(2.93)	(2.94)	(2.84)		(3.98)	(3.99)	(3.93)
				With disclosure	3.29	3.28	3.43
					(4.29)	(4.32)	(4.08)
Homogeneous	1.10	1.10	1.17	Without disclosure	1.39	1.39	1.42
	(2.57)	(2.57)	(2.58)		(3.93)	(3.92)	(3.93)
				With disclosure	0.84	0.83	0.95
					(3.26)	(3.26)	(3.29)
Panel B. Treatment-effect contrasts							
Mixed: T − R	-5.29*	-5.32*	-5.53*	Mixed, with: T − R	-1.73	-1.79	-2.65
	(2.94)	(2.96)	(2.86)		(4.36)	(4.38)	(4.04)
				Mixed, without: T − R	-8.95**	-8.95**	-8.49**
					(3.88)	(3.90)	(4.02)
Homogeneous: T − R	3.03	3.11	3.21	Homogeneous, with: T − R	8.02**	8.23**	8.26**
	(2.85)	(2.85)	(2.92)		(3.73)	(3.72)	(3.78)
				Homogeneous, without: T − R	-1.89	-1.92	-1.76
					(4.18)	(4.16)	(4.33)
Δ_C: composition gradient	8.32**	8.43**	8.74**	Δ_E: H, with − M, without	16.97***	17.18***	16.75***
	(4.10)	(4.11)	(4.10)		(5.39)	(5.42)	(5.53)
Panel C. No-leader reference means and diagnostics							
Mixed	58.33	58.33	58.33	Without disclosure	58.88	58.88	58.88
	(1.98)	(1.97)	(1.86)		(2.45)	(2.44)	(2.26)
				With disclosure	57.79	57.79	57.79
					(3.11)	(3.11)	(2.97)
Homogeneous	56.79	56.79	56.79	Without disclosure	57.79	57.79	57.79
	(1.73)	(1.73)	(1.72)		(2.91)	(2.91)	(2.85)
				With disclosure	55.80	55.80	55.80
					(1.84)	(1.84)	(1.88)
Wald: all no-leader cells equal	p = 0.557	p = 0.557	p = 0.543		p = 0.770	p = 0.770	p = 0.756
Panel D. Joint tests							
All composition coefficients zero	0.135	0.133	0.106	All environment coefficients zero	0.062	0.059	0.067
Observations	1,040	1,040	1,040		1,040	1,040	1,040
Group–neighborhood clusters	336	336	336		336	336	336
Neighborhoods	28	28	28		28	28	28
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes		Yes	Yes	Yes
Assignment-cell baseline slopes	Yes	Yes	Yes		Yes	Yes	Yes
25m spillover control	No	Yes	Yes		No	Yes	Yes
Gender and wealth-stratum controls	No	No	Yes		No	No	Yes

Notes: The unit is a collapsed respondent–treatment-cell observation; the outcome is average post-first-meeting salongo participation in percentage points. All specifications include neighborhood × information × composition fixed effects and globally centered pretreatment participation interacted with all 12 assignment cells. Panels A–D report, respectively, leader effects, leader contrasts, model-implied no-leader means and equality tests, and joint tests of the displayed leader effects. $\Delta_C = (\tau^T_H - \tau^T_M) - (\tau^R_H - \tau^R_M)$ and $\Delta_E = (\tau^T_{HI} - \tau^T_{MN}) - (\tau^R_{HI} - \tau^R_{MN})$. Spillover columns add 25m exposure; gender/wealth columns additionally include fixed gender and wealth-stratum controls. Inference uses the clustered delta method with one-way clustering at the group–neighborhood level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-38 – INTERACTIONS ABOUT PARTICIPATION AND ITS CONSEQUENCES

	Environment-specific effects									
	Any interaction		Discussed salongo		Asked respondent to participate		Mentioned social consequences		Mentioned divine consequences	
	Non-leader	Leader	Non-leader	Leader	Non-leader	Leader	Non-leader	Leader	Non-leader	Leader
Panel A. Leader effects relative to no leader, by environment										
Traditional leader										
Mixed, without disclosure	-0.026	0.102$^{\cdot}$	-0.064	0.049	-0.023	-0.036	-0.004	-0.002	-0.009	-0.008
	(0.077)	(0.063)	(0.074)	(0.074)	(0.032)	(0.034)	(0.011)	(0.016)	(0.014)	(0.018)
Mixed, with disclosure	0.013	0.203**	0.026	0.171**	-0.030	0.116$^{\cdot}$	0.015	0.025	-0.018	0.016
	(0.074)	(0.089)	(0.070)	(0.087)	(0.043)	(0.071)	(0.011)	(0.020)	(0.025)	(0.045)
Homogeneous, without disclosure	-0.043	0.130*	-0.052	0.037	-0.049$^{\cdot}$	0.011	-0.005	0.004	-0.006	-0.004
	(0.076)	(0.074)	(0.068)	(0.062)	(0.030)	(0.041)	(0.011)	(0.017)	(0.015)	(0.017)
Homogeneous, with disclosure	-0.068	0.007	-0.002	0.050	-0.044	0.034	0.013	0.008	-0.016	0.012
	(0.069)	(0.078)	(0.057)	(0.070)	(0.033)	(0.048)	(0.012)	(0.014)	(0.021)	(0.028)
Religious leader										
Mixed, without disclosure	-0.036	0.048	0.005	0.108	-0.019	0.040	-0.011	0.010	0.015	0.017
	(0.076)	(0.076)	(0.072)	(0.076)	(0.036)	(0.049)	(0.011)	(0.020)	(0.024)	(0.025)
Mixed, with disclosure	0.121$^{\cdot}$	0.055	0.115$^{\cdot}$	0.059	-0.017	-0.026	0.008	-0.004	-0.026	-0.031
	(0.082)	(0.080)	(0.071)	(0.074)	(0.041)	(0.046)	(0.009)	(0.004)	(0.023)	(0.024)
Homogeneous, without disclosure	-0.025	0.072	-0.078	0.031	-0.032	0.042	-0.005	0.030	0.006	0.022
	(0.075)	(0.071)	(0.056)	(0.064)	(0.033)	(0.043)	(0.011)	(0.025)	(0.017)	(0.024)
Homogeneous, with disclosure	-0.103$^{\cdot}$	0.073	-0.055	0.109	-0.027	0.028	0.001	0.008	-0.023	0.014
	(0.068)	(0.082)	(0.059)	(0.077)	(0.042)	(0.053)	(0.009)	(0.015)	(0.020)	(0.030)
Panel B. No-leader environment reference means and diagnostics										
No leader, mixed, without disclosure	0.392		0.302		0.101		0.018		0.025	
	(0.050)		(0.047)		(0.025)		(0.009)		(0.011)	
No leader, mixed, with disclosure	0.312		0.256		0.104		0.004		0.044	
	(0.057)		(0.049)		(0.037)		(0.004)		(0.020)	
No leader, homogeneous, without disclosure	0.352		0.295		0.094		0.011		0.018	
	(0.043)		(0.038)		(0.022)		(0.008)		(0.009)	
No leader, homogeneous, with disclosure	0.390		0.268		0.102		0.006		0.029	
	(0.048)		(0.037)		(0.029)		(0.005)		(0.019)	
Wald: four no-leader cells equal	p = 0.682		p = 0.874		p = 0.994		p = 0.512		p = 0.670	
Observations	2,844	2,844	2,844	2,844	2,844	2,844	2,844	2,844	2,844	2,844
Groups	336	336	336	336	336	336	336	336	336	336
Neighborhoods	28	28	28	28	28	28	28	28	28	28
Baseline participation control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The last three outcomes record whether the referenced person asked the respondent to participate, mentioned social consequences, or mentioned divine consequences. The sample excludes respondent-leaders; each observation is a non-leader respondent evaluating a referenced person. Only composition-by-disclosure environment estimates are shown. Within each outcome block, the non-leader column reports the leader-assignment effect for participant-to-participant dyads; the leader column reports the total effect for participant-to-leader dyads. All effects are relative to dyads in no-leader groups in the same environment. Panel B reports no-leader environment means and an equality test. All regressions control for respondent i's average observed *salongo* participation before the first accountability meeting held in the neighborhood. Standard errors are clustered by randomized group within neighborhood. Significance: $^{\cdot}$ $p < 0.15$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-39 – EXAMPLES OF THE THREE SPEECH TOPICS

Topic and typical words	Illustrative speech
Attendance and performance *hours, week, absence, percentage, punctuality*	"Last week we completed only a few hours. We should reach five hours per person each day and come regularly." "I call on those who did not work last week to join us so that we can maintain the places where we live." "Everyone was present this week. Next week, let us work with the same commitment to improve our neighborhood." **Chief:** "Doing this work is like being on a football team: those who want to win compete for the cup. [. . .] We must improve our performance to win the contest. [. . .] We must encourage those who did not come and go and see them to find out why they were absent. We must help one another so that our group does better." **Resident:** "[. . .] I ask that encouragement become a habit in our group. If I am absent, I should be reminded to come and take part in the work [. . .]."
Requests for material support *tools, sacks, hoes, wheelbarrows, avenues*	"The municipality gave us hoes, machetes, spades, and sacks to improve our environment." "Volunteers brought sacks, and the work is progressing. We still need wheelbarrows, hoes, picks, and more sacks." "We now have the tools, and the people with strength to work are here. We can keep improving the neighborhood." **Resident:** "We have started salongo well, and now we are approaching the second week. We ask for help so that we can maintain our streets: we need tools and empty sacks. We will keep doing this voluntarily to maintain our neighborhood and our streets. We ask you to support us so that we can do this work."
Divine reward and solidarity *God, bless, reward, united, together*	"Salongo is group work, not one person's work. With God's support, let us have patience, peace, and gentleness." "Volunteers should not expect wages, but look to God, who rewards people according to their work." "A person reaps what they sow; God rewards each person according to their deeds." **Pastor:** "This work is voluntary and meant to help everyone. [. . .] It is not a person on earth who can pay us; it is God who will do so. I ask only for each person's willingness to work." **Resident:** "I liked what you said. [. . .] God will pay us, and let us respect the working hours, from nine until three." **Pastor, closing:** "[. . .] May God bless you, and may he also bless our group so that we can move forward."

Notes: The table shows why we gave the three topics their names. The left column lists words that frequently characterize each topic. The right column gives translated examples of chief and pastor speech that the model placed in that topic, followed by additional illustrative excerpts with speaker labels. These include residents' requests and responses to leaders. Excerpts are shortened for readability; ellipses mark internal omissions. The labelled exchanges illustrate the appeals and responses, rather than identifying causal effects of particular speeches. The topic model formed the categories from word patterns without using leader type or treatment assignment. The table is discussed in Section 5.3. Figure 8 shows the leaders' mean topic shares; Appendix Figure A-4 adds speaking time and separates the experimental environments; Appendix Figure A-5 shows how leader assignment changes ordinary participants' speech.

