Getting to Youths: Development Programming, Conflict Resolution, and Political Violence in Niger [*]

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Abstract

How can practitioners reduce participation in violence where neither the state nor conflict resolution institutions are accessible? This paper presents a cluster-randomized control trial in the Maradi and Tillabéri regions of Niger. Within a vocational training and civic engagement program, young people in randomly assigned villages trained in Interest-Based Negotiation and Mediation (IBNM), an approach to conflict management. In a follow-up survey of 1,734 youth across 118 villages, we show that youth who participated in both Youth Connect and IBNM training are less likely to support violence than youth who only participated in Youth Connect a year after IBNM trainings took place. We find no difference between youth who participated in Youth Connect by itself and a pure control group. A difference-in-differences approach using geo-referenced conflict incidents corroborates these results: villages which received both Youth Connect and IBNM experienced fewer violent incidents than villages which only received Youth Connect.

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When do youth participate in political violence or join violent groups? A plethora of recent interventions by governments, NGOs, and donor organizations have attempted to reduce participation in political violence among youth (Bhatt et al. 2024; Blattman and Annan 2016; Pruett et al. 2024). The results of such interventions are mixed, as are findings within the broader literature on violence prevention (Dasgupta, Gawande, and Kapur 2017; Premand and Rohner 2024; Sexton and Zürcher 2024). In contexts like Niger where neither the state nor institutions of conflict resolution are accessible, addressing individual-levels drivers of violence may not suffice to reduce levels of violence. How can policymakers and researchers shift the attitudes and behaviors of young people while addressing factors which drive their participation in—and vulnerability to—violence?

This study uses a randomized controlled trial (RCT) in the Maradi and Tillabéri regions of Niger to provide causal evidence that training youth in conflict management skills reduces support for violent extremism above and beyond a standard intervention package of youth-focused economic opportunities and civic engagement activities. Our research takes place within a broader USAID-funded program, titled Youth Connect, which uses a mixture of vocational training, economic start-up kits, and civic engagement training to help youth enhance their livelihoods. Youth Connected aimed to reduce support for violence and vulnerability to recruitment by violent extremist organizations (VEOs). Within the 84 villages which received Youth Connect, we randomize village participation in Interest Based Mediation and Negotiation (IBNM) training. IBNM is a conflict management strategy which follows a training of trainers models, wherein selected youth leaders participated in trainings in regional capitals and disseminated their experiences to other young people in their villages. To capture outcomes, we surveyed 1,734 youth in 41 villages which received Youth Connect, 41 villages which received Youth Connect alongside the IBNM training, and an additional 36 villages which serve as a pure control.

We study violence in the context of Niger, a country in the West African Sahel with high rates of poverty and low state capacity.[1] Violent extremism in the Sahel has led to increasing fragility and alarming humanitarian consequences in both Niger and its neighbors, making it a difficult context in which to move the needle on support for political violence. For example, a randomized cash transfer program in Niger actually increased the number of violent incidents experienced by villages (Premand and Rohner 2024). Similarly, pro-peace religious messaging led to a backlash towards non-coethnics in neighboring Burkina Faso (Grossman, Nomikos, and Siddiqui 2023). Furthermore, the Youth Connect program specifically targeted villages within

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[1]Nigers' GDP per capita in 2023 was 618 USD, making it the 9th poorest country in the world, according to the World Bank.

Maradi and Tillabéri where youth vulnerability to political extremism was perceived to be highest. As such, Niger is a "most difficult" case in which to test peacebuilding strategies.

We find a substantial reduction in support for violence among youth in villages which were selected to receive IBNM training. Respondents in Youth Connect villages that also received IBNM trainings were less likely to report that people in their village perceived retaliatory violence or violence to protect their religion to be justified compared to youth in villages which received Youth Connect only. We support these results using a series of list experiments. Youth in villages which received both IBNM training and Youth connect are less likely to agree that "it is justifiable to use violence for a political or religious cause" and slightly less likely to consider responding to a range of scenarios with violence. In addition, we use a difference-in-differences framework using violent incidents as measured by the Armed Conflict Location and Event Data (ACLED) project to show that villages selected for IBNM trainings experienced fewer violent incidents in which jihadist groups were the primary actor than villages which only received Youth Connect. These results suggest that IBNM training succeeded in helping villages to resolve conflicts before armed groups could interject themselves into local disputes. However, across these different measurement strategies, we find minimal differences between pure control villages and villages which received only Youth Connect.

A growing literature explores the effects of complimentary interventions to reduce support for and participation in political violence. Such interventions combine economic incentives with components such as cognitive behavioral therapy (Bhatt et al. 2024; Blattman, Jamison, and Sheridan 2017), mobilizing villages to overcome collective action problems (Fearon, Humphreys, and Weinstein 2015), local consultations (Sexton and Zürcher 2024, and institution-building (Hartman, R. A. Blair, and Blattman 2021). We add to this conversation by evaluating IBNM, a light-touch and low-cost training in conflict management based on Roger Fisher and William Ury's *Getting to Yes*, a mainstay of MBA syllabi (1981). IBNM has previously been used as a stand-alone peacebuilding intervention (Reardon, Wolfe, and Ogbudo 2021), but we test whether IBNM has a multiplier effect when layered atop a conventional package of vocational training, economic transfers, and civic engagement activities. While multi-pronged interventions have become more common within peacebuilding contexts, our research sheds suggests that programs which target individual-level drivers of violence may not suffice to reduce violence in contexts such as Niger, where mechanisms for conflict resolution are inaccessible.

Conventional approaches to building peace often function through existing conflict resolution institutions (Autesserre 2010). These approaches are ill-suited to contexts such as Niger, where low-state capacity and conflict have eroded the state. Local governance in rural Niger

operates mostly through customary institutions, often village councils and chiefs, who can co-opt programs to provide economic assistance for dis-empowered communities such as youth (Gibson and Woolcock 2008). In such areas, local disputes can metastasize into violent incidents (Blattman, Hartman, and R. A. Blair 2014; Kalyvas 2003). IBNM is a bottom-up peacebuilding intervention: in response to this grass-roots production of violence, IBNM provides youth with the toolkit to resolve both their own conflicts and other conflicts within their community.

The paper proceeds in eight parts. The following section introduces overviews ongoing challenges within Niger