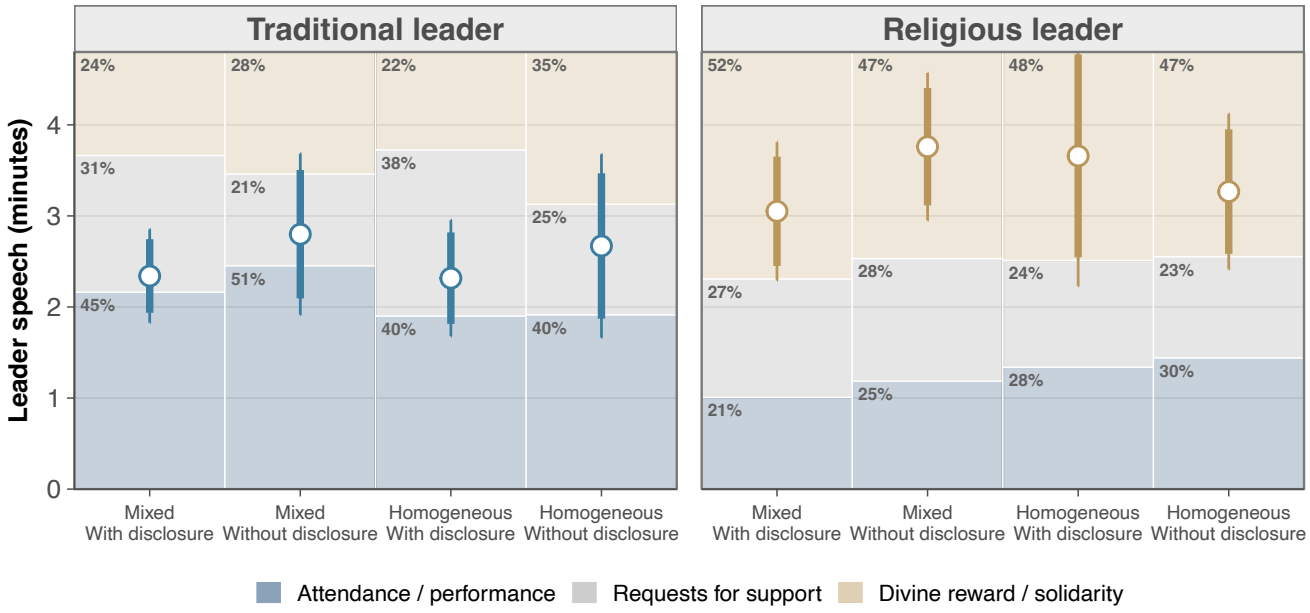


Figure A-4: TRADITIONAL LEADERS STRESS PERFORMANCE; RELIGIOUS LEADERS STRESS DIVINE REWARD ACROSS ENVIRONMENTS

Notes: Each panel describes what one leader type said during the first recorded meeting. Open circles show average total speaking time. The shaded column behind each circle divides that speech across the three topics, with the printed shares summing to 100 percent. Traditional Leaders devote more of their speech to attendance and performance, while religious leaders devote more to divine reward and solidarity; these profiles change little across the four environments. Read the percentages as shares of speech, not against the minutes axis. Thick and thin lines are 90 and 95 percent confidence intervals for speaking time. Figure 8 reports topic shares pooled across environments; Section 5.3 discusses the results. Table A-39 gives typical words and translated examples for each topic; Appendix Figure A-5 shows how leader assignment changes ordinary participants' speech.

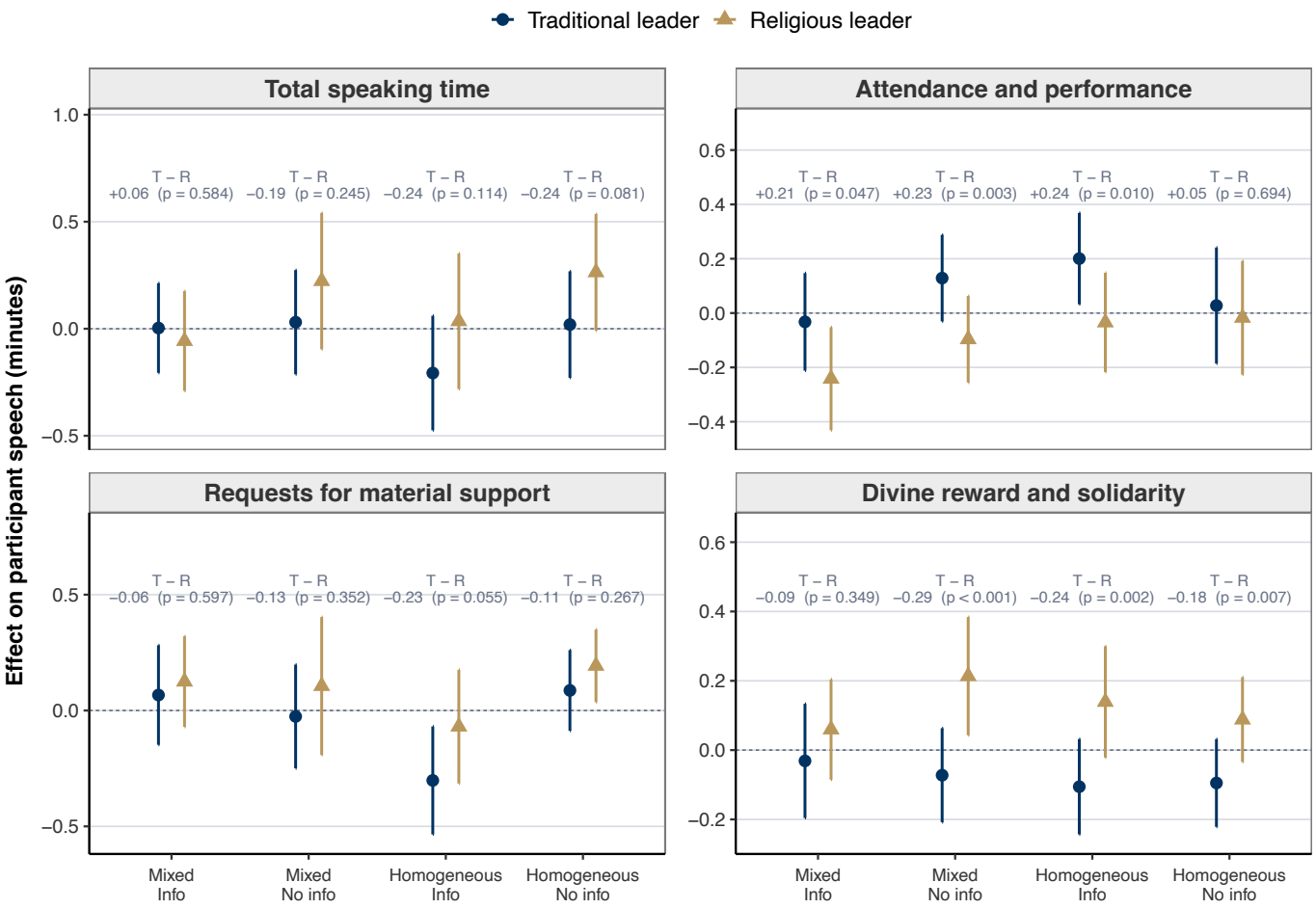


Figure A-5: LEADER ASSIGNMENT AND ORDINARY PARTICIPANTS' SPEECH

Notes: Each point shows how assigning a traditional leader or religious leader changes ordinary participants' speaking time relative to a no-leader group in the same environment. The four panels report total speaking time and time spent on each topic. Positive values mean that participants speak longer under the assigned leader. The T–R labels compare traditional leader and religious leader assignments directly. The topic pattern follows the leaders' own speech: traditional leader groups place relatively more emphasis on attendance and performance, while religious leader groups place relatively more emphasis on divine reward and solidarity. Silent participants count as zero speaking time. Lines are 95 percent confidence intervals. The figure is discussed in Section 5.3; Main Figure 8 and Table A-39 describe the leaders' speech.

Table A-40 – LEADER EFFECTS BY DAYS OF PRETREATMENT PARTICIPATION

	0–1 days	2–6 days	Difference
Panel A. Leader effects relative to no leader			
Traditional leader			
Mixed, without disclosure	-6.15	5.30	11.45
	(6.12)	(5.64)	(9.46)
Mixed, with disclosure	8.85	-6.60	-15.44
	(7.52)	(6.02)	(10.66)
Homogeneous, without disclosure	-9.27	7.92	17.19*
	(7.71)	(5.11)	(9.26)
Homogeneous, with disclosure	23.49***	-4.35	-27.84***
	(6.32)	(3.84)	(8.04)
Religious leader			
Mixed, without disclosure	6.09	9.92*	3.83
	(6.30)	(5.65)	(8.88)
Mixed, with disclosure	6.72	-2.33	-9.05
	(7.44)	(5.10)	(9.48)
Homogeneous, without disclosure	-2.64	6.40	9.04
	(7.39)	(5.28)	(10.23)
Homogeneous, with disclosure	0.42	1.93	1.52
	(5.33)	(4.44)	(7.14)
	0–1 days	2–6 days	Difference
Panel B. Implied no-leader counterfactuals			
Traditional leader			
Mixed, without disclosure	49.34	64.18	14.84*
	(4.83)	(4.50)	(7.80)
Mixed, with disclosure	44.95	71.98	27.02***
	(5.03)	(4.49)	(7.45)
Homogeneous, without disclosure	46.13	64.54	18.42**
	(5.68)	(4.25)	(7.79)
Homogeneous, with disclosure	34.81	70.75	35.94***
	(4.50)	(2.95)	(6.39)
Religious leader			
Mixed, without disclosure	49.64	62.41	12.76
	(4.79)	(4.52)	(7.82)
Mixed, with disclosure	42.76	73.24	30.48***
	(5.09)	(4.18)	(7.18)
Homogeneous, without disclosure	46.67	63.79	17.12**
	(5.85)	(4.28)	(8.05)
Homogeneous, with disclosure	38.79	65.34	26.56***
	(4.29)	(3.13)	(6.23)
Observations		1,040	
Group clusters		336	

Notes: Difference subtracts the first category from the second in each block. These are the estimates plotted in Figure 9 and discussed in Section 5.4. The categories are participation on 0–1 versus 2–6 eligible days before any meeting. The regression uses all 1,040 residents and 336 groups, baseline participation, neighborhood-by-composition-by-disclosure fixed effects, and separate pretreatment-category levels and leader interactions in every environment. No-leader predictions set the centered baseline control to zero and average the fixed-effect and category components over the treated residents supporting each displayed effect. Standard errors are clustered by group. Table A-41 reports additional-control and fixed-effect sensitivity. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In mixed groups, disclosure changes the implied no-leader comparison by -6.88 points (SE 6.73) among residents who participated on 0–1 days and 10.83 points (SE 5.60) among residents who participated on 2–6 days; the corresponding changes in the pastor effect are 0.63 (SE 9.75) and -12.25 (SE 7.59).

Table A-41 – LEADER EFFECTS BY RESPONDENT PRETREATMENT PARTICIPATION

	Neighborhood × environment fixed effects				Separate neighborhood and environment fixed effects		
	0–1 days	2–6 days	Difference		0–1 days	2–6 days	Difference
Panel A. Leader effects relative to no leader							
Traditional leader							
Mixed, without disclosure	-6.32	6.13	12.45	Mixed, without disclosure	-5.94	5.67	11.61
	(5.97)	(5.75)	(9.50)		(6.57)	(5.60)	(9.01)
Mixed, with disclosure	7.82	-6.94	-14.76	Mixed, with disclosure	8.12	-7.34	-15.46
	(7.08)	(5.52)	(9.89)		(8.55)	(5.08)	(10.15)
Homogeneous, without disclosure	-7.06	6.27	13.33	Homogeneous, without disclosure	-8.46	7.34	15.80*
	(7.50)	(5.48)	(9.32)		(7.82)	(5.32)	(9.14)
Homogeneous, with disclosure	22.00***	-2.47	-24.47***	Homogeneous, with disclosure	22.12***	-1.26	-23.37***
	(6.40)	(3.92)	(8.11)		(6.67)	(4.37)	(8.15)
Religious leader							
Mixed, without disclosure	4.98	10.81*	5.83	Mixed, without disclosure	5.58	10.51*	4.94
	(6.18)	(5.80)	(9.01)		(6.22)	(5.86)	(8.36)
Mixed, with disclosure	6.74	-1.93	-8.67	Mixed, with disclosure	3.66	1.65	-2.00
	(7.13)	(4.66)	(8.90)		(7.35)	(4.76)	(8.48)
Homogeneous, without disclosure	-1.73	5.61	7.34	Homogeneous, without disclosure	-3.92	7.27	11.20
	(7.32)	(5.14)	(9.97)		(7.72)	(4.88)	(9.60)
Homogeneous, with disclosure	0.12	2.29	2.17	Homogeneous, with disclosure	2.90	0.08	-2.82
	(5.37)	(4.43)	(7.06)		(5.80)	(4.70)	(7.03)
Panel B. Implied no-leader counterfactuals							
Traditional leader							
Mixed, without disclosure	50.21	62.94	12.73	Mixed, without disclosure	50.23	63.07	12.84*
	(4.62)	(4.47)	(7.73)		(4.75)	(4.32)	(6.97)
Mixed, with disclosure	46.08	71.11	25.02***	Mixed, with disclosure	46.18	71.03	24.85***
	(4.92)	(4.26)	(7.21)		(5.79)	(3.99)	(7.16)
Homogeneous, without disclosure	45.56	64.99	19.43**	Homogeneous, without disclosure	47.37	63.55	16.19**
	(5.57)	(4.17)	(7.67)		(5.71)	(3.96)	(7.31)
Homogeneous, with disclosure	35.44	70.11	34.67***	Homogeneous, with disclosure	35.74	68.54	32.80***
	(4.54)	(3.00)	(6.43)		(4.57)	(3.37)	(6.19)
Religious leader							
Mixed, without disclosure	50.43	61.28	10.86	Mixed, without disclosure	50.22	61.27	11.05
	(4.58)	(4.46)	(7.74)		(4.77)	(4.28)	(6.97)
Mixed, with disclosure	43.43	72.95	29.52***	Mixed, with disclosure	46.91	69.03	22.12***
	(4.90)	(3.87)	(6.75)		(5.75)	(3.80)	(6.92)
Homogeneous, without disclosure	46.11	64.24	18.13**	Homogeneous, without disclosure	48.66	62.19	13.53*
	(5.77)	(4.16)	(7.93)		(5.78)	(3.97)	(7.41)
Homogeneous, with disclosure	39.12	65.08	25.97***	Homogeneous, with disclosure	36.71	66.90	30.19***
	(4.29)	(3.20)	(6.25)		(4.49)	(3.34)	(6.03)
Observations		1,040				1,040	
Group-neighborhood clusters		336				336	
Respondents: 0–1 / 2–6 days		514 / 526				514 / 526	
Traditional-leader groups (0–1 / 2–6 days)		90 / 80				90 / 80	
Religious-leader groups (0–1 / 2–6 days)		93 / 95				93 / 95	
No-leader groups (0–1 / 2–6 days)		87 / 89				87 / 89	
Category effects by environment		Yes				Yes	

Notes: Difference subtracts the first category from the second in each block. This table asks whether leaders affect residents who previously participated on 0–1 days and those who participated on 2–6 days differently. Pretreatment participation is measured during the common week before any meeting. The columns distinguish participation on 0–1 versus 2–6 eligible days. Panel A reports the leader effect for both groups and their difference in every experimental environment. Panel B shows how often the same types of residents would participate without a leader, which separates differences in leader influence from residents' usual participation. The first block compares residents within the same neighborhood, composition, and disclosure condition. The second uses separate neighborhood and composition-by-disclosure fixed effects. Both use the same 1,040 residents and adjust for pretreatment participation, nearby treatment exposure, gender, and wealth group. Effects are percentage points, with standard errors in parentheses allowing for residents assigned to the same group. The table is discussed in Section 5.4; Figure 9 highlights chiefs in homogeneous groups and pastors in mixed groups. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-42 – LEADER EFFECTS BY OTHER GROUP MEMBERS' PRETREATMENT PARTICIPATION

	Neighborhood × environment fixed effects				Separate neighborhood and environment fixed effects		
	≤ 11.59 hours	> 11.59 hours	Difference		≤ 11.59 hours	> 11.59 hours	Difference
Panel A. Leader effects relative to no leader							
Traditional leader							
Mixed, without disclosure	-1.29	1.57	2.86	Mixed, without disclosure	-1.21	3.27	4.48
	(4.90)	(5.47)	(7.49)		(5.78)	(6.06)	(8.37)
Mixed, with disclosure	2.67	-0.64	-3.31	Mixed, with disclosure	5.28	-3.77	-9.05
	(6.27)	(6.49)	(9.56)		(7.73)	(6.60)	(10.36)
Homogeneous, without disclosure	-1.36	3.69	5.04	Homogeneous, without disclosure	-0.72	4.12	4.84
	(9.77)	(5.57)	(11.78)		(8.99)	(5.57)	(10.94)
Homogeneous, with disclosure	15.79***	3.57	-12.22*	Homogeneous, with disclosure	17.83***	5.18	-12.65
	(5.67)	(3.78)	(6.97)		(5.73)	(4.95)	(7.90)
Religious leader							
Mixed, without disclosure	11.39**	6.12	-5.27	Mixed, without disclosure	11.87**	6.05	-5.82
	(5.50)	(6.19)	(8.61)		(5.86)	(6.76)	(8.97)
Mixed, with disclosure	8.76	-2.27	-11.03	Mixed, with disclosure	6.07	0.80	-5.27
	(5.99)	(6.36)	(9.27)		(6.12)	(7.02)	(9.51)
Homogeneous, without disclosure	-0.01	3.49	3.51	Homogeneous, without disclosure	0.57	3.37	2.80
	(8.35)	(5.06)	(10.39)		(7.88)	(5.53)	(9.79)
Homogeneous, with disclosure	4.36	-3.21	-7.58	Homogeneous, with disclosure	8.63*	-5.38	-14.01*
	(5.33)	(5.12)	(7.93)		(5.05)	(6.10)	(8.27)
Panel B. Implied no-leader counterfactuals							
Traditional leader							
Mixed, without disclosure	52.70	61.45	8.75	Mixed, without disclosure	52.85	59.60	6.76
	(3.55)	(3.62)	(5.35)		(4.36)	(4.09)	(6.08)
Mixed, with disclosure	55.36	60.18	4.82	Mixed, with disclosure	52.93	63.14	10.21
	(4.68)	(5.11)	(7.76)		(4.60)	(5.56)	(7.38)
Homogeneous, without disclosure	48.44	62.74	14.30	Homogeneous, without disclosure	47.92	62.18	14.27*
	(7.76)	(3.37)	(8.91)		(7.17)	(3.37)	(8.21)
Homogeneous, with disclosure	43.46	64.15	20.70***	Homogeneous, with disclosure	41.66	62.36	20.70***
	(4.01)	(2.63)	(5.29)		(3.50)	(3.64)	(5.69)
Religious leader							
Mixed, without disclosure	53.40	57.67	4.27	Mixed, without disclosure	52.93	57.78	4.85
	(3.48)	(3.30)	(5.02)		(4.35)	(3.95)	(5.93)
Mixed, with disclosure	50.57	65.99	15.42**	Mixed, with disclosure	53.33	62.82	9.49
	(4.52)	(4.57)	(6.87)		(4.58)	(5.49)	(7.25)
Homogeneous, without disclosure	47.75	61.05	13.29	Homogeneous, without disclosure	47.18	61.11	13.93*
	(7.58)	(3.36)	(8.89)		(7.11)	(3.30)	(8.12)
Homogeneous, with disclosure	45.22	59.07	13.85**	Homogeneous, with disclosure	40.98	61.26	20.28***
	(3.91)	(3.03)	(5.71)		(3.47)	(3.65)	(5.72)
Observations		1,040				1,040	
Group-neighborhood clusters		336				336	
Respondents: ≤ 11.59 / > 11.59 hours		520 / 520				520 / 520	
Traditional-leader groups (≤ 11.59 / > 11.59 hours)		80 / 57				80 / 57	
Religious-leader groups (≤ 11.59 / > 11.59 hours)		86 / 76				86 / 76	
No-leader groups (≤ 11.59 / > 11.59 hours)		68 / 74				68 / 74	
Category effects by environment		Yes				Yes	

Notes: Difference subtracts the first category from the second in each block. Entries are percentage-point effects on average post-first-meeting salongo participation; group-neighborhood-clustered standard errors are in parentheses. Each block reports effects for the two hours categories and their difference from one interacted regression containing all eight leader-by-environment effects. The first block uses neighborhood-by-composition-by-disclosure fixed effects; the second block uses additive neighborhood and composition-by-disclosure fixed effects on the identical sample. In both blocks, the moderator is also interacted with each of the four composition-by-disclosure environments. Peer participation is the total pretreatment hours supplied by every other ordinary group member during the common week before any experimental meeting. It excludes the respondent and all actual or placebo leaders after group-person-day de-duplication. The threshold of 11.59 hours divides the sample into two equally sized categories: 520 respondents whose other group members supplied at most 11.59 hours and 520 whose other group members supplied more. The threshold was selected to make the two categories as similar in size as possible, keeping equal values together. Panel B reports coefficient-specific implied no-leader predictions from the same regression. For each leader-by-environment row and moderator group, it averages the fixed-effect and environment-specific moderator components over the actual treated observations supporting that coefficient, while setting centered covariates and all treatment components to zero. Its Difference column tests whether those subgroup-specific implied counterfactuals differ. All specifications include the ANCOVA pretreatment outcome, the 25m spillover indicator, and fixed gender and wealth-stratum controls. The sample is not split for estimation. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.5 Beyond Participation

Table A-43 – LEADER EFFECTS ON COOPERATION NORMS AND DIVINE MONITORING BELIEFS

Panel A. Evolution in the two environments where leaders increased participation

	Traditional leader: homogeneous, with disclosure				Religious leader: mixed, without disclosure			
Outcome	Meeting 1	Meeting 2	Endline	Pooled	Meeting 1	Meeting 2	Endline	Pooled
Active participants are well regarded	0.01	-0.11	-0.01	-0.03	-0.06	-0.10	-0.01	-0.06
	(0.08)	(0.10)	(0.10)	(0.07)	(0.09)	(0.10)	(0.12)	(0.08)
Contributing is a moral obligation	-0.09	-0.11	0.01	-0.06	-0.09	-0.00	0.04	-0.02
	(0.07)	(0.08)	(0.10)	(0.07)	(0.08)	(0.09)	(0.09)	(0.06)
God rewards community support	-0.03	-0.01	0.03	0.00	-0.09	-0.05	0.02	-0.04
	(0.06)	(0.08)	(0.07)	(0.06)	(0.06)	(0.07)	(0.08)	(0.05)
God punishes neglect of community obligations	-0.14	-0.09	-0.15	-0.13	-0.21	-0.05	-0.06	-0.11
	(0.15)	(0.14)	(0.10)	(0.09)	(0.15)	(0.13)	(0.13)	(0.10)
Family p-value	0.889	0.591	0.573	0.677	0.431	1.000	1.000	1.000

Panel B. Pooled effects in every leader environment

	Well regarded	Moral obligation	God rewards	God punishes	Family p-value
Traditional leader					
Mixed, without disclosure	0.04	-0.05	-0.04	-0.13	0.610
	(0.08)	(0.07)	(0.05)	(0.09)	
Mixed, with disclosure	0.14*	0.12**	0.08	0.21**	0.149
	(0.08)	(0.06)	(0.05)	(0.10)	
Homogeneous, without disclosure	-0.04	0.05	-0.05	0.02	1.000
	(0.07)	(0.08)	(0.05)	(0.08)	
Homogeneous, with disclosure	-0.03	-0.06	0.00	-0.13	0.677
	(0.07)	(0.07)	(0.06)	(0.09)	
Religious leader					
Mixed, without disclosure	-0.06	-0.02	-0.04	-0.11	1.000
	(0.08)	(0.06)	(0.05)	(0.10)	
Mixed, with disclosure	-0.06	0.04	-0.03	0.13	1.000
	(0.09)	(0.07)	(0.05)	(0.12)	
Homogeneous, without disclosure	-0.07	0.10	0.00	0.07	0.565
	(0.06)	(0.07)	(0.05)	(0.09)	
Homogeneous, with disclosure	0.01	0.00	0.02	-0.02	1.000
	(0.08)	(0.07)	(0.06)	(0.10)	

Notes: Counterfactual means for both panels appear on the continuation page of this table. The four cooperation-norm outcomes range from 1 (strongly disagree) to 5 (strongly agree). Non-substantive responses are coded missing. Each estimate is relative to no leader in the same composition-by-disclosure environment. Meeting 1 is elicited immediately after the first accountability meeting, Meeting 2 at the second meeting, and Endline after the recognition period. Wave-specific regressions control for the same outcome's baseline response, gender, and assignment wealth stratum and absorb neighborhood × disclosure × composition fixed effects. Pooled regressions stack the three surveys, absorb survey-wave × neighborhood × disclosure × composition fixed effects, and allow the baseline-response, gender, and wealth-stratum slopes to vary by wave. Standard errors in parentheses are clustered by group-neighborhood using the main paper's finite-sample correction. Family p-values apply the Bonferroni minimum-p test across the four displayed outcomes within each leader-environment and survey wave. Depending on item availability and don't-know responses, observations range from 983 at Meeting 1, 943–946 at Meeting 2, 965 at Endline, and 2891–2894 in pooled regressions. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (unadjusted).

Table A-43 – LEADER EFFECTS ON COOPERATION NORMS AND DIVINE MONITORING BELIEFS (continued)

Panel C. No-leader counterfactual means for the evolution in Panel A

	Homogeneous, with disclosure				Mixed, without disclosure			
Outcome	Meeting 1	Meeting 2	Endline	Pooled	Meeting 1	Meeting 2	Endline	Pooled
Well regarded	4.45	4.48	4.45	4.46	4.43	4.42	4.41	4.42
	(0.06)	(0.06)	(0.07)	(0.05)	(0.06)	(0.06)	(0.07)	(0.04)
Moral obligation	4.59	4.52	4.46	4.52	4.41	4.51	4.56	4.49
	(0.05)	(0.05)	(0.07)	(0.05)	(0.06)	(0.06)	(0.07)	(0.05)
God rewards	4.64	4.55	4.63	4.61	4.57	4.55	4.57	4.56
	(0.04)	(0.06)	(0.05)	(0.04)	(0.04)	(0.05)	(0.06)	(0.04)
God punishes	4.14	4.05	4.22	4.14	4.19	4.29	4.30	4.26
	(0.11)	(0.10)	(0.07)	(0.07)	(0.08)	(0.09)	(0.08)	(0.06)

Panel D. Pooled no-leader counterfactual means for Panel B

Environment	Well regarded	Moral obligation	God rewards	God punishes
Mixed, without disclosure	4.42	4.49	4.56	4.26
	(0.04)	(0.05)	(0.04)	(0.06)
Mixed, with disclosure	4.39	4.41	4.58	4.05
	(0.06)	(0.05)	(0.04)	(0.08)
Homogeneous, without disclosure	4.52	4.35	4.54	4.13
	(0.04)	(0.05)	(0.03)	(0.05)
Homogeneous, with disclosure	4.46	4.52	4.61	4.14
	(0.05)	(0.05)	(0.04)	(0.07)

Notes: The means use the same outcomes, estimation samples, controls, and fixed effects as the corresponding treatment estimates on the preceding page. They average model predictions over no-leader observations in each displayed environment and survey window, retaining those observations' covariates. The pooled means use the stacked survey sample. Both leader comparisons in Panel B use the same environment-specific reference. Parentheses report group-clustered standard errors.

Table A-44 – LEADER EFFECTS ON IDENTITY SALIENCE

Panel A. Evolution in the two environments where leaders increased participation

	Traditional leader: homogeneous, with disclosure				Religious leader: mixed, without disclosure			
Identity rank	Meeting 1	Meeting 2	Endline	Pooled	Meeting 1	Meeting 2	Endline	Pooled
Congolese	0.45*	0.20	0.05	0.23	-0.09	-0.26	0.06	-0.10
	(0.25)	(0.23)	(0.18)	(0.14)	(0.19)	(0.23)	(0.22)	(0.18)
Kasaien	0.09	-0.20	0.02	-0.03	0.06	0.18	0.21	0.15
	(0.23)	(0.20)	(0.21)	(0.14)	(0.19)	(0.24)	(0.20)	(0.15)
Christian	0.02	0.44**	-0.05	0.13	0.28	0.28	0.59**	0.38*
	(0.24)	(0.22)	(0.24)	(0.18)	(0.26)	(0.26)	(0.26)	(0.20)
Own territory	0.26	-0.05	0.45***	0.23*	-0.12	-0.15	-0.25	-0.17
	(0.18)	(0.20)	(0.17)	(0.13)	(0.23)	(0.22)	(0.21)	(0.16)
Own denomination	-0.31	-0.26	-0.14	-0.23	-0.27	-0.30	-0.20	-0.26*
	(0.24)	(0.24)	(0.22)	(0.19)	(0.21)	(0.20)	(0.18)	(0.14)
Own tribe	-0.49*	-0.12	-0.34	-0.32*	0.13	0.25	-0.42*	-0.01
	(0.26)	(0.23)	(0.21)	(0.18)	(0.19)	(0.21)	(0.23)	(0.16)
Family p-value	0.354	0.274	0.045	0.487	1.000	0.870	0.140	0.331

Panel B. Pooled effects in every leader environment

Traditional leader	Congolese	Kasaien	Christian	Own territory	Own denomination	Own tribe	Family p-value
Mixed, without disclosure	0.00	0.14	-0.11	-0.21	0.11	0.07	0.872
	(0.16)	(0.15)	(0.16)	(0.14)	(0.15)	(0.15)	
Mixed, with disclosure	0.17	0.20	-0.19	-0.02	-0.31**	0.16	0.258
	(0.12)	(0.14)	(0.15)	(0.16)	(0.15)	(0.13)	
Homogeneous, without disclosure	0.11	-0.05	0.07	-0.03	-0.19	0.13	1.000
	(0.16)	(0.11)	(0.18)	(0.12)	(0.15)	(0.13)	
Homogeneous, with disclosure	0.23	-0.03	0.13	0.23*	-0.23	-0.32*	0.487
	(0.14)	(0.14)	(0.18)	(0.13)	(0.19)	(0.18)	
Religious leader							
Mixed, without disclosure	-0.10	0.15	0.38*	-0.17	-0.26*	-0.01	0.331
	(0.18)	(0.15)	(0.20)	(0.16)	(0.14)	(0.16)	
Mixed, with disclosure	-0.05	-0.11	-0.02	0.18	0.02	-0.03	1.000
	(0.13)	(0.15)	(0.15)	(0.14)	(0.16)	(0.14)	
Homogeneous, without disclosure	0.06	0.04	0.15	-0.21*	-0.33**	0.25**	0.208
	(0.14)	(0.12)	(0.17)	(0.12)	(0.15)	(0.12)	
Homogeneous, with disclosure	0.07	0.10	0.30	0.06	-0.19	-0.34**	0.242
	(0.15)	(0.14)	(0.20)	(0.14)	(0.17)	(0.17)	

Notes: Counterfactual means for both panels appear on the continuation page of this table. Outcomes are reverse-coded salience ranks from a forced ranking of six identities; higher values indicate greater salience. Each estimate is relative to no leader in the same composition-by-disclosure environment. Meeting 1 is elicited immediately after the first accountability meeting, Meeting 2 at the second meeting, and Endline after the recognition period. Wave-specific regressions control for the same identity's baseline rank, gender, and assignment wealth stratum and absorb neighborhood × disclosure × composition fixed effects. Pooled regressions stack the three surveys, absorb survey-wave × neighborhood × disclosure × composition fixed effects, and allow the baseline-rank, gender, and wealth-stratum slopes to vary by wave. Standard errors in parentheses are clustered by group-neighborhood using the main paper's finite-sample correction. Family p-values apply the Bonferroni minimum-p test across the six ranks within the displayed leader-environment and survey wave; the ranks sum mechanically to 21, so no aggregate identity index is reported. Observations are 983 at Meeting 1, 947 at Meeting 2, 961 at Endline, and 2,891 in the pooled regressions. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (unadjusted).

Table A-44 – LEADER EFFECTS ON IDENTITY SALIENCE (continued)

Panel C. No-leader counterfactual means for the evolution in Panel A

	Homogeneous, with disclosure				Mixed, without disclosure			
Outcome	Meeting 1	Meeting 2	Endline	Pooled	Meeting 1	Meeting 2	Endline	Pooled
Congolese	3.80	4.25	4.21	4.09	4.27	4.38	4.35	4.33
	(0.17)	(0.15)	(0.13)	(0.10)	(0.14)	(0.15)	(0.14)	(0.12)
Kasaien	3.25	3.89	3.89	3.68	3.53	3.65	3.93	3.70
	(0.13)	(0.13)	(0.13)	(0.09)	(0.13)	(0.13)	(0.14)	(0.11)
Christian	4.04	3.58	3.66	3.75	3.74	3.71	3.28	3.58
	(0.17)	(0.16)	(0.16)	(0.14)	(0.16)	(0.18)	(0.17)	(0.12)
Own territory	3.19	3.18	2.90	3.09	3.18	3.50	3.57	3.41
	(0.11)	(0.12)	(0.12)	(0.08)	(0.14)	(0.15)	(0.14)	(0.11)
Own denomination	3.08	2.95	2.93	2.99	3.12	2.84	2.64	2.87
	(0.17)	(0.17)	(0.14)	(0.13)	(0.16)	(0.14)	(0.12)	(0.10)
Own tribe	3.64	3.15	3.40	3.40	3.15	2.92	3.23	3.10
	(0.18)	(0.14)	(0.16)	(0.13)	(0.13)	(0.12)	(0.15)	(0.10)

Panel D. Pooled no-leader counterfactual means for Panel B

Environment	Congolese	Kasaien	Christian	Own territory	Own denomination	Own tribe
Mixed, without disclosure	4.33	3.70	3.58	3.41	2.87	3.10
	(0.12)	(0.11)	(0.12)	(0.11)	(0.10)	(0.10)
Mixed, with disclosure	4.23	3.71	3.90	3.25	2.82	3.10
	(0.08)	(0.09)	(0.11)	(0.10)	(0.11)	(0.09)
Homogeneous, without disclosure	4.29	3.78	3.76	3.47	2.86	2.83
	(0.09)	(0.07)	(0.13)	(0.07)	(0.11)	(0.09)
Homogeneous, with disclosure	4.09	3.68	3.75	3.09	2.99	3.40
	(0.10)	(0.09)	(0.14)	(0.08)	(0.13)	(0.13)

Notes: The means use the same outcomes, estimation samples, controls, and fixed effects as the corresponding treatment estimates on the preceding page. They average model predictions over no-leader observations in each displayed environment and survey window, retaining those observations' covariates. The pooled means use the stacked survey sample. Both leader comparisons in Panel B use the same environment-specific reference. Parentheses report group-clustered standard errors.

Table A-45 – GOVERNANCE AND INSTITUTIONAL OUTCOMES: TESTS BY OUTCOME FAMILY

		Smallest adjusted q in the two mobilizing environments		Rejections at $q < 0.10$	
Outcome family	Outcomes	Chief: homogeneous, disclosure	Pastor: mixed, no disclosure	All leader effects	All contrasts
Government service performance	5	0.858	0.858	0/60	0/40
Government accountability and knowledge	4	0.849	0.608	0/48	0/32
Performance: public and community authorities	4	0.901	0.901	0/48	0/32
Performance: other authorities	4	0.638	0.638	0/48	0/32
Expected assistance from the locality chief	3	0.812	0.686	0/36	0/24
Local chief service performance	5	0.561	0.999	0/60	0/40
Local chief accountability and capture	2	0.664	0.664	0/24	0/16
Trust in public and local authorities	5	0.304	0.304	0/60	0/40
Trust in religious and civil-society actors	4	0.641	0.641	0/48	0/32
All governance families	36	0.304	0.304	0/432	0/288

Notes: The table compresses 36 baseline–endline outcomes in nine families. Every underlying ANCOVA controls for the corresponding baseline outcome, gender, and wealth stratum and absorbs neighborhood × disclosure × composition fixed effects; standard errors are clustered at the randomized group–neighborhood level using the main-paper finite-sample correction. The table reports the smallest Benjamini–Hochberg adjusted q-value within each mobilizing treatment cell and outcome family. Adjustment is performed separately within each thematic outcome family for all leader effects and all treatment-effect contrasts; it is never pooled across the nine families or across effects and contrasts. The last two columns report adjusted rejections divided by tests across all leader effects and all treatment-effect contrasts, respectively. The audited battery excludes predetermined chief ties, mechanically complementary diversion allocations, and nominal provider categories that do not form ordered outcomes. Appeal frequency is recoded monotonically, institutional-performance items are oriented so higher values indicate better performance, and refusals on name-knowledge items are treated as missing. No estimate in the 720-test audited battery survives adjustment at $q < 0.10$.

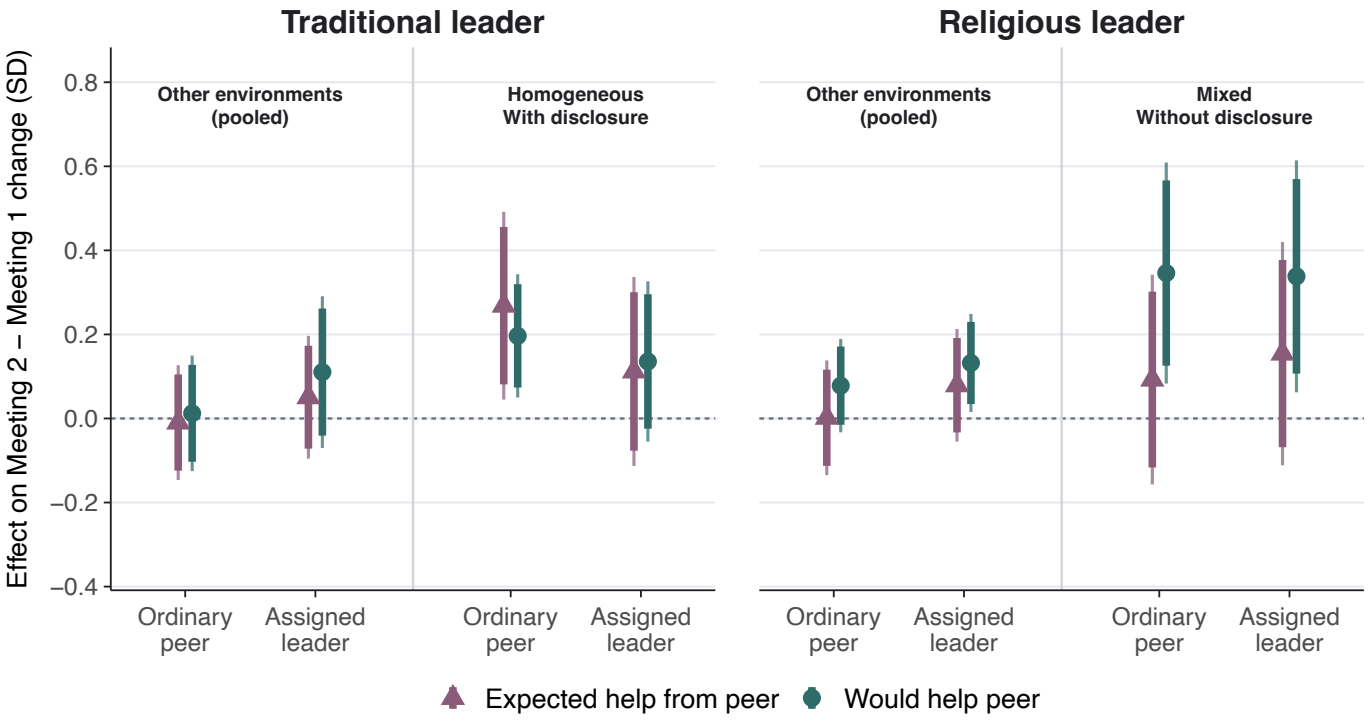


Figure A-6: WILLINGNESS TO HELP AND EXPECTED HELP WITHIN THE GROUP

Notes: Before the first discussion and again at the start of Meeting 2, residents reported how much they would help a named group member and how much help they expected from that person. The figure shows how leader assignment changes those answers over time. The left panel reports traditional leaders and the right panel religious leaders. Within each panel, the right block isolates the environment where that leader raises participation; the left block pools the other three environments. Circles show changes in willingness to help and triangles changes in expected help. The named person is either an ordinary peer or the assigned leader. The more positive changes in the effective environments show that successful mobilization is accompanied by stronger helping relationships within the group. The vertical scale measures change in baseline standard deviations; thick and thin lines are 90 and 95 percent confidence intervals. Figure 10 reports willingness to help in the main text. In homogeneous groups with disclosure, traditional leader assignment raises expected help from an ordinary peer by 0.27 standard deviations, corresponding to about 2,700 Congolese francs or 59 percent of the baseline mean. Section 6 discusses the relationship outcomes; Appendix Tables A-46 and A-47 report all outcomes and environments.

Table A-46 – ENDPOINT CHANGES IN OPINIONS ABOUT ORDINARY PEERS

	Inspires respondent	Moral example	Enforcement authority	Trusts responsibility	Would help target	Expected help from target
Panel A. Leader effects by composition						
Traditional leader						
Mixed	0.04	0.11	0.09	0.14	0.12	0.02
	(0.09)	(0.09)	(0.08)	(0.10)	(0.09)	(0.09)
Homogeneous	0.14	0.08	0.04	-0.09	0.01	0.11
	(0.08)	(0.08)	(0.08)	(0.09)	(0.07)	(0.09)
Religious leader						
Mixed	0.20*	0.05	0.08	0.12	0.24**	0.05
	(0.09)	(0.08)	(0.08)	(0.09)	(0.09)	(0.10)
Homogeneous	0.05	0.08	0.09	-0.07	0.05	-0.00
	(0.09)	(0.08)	(0.08)	(0.10)	(0.07)	(0.08)
Panel B. Environment-specific leader effects						
Traditional leader						
Mixed, without disclosure	0.14	0.27	0.14	0.08	0.18	0.13
	(0.12)	(0.12)	(0.10)	(0.14)	(0.14)	(0.12)
Mixed, with disclosure	-0.07	-0.06	0.04	0.20	0.04	-0.10
	(0.12)	(0.12)	(0.14)	(0.14)	(0.12)	(0.13)
Homogeneous, without disclosure	0.10	0.05	0.07	-0.16	-0.21	-0.11
	(0.12)	(0.11)	(0.12)	(0.13)	(0.10)	(0.12)
Homogeneous, with disclosure	0.17	0.10	0.01	-0.02	0.21**	0.32**
	(0.11)	(0.11)	(0.10)	(0.12)	(0.08)	(0.13)
Religious leader						
Mixed, without disclosure	0.25	0.06	0.16	0.20	0.43**	0.15
	(0.12)	(0.10)	(0.10)	(0.13)	(0.15)	(0.14)
Mixed, with disclosure	0.15	0.04	0.01	0.03	0.06	-0.04
	(0.13)	(0.13)	(0.14)	(0.13)	(0.09)	(0.14)
Homogeneous, without disclosure	0.13	0.11	0.12	-0.11	-0.02	-0.13
	(0.12)	(0.11)	(0.11)	(0.14)	(0.10)	(0.12)
Homogeneous, with disclosure	-0.04	0.05	0.06	-0.03	0.11	0.13
	(0.12)	(0.11)	(0.12)	(0.14)	(0.10)	(0.11)
Panel C. No-leader counterfactual means						
Mixed, pooled across disclosure	0.21	0.21	0.01	0.00	-0.14	-0.08
	(0.06)	(0.05)	(0.05)	(0.05)	(0.07)	(0.06)
Homogeneous, pooled across disclosure	0.11	0.13	-0.04	-0.02	-0.09	-0.08
	(0.05)	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)
Mixed, without disclosure	0.12	0.13	0.03	-0.06	-0.21	-0.13
	(0.07)	(0.06)	(0.06)	(0.08)	(0.11)	(0.09)
Mixed, with disclosure	0.29	0.29	-0.02	0.07	-0.06	-0.02
	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)
Homogeneous, without disclosure	0.08	0.12	-0.07	0.03	-0.01	-0.00
	(0.07)	(0.06)	(0.07)	(0.08)	(0.06)	(0.07)
Homogeneous, with disclosure	0.15	0.15	-0.01	-0.07	-0.18	-0.17
	(0.07)	(0.06)	(0.07)	(0.08)	(0.06)	(0.08)
Respondent–target pairs	2,632	2,631	2,633	2,633	2,633	2,633
Ordinary-target pairs	2,071	2,071	2,072	2,072	2,072	2,072
Assigned-leader-target pairs	561	560	561	561	561	561
Respondents	885	885	885	885	885	885
Group-neighborhood clusters	323	323	323	323	323	323
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline outcome control	No	No	No	No	No	No

Notes: Panel C reports model-implied endpoint changes for ordinary targets in no-leader groups, in the same baseline-standard-deviation units as the effects. Predictions are averaged over the matched no-leader respondent–target pairs in each composition or environment. These are the common counterfactuals for both the assigned-leader and ordinary-peer effects. This table shows how residents' opinions of ordinary teammates changed between the start of Meeting 1, before any discussion, and the start of Meeting 2. Each outcome is the later answer minus the earlier answer for the same respondent and teammate, expressed in baseline standard deviations. Expected-help amounts are transformed before the change is calculated. Panel A pools the disclosure conditions and compares mixed with homogeneous groups. Panel B reports all four environments. Every effect is relative to opinions of ordinary teammates in no-leader groups under the same experimental conditions. The companion Table A-47 uses the same model to report changes in opinions of assigned leaders. Standard errors in parentheses allow for people evaluated within the same randomized group. The table is discussed in Section 6; Figure 10 highlights the two help outcomes. Stars use Benjamini–Hochberg adjusted p-values across the six ordinary-peer outcomes for each treatment comparison: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-47 – ENDPOINT CHANGES IN OPINIONS ABOUT ASSIGNED LEADERS

	Inspires respondent	Moral example	Enforcement authority	Trusts responsibility	Would help target	Expected help from target
Panel A. Leader effects by composition						
Traditional leader						
Mixed	0.05	0.02	-0.02	0.19	0.08	0.07
	(0.11)	(0.10)	(0.08)	(0.10)	(0.12)	(0.10)
Homogeneous	0.03	-0.02	-0.04	0.12	0.16	0.07
	(0.08)	(0.08)	(0.08)	(0.10)	(0.08)	(0.08)
Religious leader						
Mixed	0.07	0.08	0.09	0.23*	0.32***	0.17
	(0.09)	(0.08)	(0.09)	(0.10)	(0.10)	(0.10)
Homogeneous	0.11	0.00	0.08	0.16	0.04	0.02
	(0.09)	(0.08)	(0.09)	(0.11)	(0.06)	(0.08)
Panel B. Environment-specific leader effects						
Traditional leader						
Mixed, without disclosure	0.18	0.09	0.03	0.32	0.28	0.12
	(0.15)	(0.16)	(0.12)	(0.15)	(0.16)	(0.14)
Mixed, with disclosure	-0.09	-0.05	-0.09	0.06	-0.14	0.01
	(0.14)	(0.13)	(0.10)	(0.13)	(0.17)	(0.14)
Homogeneous, without disclosure	0.15	-0.04	0.13	0.14	0.18	-0.02
	(0.11)	(0.11)	(0.11)	(0.14)	(0.14)	(0.11)
Homogeneous, with disclosure	-0.08	-0.01	-0.19	0.11	0.15	0.16
	(0.11)	(0.11)	(0.12)	(0.15)	(0.10)	(0.13)
Religious leader						
Mixed, without disclosure	0.15	0.10	0.14	0.22	0.42**	0.21
	(0.11)	(0.09)	(0.13)	(0.13)	(0.16)	(0.15)
Mixed, with disclosure	-0.01	0.06	0.03	0.24	0.22	0.14
	(0.15)	(0.13)	(0.13)	(0.16)	(0.12)	(0.13)
Homogeneous, without disclosure	0.16	0.05	0.14	0.03	-0.06	-0.10
	(0.12)	(0.10)	(0.14)	(0.15)	(0.10)	(0.10)
Homogeneous, with disclosure	0.06	-0.04	0.01	0.29	0.15	0.14
	(0.13)	(0.12)	(0.13)	(0.15)	(0.07)	(0.14)
Panel C. No-leader counterfactual means						
Mixed, pooled across disclosure	0.21	0.21	0.01	0.00	-0.14	-0.08
	(0.06)	(0.05)	(0.05)	(0.05)	(0.07)	(0.06)
Homogeneous, pooled across disclosure	0.11	0.13	-0.04	-0.02	-0.09	-0.08
	(0.05)	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)
Mixed, without disclosure	0.12	0.13	0.03	-0.06	-0.21	-0.13
	(0.07)	(0.06)	(0.06)	(0.08)	(0.11)	(0.09)
Mixed, with disclosure	0.29	0.29	-0.02	0.07	-0.06	-0.02
	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.09)
Homogeneous, without disclosure	0.08	0.12	-0.07	0.03	-0.01	-0.00
	(0.07)	(0.06)	(0.07)	(0.08)	(0.06)	(0.07)
Homogeneous, with disclosure	0.15	0.15	-0.01	-0.07	-0.18	-0.17
	(0.07)	(0.06)	(0.07)	(0.08)	(0.06)	(0.08)
Respondent–target pairs	2,632	2,631	2,633	2,633	2,633	2,633
Ordinary-target pairs	2,071	2,071	2,072	2,072	2,072	2,072
Assigned-leader-target pairs	561	560	561	561	561	561
Respondents	885	885	885	885	885	885
Group-neighborhood clusters	323	323	323	323	323	323
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes	Yes	Yes
Baseline outcome control	No	No	No	No	No	No

Notes: Panel C reports model-implied endpoint changes for ordinary targets in no-leader groups, in the same baseline-standard-deviation units as the effects. Predictions are averaged over the matched no-leader respondent–target pairs in each composition or environment. These are the common counterfactuals for both the assigned-leader and ordinary-peer effects. This table shows how residents' opinions of their assigned chief or pastor changed between the start of Meeting 1, before any discussion, and the start of Meeting 2. Each outcome is the later answer minus the earlier answer for the same respondent and leader, expressed in baseline standard deviations. Expected-help amounts are transformed before the change is calculated. Panel A pools the disclosure conditions and compares mixed with homogeneous groups. Panel B reports all four environments. Every effect is measured relative to opinions of ordinary teammates in no-leader groups under the same experimental conditions. The companion Table A-46 uses the same model to report changes in opinions of ordinary teammates. Standard errors in parentheses allow for people evaluated within the same randomized group. The table is discussed in Section 6; Figure 10 highlights the two help outcomes. Stars use Benjamini–Hochberg adjusted p-values across the six assigned-leader outcomes for each treatment comparison: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Fieldwork continuation after Wave 8

On Friday, May 15, 2026, the Democratic Republic of the Congo declared a Bundibugyo Ebola outbreak after laboratory confirmation in Ituri Province; on May 17, the World Health Organization (WHO) determined that the event constituted a Public Health Emergency of International Concern. Wave 8 was underway when the outbreak happened. Waves 9 and 10 were scheduled for the upcoming months of June, July, and August and would have required the repeated convening of new groups for collective work and program meetings. The institutional response to the outbreak underscored the risks of initiating such activities. WHO recommended considering the postponement of mass gatherings; UN staff in Bunia were instructed to work from home and avoid physical contact and crowded areas; the U.S. Department of State temporarily suspended all visa services at its embassy in Kinshasa; and the African Development Bank introduced remote participation for its Annual Meetings in neighboring Congo while explicitly monitoring risks from the outbreak, despite no confirmed or suspected cases in the host country. Initiating additional waves under these conditions was unwarranted and posed severe ethical risks. We therefore decided to stop the study on May 30, 2026, interrupting the implementation of the upcoming cohorts.[19] The emergency overlapped Wave 8 implementation. At the time of the declaration, Wave 8 was only 30.4 percent complete.

For those reasons, the paper's main sample uses Waves 1–7. Companion estimates reported in this appendix add disrupted Wave 8 observations. The appendix proceeds in three steps. Section B.1 compares predetermined characteristics of early- and late-wave neighborhoods. Section B.2 reassesses power under the planned ten-wave design and the seven waves completed before the declaration. Section B.3 reports matched Waves 1–7 and Waves 1–8 estimates, outcome-window checks, and one-wave-deletion diagnostics.

B.1 Predetermined neighborhood characteristics

The ten-wave rollout order was randomly assigned before implementation. Table B-48 compares property values, building size and counts, and commune composition for neighborhoods assigned to Waves 1–7 and Waves 8–10.

None of the 12 characteristic-specific tests rejects at the 5 percent level. The exact max-statistic test across all 12 characteristics yields $p = 0.787$. The available predetermined characteristics therefore provide no evidence of systematic differences between early- and late-wave neighborhoods. The balance assessment has limited power to establish equivalence, and unmeasured cohort differences remain possible. Within each realized neighborhood, treatment remains randomized, and the specifications absorb neighborhood × disclosure × composition fixed effects.

B.2 Power under ten planned waves and seven pre-declaration waves

Figure B-7 reassesses expected precision for the planned ten-wave design and for Waves 1–7, the sample completed before the national declaration. The calibration data come from the municipal field pilot conducted before July 1, 2025, well before the current experiment. The sample contains 251 participants from four pilot neighborhoods with a six-day baseline week followed by a separate six-day outcome week. Ngdilayi, an earlier pilot neighborhood, is excluded because its calendar lacks this consecutive-week structure. The preregistration set a minimum detectable effect (MDE) of 0.20 outcome standard deviations and planned 40 neighborhoods and 480 groups. The reassessment keeps that effect-size threshold and applies the group sizes used in each wave: no-leader groups contain four ordinary participants in Waves 1–2 and three thereafter, while traditional- and religious-leader groups contain three ordinary participants in every wave. The seven-wave design therefore has 1,040 participant observations in 336 groups, and the ten-wave design has 1,472 observations in 480 groups. Every simulation estimates the paper's model with neighborhood × disclosure × composition fixed effects and baseline participation; standard errors are clustered by group within neighborhood.

At the preregistered MDE of 0.20 outcome standard deviations (7.95 percentage points), power for the T_H and R_M leader effects ranges from 0.902 to 0.902 under ten waves and from 0.791 to 0.793 under seven waves. For the T_{H1} and R_{M0} effects, the corresponding ranges are 0.646 to 0.647 and 0.514 to 0.518. Power for the composition contrast Δ_C is 0.620 under ten waves and 0.478 under seven waves. Power for the environment contrast Δ_E is 0.364 and 0.276 under the two designs. Across the four leader effects and two delta contrasts, moving from ten to seven waves raises simulated standard errors by 17–19 percent. The ten-wave plan would

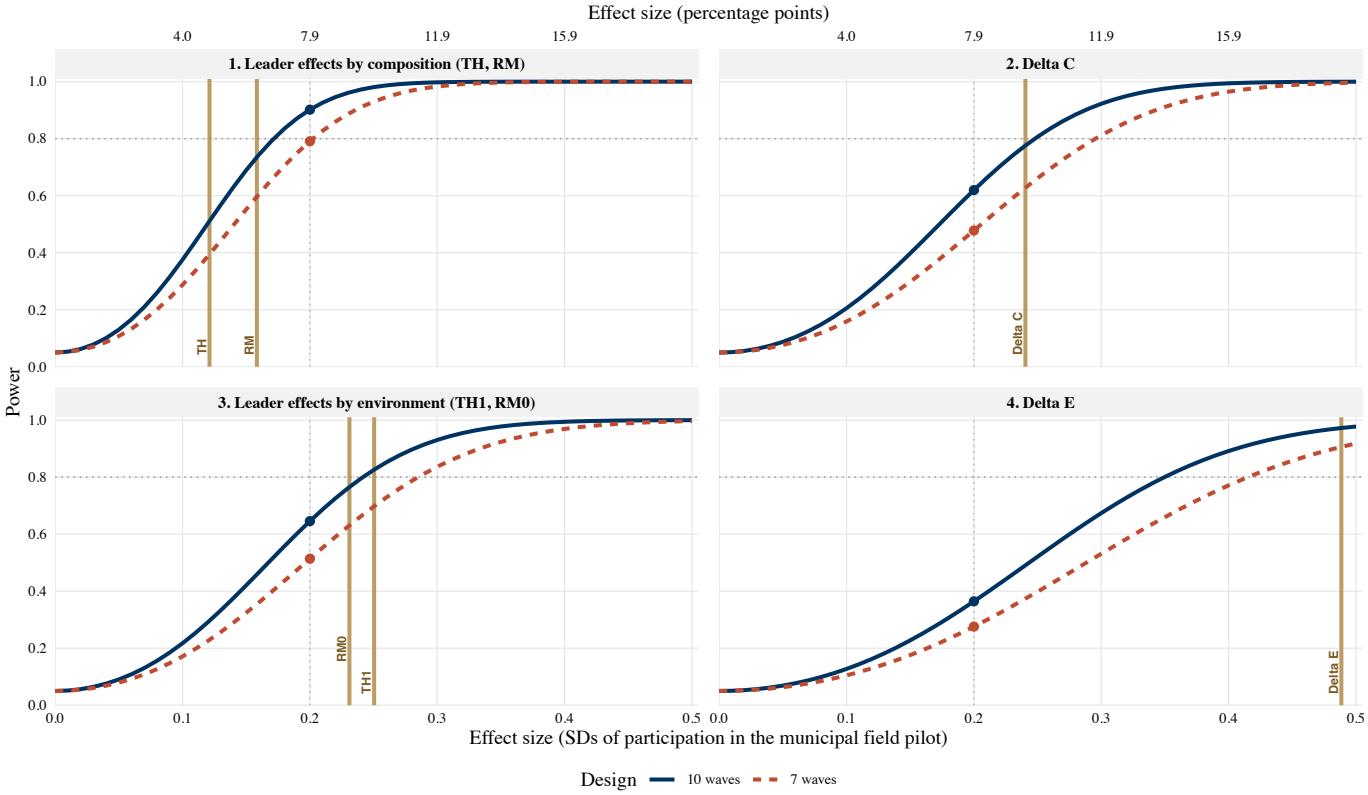
[19] WHO, Disease Outbreak News, May 16, 2026; WHO, PHEIC statement, May 17, 2026; Associated Press, May 18, 2026; U.S. Department of State, May 18, 2026; African Development Bank, May 21, 2026; and WHO, Disease Outbreak News, July 3, 2026.

Table B-48 – PREDETERMINED NEIGHBORHOOD CHARACTERISTICS BY PLANNED WAVE

	Waves 1–7	Waves 8–10	Difference	Std. diff.	RI p
Mean predicted property value (USD)	2612.09	2183.94	-428.15	-0.47	0.358
Median predicted property value (USD)	1359.32	1165.25	-194.07	-0.66	0.187
Mean building area	39.70	38.13	-1.57	-0.34	0.518
Median building area	34.59	33.50	-1.09	-0.35	0.496
Total built area	19261.66	18624.36	-637.30	-0.12	0.810
Number of main properties	313.86	322.00	8.14	0.15	0.769
Number of buildings	484.82	473.17	-11.65	-0.12	0.803
Commune: Kananga (share)	0.11	0.25	0.14	0.50	0.429
Commune: Katoka (share)	0.21	0.08	-0.13	-0.39	0.500
Commune: Lukonga (share)	0.36	0.42	0.06	0.13	1.000
Commune: Ndesha (share)	0.07	0.08	0.01	0.06	1.000
Commune: Nganza (share)	0.25	0.17	-0.08	-0.20	0.868
Joint max-statistic test			Maximum |standardized difference| = 0.66		0.787

Notes: The sample contains the 40 treated neighborhoods assigned ex ante to ten rollout waves: 28 neighborhoods in Waves 1–7 and 12 in Waves 8–10. Each row reports the early-wave mean, late-wave mean, their difference (late minus early), the difference standardized by the across-pair standard deviation, and a two-sided exact randomization-inference p-value. Wave assignment occurred at the neighborhood-pair level. The randomization distribution exhausts all 15,015 assignments that place two of six placebo pairs and four of fourteen non-placebo pairs in Waves 8–10. Commune rows are neighborhood shares. The final row reports the randomization p-value for the maximum absolute standardized difference across all 12 predetermined characteristics.

have improved precision. Power at the 0.20-SD effect size would remain below 80 percent for T_{H1}, R_{M0}, Δ_C, and Δ_E. The seven-wave curves correspond to the Waves 1–7 main analysis; Section B.3 reports the same estimands after adding all Wave 8 observations. Gold bars show the magnitudes of the Waves 1–7 estimates for scale.



Dotted line: preregistered MDE benchmark (0.20 SD); gold bars: magnitudes of the labeled Waves 1–7 estimates

Figure B-7 – POWER FOR LEADER EFFECTS AND THE PAPER'S COMPOSITION AND ENVIRONMENT CONTRASTS

Notes: T_H is the traditional-leader effect in homogeneous groups, and R_M is the religious-leader effect in mixed groups. T_{H1} is the traditional-leader effect in homogeneous groups under disclosure, and R_{M0} is the religious-leader effect in mixed groups without disclosure. The four panels report power for (1) T_H and R_M, (2) $\Delta_C = (\tau_H^T - \tau_M^T) - (\tau_H^R - \tau_M^R)$, (3) T_{H1} and R_{M0}, and (4) $\Delta_E = (\tau_{HI}^T - \tau_{MN}^T) - (\tau_{HI}^R - \tau_{MN}^R)$. Panels 1 and 3 plot the lower-power curve across their two stated cells; Panels 2 and 4 use the full clustered covariance matrix for the stated linear contrast. The bottom axis reports standardized effects; the top axis translates them into participation percentage points using the outcome standard deviation from the municipal field pilot. The horizontal dotted line marks 80% power, and the vertical dotted line marks the preregistered 0.20-standard-deviation MDE benchmark (7.95 percentage points). Gold bars mark the magnitudes of the Waves 1–7 T_H and R_M estimates in Panel 1, Δ_C in Panel 2, T_{H1} and R_{M0} in Panel 3, and Δ_E in Panel 4. The ten- and seven-wave SEs are, respectively, 0.061–0.061 SD (2.42–2.43 percentage points) and 0.072–0.072 SD (2.85–2.86 percentage points) for T_H and R_M; 0.088–0.088 SD (3.51–3.51 percentage points) and 0.105–0.105 SD (4.17–4.17 percentage points) for Δ_C; 0.086–0.086 SD (3.41–3.41 percentage points) and 0.101–0.101 SD (4.00–4.02 percentage points) for T_{H1} and R_{M0}; and 0.124–0.124 SD (4.94–4.94 percentage points) and 0.148–0.148 SD (5.86–5.86 percentage points) for Δ_E. The outcome and pretreatment covariate are separate consecutive six-day participation weeks drawn from the municipal field pilot conducted before July 1, 2025. The simulation applies the group sizes implemented in the randomization scripts and uses 1,000 bootstrap designs; the largest Monte Carlo standard error among the reported power calculations is 0.003. Every simulated ANCOVA absorbs neighborhood × disclosure × composition fixed effects, identifies leader effects within the corresponding fixed-effect cell, and controls for baseline-week participation. Standard errors cluster by group within neighborhood using the paper's small-sample correction.

B.3 Comparison of Waves 1–7 and Waves 1–8

Table B-49 – ENVIRONMENT-SPECIFIC ESTIMATES IN WAVES 1–7 AND WAVES 1–8

	Waves 1–7			Waves 1–8		
	Baseline adjustment	+ Spillover	+ Gender/wealth	Baseline adjustment	+ Spillover	+ Gender/wealth
Panel A. Leader effects relative to no leader						
Traditional leader						
Mixed, without disclosure	-0.29	-0.31	0.18	0.45	0.45	1.12
	(3.48)	(3.48)	(3.39)	(3.39)	(3.39)	(3.28)
Mixed, with disclosure	1.75	1.67	0.94	5.53	5.35	5.06
	(4.53)	(4.52)	(4.24)	(4.32)	(4.32)	(4.16)
Homogeneous, without disclosure	-0.31	-0.34	-0.18	-3.47	-3.65	-3.92
	(4.46)	(4.45)	(4.47)	(4.43)	(4.44)	(4.55)
Homogeneous, with disclosure	9.95***	10.13***	10.06***	7.63**	7.89**	7.86**
	(3.63)	(3.63)	(3.63)	(3.73)	(3.72)	(3.64)
Religious leader						
Mixed, without disclosure	9.18**	9.16**	9.10**	6.80*	6.88*	7.01*
	(3.97)	(3.98)	(3.93)	(3.78)	(3.79)	(3.72)
Mixed, with disclosure	3.41	3.40	3.56	5.57	5.51	5.64
	(4.30)	(4.32)	(4.09)	(4.01)	(4.04)	(3.88)
Homogeneous, without disclosure	1.66	1.67	1.59	3.29	3.21	3.17
	(3.81)	(3.80)	(3.80)	(3.75)	(3.74)	(3.80)
Homogeneous, with disclosure	0.01	-0.01	0.08	-0.72	-0.72	-0.78
	(3.27)	(3.26)	(3.32)	(3.13)	(3.13)	(3.13)
Panel B. Treatment-effect contrasts						
Mixed, with: T − R	-1.66	-1.73	-2.61	-0.04	-0.16	-0.58
	(4.40)	(4.41)	(4.06)	(4.15)	(4.17)	(3.96)
Mixed, without: T − R	-9.47**	-9.47**	-8.92**	-6.34*	-6.43*	-5.90
	(3.98)	(4.01)	(4.08)	(3.84)	(3.87)	(3.92)
Homogeneous, with: T − R	9.94**	10.13**	9.98**	8.35**	8.60**	8.64**
	(4.03)	(4.00)	(4.05)	(3.98)	(3.96)	(3.93)
Homogeneous, without: T − R	-1.97	-2.01	-1.77	-6.77	-6.86	-7.09
	(4.13)	(4.12)	(4.26)	(4.30)	(4.28)	(4.48)
Δ_E: H, with − M, without	19.41***	19.60***	18.90***	14.69***	15.04***	14.54***
	(5.67)	(5.69)	(5.78)	(5.55)	(5.56)	(5.58)
Observations	1,040	1,040	1,040	1,184	1,184	1,184
Group-neighborhood clusters	336	336	336	384	384	384
Neighborhoods	28	28	28	32	32	32
Neighborhood × disclosure × composition FEs	Yes	Yes	Yes	Yes	Yes	Yes
Pretreatment participation control	Yes	Yes	Yes	Yes	Yes	Yes
25m spillover control	No	Yes	Yes	No	Yes	Yes
Gender and wealth-stratum controls	No	No	Yes	No	No	Yes

Notes: Panel C on the continuation page reports counterfactual means for all six specifications. This table reproduces Panels A and B of the environment-specific half of the main table for Waves 1–7 and the pooled Waves 1–8 sample. The unit is a collapsed respondent–treatment-cell observation; the outcome is average salongo participation on six eligible days strictly after Meeting 1 through the Meeting 2 day, in percentage points. Panel A reports leader effects relative to no leader; Panel B reports traditional-minus-religious contrasts and Δ_E. All columns include the ANCOVA pretreatment-participation control and neighborhood × disclosure × composition fixed effects. The spillover columns add the 25m exposure indicator. The columns labeled + Gender/wealth add fixed gender and wealth-stratum controls in both wave windows. Standard errors are clustered by group within neighborhood. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B-49 – ENVIRONMENT-SPECIFIC ESTIMATES IN WAVES 1–7 AND WAVES 1–8 (continued)

	Waves 1–7			Waves 1–8		
	Baseline adjustment	+ Spillover	+ Gender/wealth	Baseline adjustment	+ Spillover	+ Gender/wealth
Panel C. No-leader counterfactual means						
Mixed, without disclosure	58.88	58.88	58.88	57.53	57.53	57.53
	(2.46)	(2.44)	(2.27)	(2.36)	(2.34)	(2.18)
Mixed, with disclosure	57.79	57.79	57.79	54.65	54.65	54.65
	(3.14)	(3.14)	(3.01)	(2.97)	(2.97)	(2.89)
Homogeneous, without disclosure	57.79	57.79	57.79	57.69	57.69	57.69
	(2.91)	(2.91)	(2.85)	(2.74)	(2.75)	(2.72)
Homogeneous, with disclosure	55.80	55.80	55.80	56.09	56.09	56.09
	(1.96)	(1.98)	(2.00)	(1.97)	(1.99)	(1.96)

Notes: These are the model-implied no-leader participation means, in percentage points, for the six specifications on the preceding page. Predictions are averaged over the no-leader observations in each environment in the corresponding column's estimation sample. Parentheses report group-clustered standard errors.

Table B-49 compares Waves 1–7 with all eight collected waves while holding the estimator fixed. The columns labeled + Gender/wealth use fixed gender and wealth-stratum controls in both samples. The Waves 1–8 columns retain the complete scheduled Wave 8 six-day outcome window, including Friday, May 15, and Saturday, May 16.

Across the three specifications, Δ_E is 18.90–19.60 percentage points in Waves 1–7 and 14.54–15.04 percentage points in Waves 1–8 (SE 5.55–5.58; p-values 0.007–0.010). Adding Wave 8 therefore reduces the point estimate by roughly 4.6–4.8 percentage points. The Waves 1–8 estimate remains positive, and its confidence interval excludes zero in every specification. The two component leader effects retain their signs. The traditional-leader effect in homogeneous groups under disclosure, T_{H1}, is 7.63–7.89 percentage points (p-values 0.031–0.041). The religious-leader effect in mixed groups without disclosure, R_{M0}, is 6.80–7.01 percentage points (p-values 0.060–0.073).

C Randomization and Team-Construction Protocol

This appendix describes how residents were assigned to accountability teams. The implementation was conducted in waves, with four study neighborhoods entering the team experiment in each wave. Within each neighborhood, the procedure formed twelve four-person teams. These twelve teams filled the complete $3 \times 2 \times 2$ factorial design: three leader conditions (traditional leader, religious leader, or no leader), two composition conditions (homogeneous or mixed), and two information conditions (individual disclosure or no individual disclosure). The procedure can be summarized in six steps: construct the eligible roster; sample residents within sex-by-wealth strata; form homogeneous and mixed teams; select one assignment using the prespecified rerandomization rule; attach the information and scheduling assignments; and replace unavailable residents according to pre-generated queues. All random choices were generated by code and stored with the wave-specific assignment files.

C.1 Stage 1: Eligible rosters and fixed participants

The assignment was run separately by wave using the recruitment roster that had been completed before that wave's cutoff date. The roster was restricted to consenting residents with the records required for field implementation. Individuals identified as other religious or traditional leaders were excluded from the ordinary-resident pool, preventing the nominal no-leader condition from inadvertently containing another recruited leader.

Each neighborhood had one traditional leader and one religious leader identified before team assignment. These individuals were held fixed and inserted into the teams assigned to their respective leader condition. Beginning in Wave 3, the program also randomly selected one ordinary resident from the traditional leader's territorial origin to occupy the fourth seat in the no-leader teams. We refer to this person as the fixed placebo participant. The placebo participant had no leadership role; using the same person across the no-leader environments made team size and the number of repeated participants symmetric across leader conditions.

C.2 Stage 2: Sampling ordinary residents

Within each neighborhood, ordinary residents were divided into four sampling strata defined by sex and predicted housing wealth: female–lower wealth, female–higher wealth, male–lower wealth, and male–higher wealth. Higher and lower wealth were defined relative to the sex-specific median within the neighborhood; residents with missing property values entered the lower-wealth stratum.

The program constructed separate candidate pools for homogeneous and mixed teams. The homogeneous pool prioritized residents whose administrative territory of origin matched that of the traditional leader. The mixed pool was drawn from the remaining eligible residents and distributed across territories. The program sampled up to 24 residents for each composition pool. Eighteen from each pool were used in the twelve accountability teams; the remaining sampled residents were not assigned to these teams.

The initial wave targeted equal representation of the four sex-by-wealth strata. In subsequent waves, the quotas were adjusted using the cumulative distribution of assignments in completed waves: an overrepresented stratum received fewer positions in the next wave and an underrepresented stratum received more. The adjusted totals were then distributed across the four neighborhoods entering that wave, with each neighborhood receiving the same total number of positions.

When a requested sex-by-wealth cell did not contain enough eligible residents, the program used a fixed priority order: first the opposite sex within the same wealth category, then the same sex in the other wealth category, and finally any remaining eligible resident in the relevant composition pool. These fallbacks preserved team size while keeping the assignment as close as possible to the target margins.

C.3 Stage 3: Forming homogeneous and mixed teams

The twelve group identifiers encoded the factorial cells shown in Table C-50. The order in which group identifiers were filled was randomized. Each group received three ordinary residents. The appropriate traditional leader, religious leader, or fixed placebo participant was then added as the fourth member.

Homogeneous teams. The program filled groups 1–6 from the pool associated with the traditional leader's territorial origin. It assigned residents to teams in randomized group order and one position at a time, which prevented any group identifier from systematically receiving the first available residents. When

Table C-50: Mapping from group identifiers to experimental cells

Group identifiers	Composition	Fourth team member	Individual disclosure
1, 2	Homogeneous	Fixed placebo participant	No, Yes
3, 4	Homogeneous	Traditional leader	No, Yes
5, 6	Homogeneous	Religious leader	No, Yes
7, 8	Mixed	Fixed placebo participant	No, Yes
9, 10	Mixed	Traditional leader	No, Yes
11, 12	Mixed	Religious leader	No, Yes

Notes: Within each row, the odd-numbered group received no individual disclosure and the even-numbered group received individual disclosure. In Waves 1–2, no fixed placebo participant was used; the no-leader teams instead contained four independently sampled ordinary residents.

a territorial-origin or sampling-stratum constraint was binding, the program used the prespecified fallback order described above to preserve team size.

Mixed teams. The program filled groups 7–12 in randomized group order and one position at a time. For every new position, it first selected from territories not already represented in that team. If no such resident remained, it selected from the remaining eligible mixed-team pool. This round-robin rule maximized territorial diversity subject to the available roster while preserving the sex-by-wealth targets.

Repeated participants. The traditional leader appeared in four teams in the same neighborhood: two homogeneous and two mixed, split evenly across information conditions. The religious leader and, from Wave 3 onward, the fixed placebo participant were used in the same way. Leaders participated as ordinary team members and received no script or formal power. The estimation sample excludes the traditional leader, religious leader, and fixed placebo participant.

C.4 Stage 4: Cumulative rerandomization

Respondent sampling and team formation contained random choices. The program therefore generated multiple complete candidate assignments and retained the candidate selected by the prespecified rule documented in Appendix A.2. We considered 48 candidate assignments per wave and evaluated each candidate jointly with the assignments already implemented in earlier waves[20]. No candidate was selected by visual inspection or by reference to treatment effects. The selected seed, assignment roster, and ordered replacement queues were archived.

C.5 Stage 5: Information and time slots

The two group identifiers within every leader-by-composition cell were assigned opposite information conditions. Odd-numbered groups received the status-quo information regime, in which individual contributions were not disclosed; even-numbered groups received individual disclosure. Random allocation across group identifiers produced one team in each information condition for every leader-by-composition cell in every neighborhood.

Finally, the twelve group identifiers were mapped without replacement to the twelve available weekday-by-daypart meeting slots. A group identifier had the same slot across the four neighborhoods in a given wave. In the second half of fieldwork, the slot ordering mirrored the first-half ordering, reversing both day and daypart so that leader and information cells were not systematically concentrated in earlier or later slots.

C.6 Stage 6: Unavailable residents and replacements

Before field implementation, all eligible residents not selected for the initial roster received a random replacement order within neighborhood and territorial-origin category. If a selected ordinary resident was unavailable,

[20] We followed Banerjee et al. (2020), which advocates for the minimum between 100 and the number of randomized units (4 neighborhoods × 12 groups per neighborhood = 48 treatment assignments). Wave 1 used 100 candidates and selected seed 58.

the program assigned the first available person in the corresponding queue, prioritizing the same neighborhood and territorial-origin category before relaxing the match within neighborhood. For mixed teams, the fallback continued to prioritize residents outside the traditional leader's territorial origin. Replacement residents inherited the unavailable resident's group, experimental condition, and meeting slot. Leader replacements were handled separately and preserved leader type.

C.7 Implementation refinements across waves

The assignment logic was stable across the main sample, with two implementation refinements:

1. In Waves 1–2, no-leader teams contained four independently sampled ordinary residents. Beginning in Wave 3, one fixed placebo participant occupied the fourth seat in all four no-leader teams in a neighborhood, leaving three non-repeated residents in every experimental team.
2. Beginning in Wave 3, the sampling quotas explicitly offset cumulative sex-by-wealth differences across group identifiers from completed waves. Candidate assignments had already been evaluated cumulatively beginning in Wave 2.

These changes standardized the fourth team position and improved cumulative balance. They did not alter the mapping between group identifiers and the leader, composition, or information treatments.

C.8 Pseudocode

Input: eligible recruitment roster for four neighborhoods in wave w; fixed traditional and religious leaders; pretreatment covariates; assignments from waves $1, \ldots, w-1$.

For each neighborhood:
  identify the traditional leader's territorial origin;
  select the fixed placebo participant when $w \geq 3$;
  divide ordinary residents into sex $\times$ wealth strata;
  create candidate pools for homogeneous and mixed teams;
  set stratum quotas, offsetting cumulative imbalances from earlier waves.

For candidate seed $s = 1, \ldots, S_w$**:**
  sample residents within composition pools and strata;
  generate complementary three-person stratum patterns across neighborhoods;
  randomly order homogeneous group identifiers 1-6;
  fill each homogeneous team slot-by-slot, prioritizing the leader's origin;
  randomly order mixed group identifiers 7-12;
  fill each mixed team slot-by-slot, prioritizing a new territory;
  add the traditional leader to groups 3, 4, 9, and 10;
  add the religious leader to groups 5, 6, 11, and 12;
  add the placebo participant to groups 1, 2, 7, and 8 when $w \geq 3$;
  append the candidate to completed-wave assignments;
  score candidate s using the prespecified rule in Appendix A.2.

Select the seed with the lowest prespecified assignment score.
Assign no disclosure to odd group identifiers and individual disclosure to even group identifiers.
Attach the wave's without-replacement time-slot permutation.
Export the assignment roster, seed, assignment diagnostics, and ordered replacement queues.

$S_1 = 100$ **and** $S_w = 48$ **for** $w = 2, \ldots, 7$.

D Spillover Exposure and Radius Robustness

Because accountability groups are spatially intermingled within neighborhoods, a participant's behavior could respond to treatment received by nearby households assigned to other groups. The randomized contrasts could then partly reflect the surrounding treatment environment. We address this concern by measuring cross-group proximity at the end of the rollout, adding that measure to the main specification, and checking alternative radii.

The analysis holds fixed the main sample of 1,040 participants in 336 assigned groups across 28 neighborhoods, the outcome, the pretreatment participation control, the design fixed effects, and group-level clustering. Exposure is a realized geographic measure, so its coefficient is an associational diagnostic rather than a randomized indirect effect.

Terminal exposure. Let d_{ij} denote the distance between respondents i and j and G_i respondent i's assigned accountability group. For radius r, terminal exposure on the wave's final treatment date is

$$E_i(r) = \mathbf{1}\left\{\sum_{j \neq i} \mathbf{1}\{d_{ij} \leq r\}\mathbf{1}\{G_j \neq G_i\} > 0\right\}. \tag{4}$$

The sum runs over study households in respondent i's wave, all of whose groups have been treated by that date. Thus, a respondent is exposed when at least one household from another accountability group lies within the chosen radius. Nearby members of the respondent's own group are excluded because their treatment is part of the respondent's assigned intervention rather than a spillover.

Figure D-8 illustrates the 25-meter definition in neighborhoods near the 20th, 50th, and 80th percentiles of terminal exposure. Some households appear inside a gold buffer but remain classified as unexposed because the nearby treated household belongs to the same assigned group.

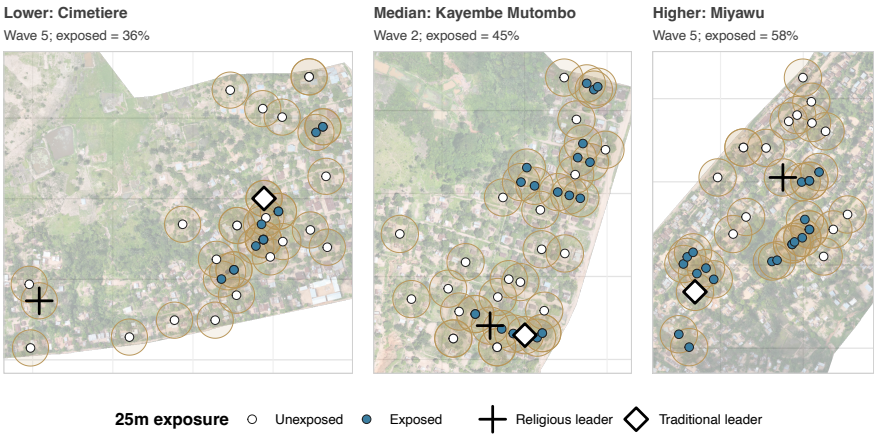


Figure D-8: TERMINAL SPILLOVER EXPOSURE IN THREE NEIGHBORHOODS

Notes: The panels show the mechanically selected neighborhoods nearest the 20th, 50th, and 80th percentiles of neighborhood-level 25-meter exposure. All panels are measured on the final treatment date. Gold disks are 25-meter buffers around treated study households. Filled points are main-sample respondents with at least one treated out-of-group household within 25 meters; white points are unexposed. Crosses mark religious leaders and diamonds traditional leaders. The registry TIFF is shown translucently. A respondent inside a buffer remains unexposed when the nearby household belongs to the respondent's own assigned group.

Radius choice and additive adjustment. Panel A of Figure D-9 shows the identifying-support tradeoff. At 25 meters, 483 of 1,040 participants are exposed, or 46.4 percent of the sample; at 30 meters the share is 55.4 percent. The 25-meter cutoff is therefore the stored grid point closest to an even split, selected without using outcomes. Exposure rises to 79.4 percent at 50 meters and 89.4 percent at 70 meters, leaving progressively fewer unexposed respondents.

For each radius, Panel B adds $E_i(r)$ to the main specification while holding the sample and remaining specification fixed. The randomized contrasts change little: across 10–70 meters, Δ_C ranges from 9.27 to

9.75 percentage points and Δ_E from 18.99 to 20.01 percentage points. At 25 meters, the exposure coefficient is 1.65 percentage points (SE 2.09) in the composition model and 1.81 (SE 2.09) in the environment model. The exposure measure is therefore not strongly associated with participation, and controlling for it does not account for the paper's main treatment contrasts.

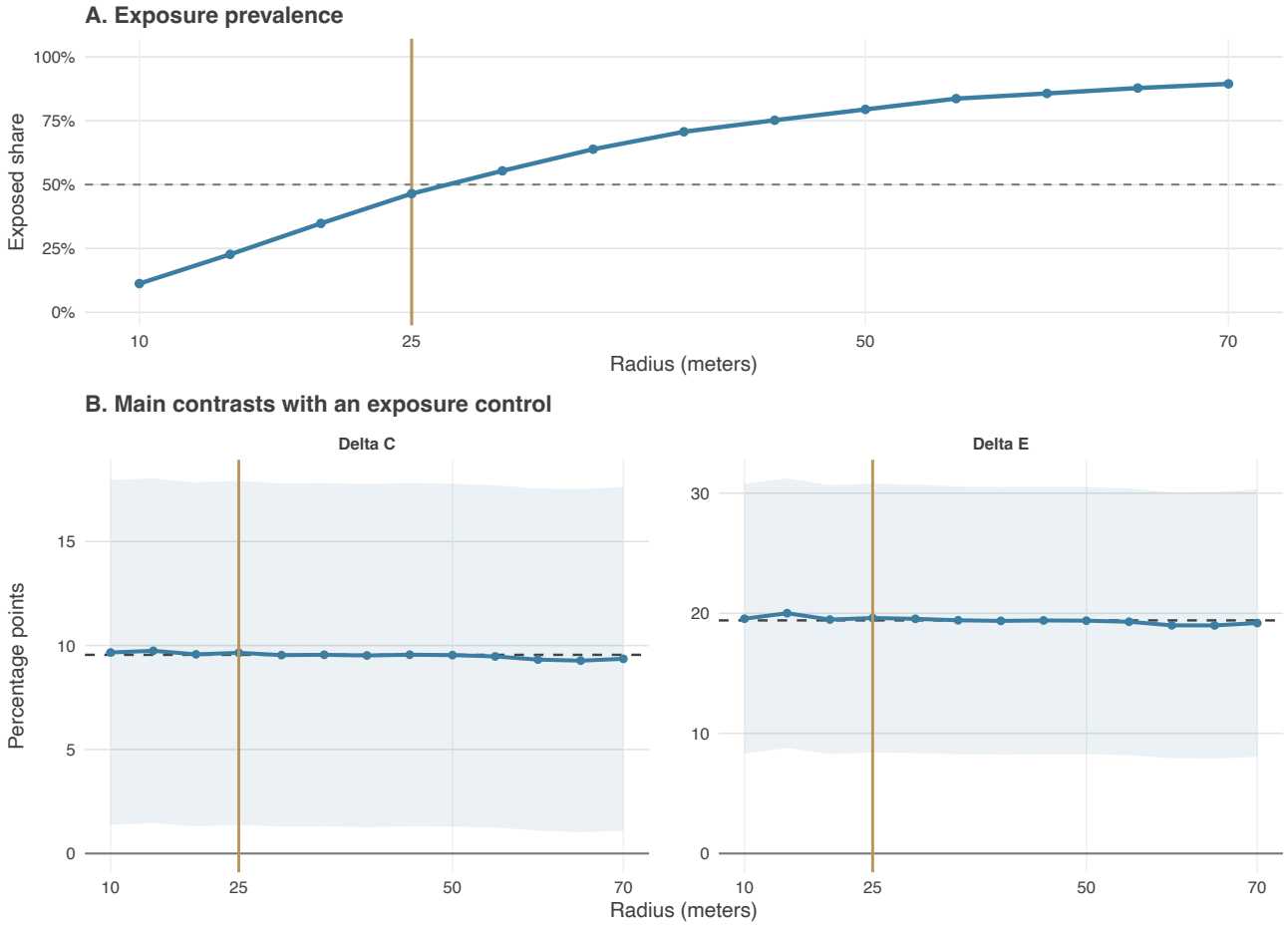


Figure D-9: EXPOSURE SUPPORT AND MAIN CONTRASTS ACROSS RADII

Notes: Panel A reports the terminal exposed share in the fixed Waves 1–7 main-regression sample. The horizontal dashed line marks 50 percent. Panel B reports Δ_C and Δ_E after adding the radius-specific exposure indicator to the raw fixed-effects specification; shaded regions are 95 percent confidence intervals based on standard errors clustered by assigned group within neighborhood. The dashed horizontal line in Panel B is the corresponding estimate without an exposure control. The gold vertical line marks 25 meters, and every contrast panel includes zero.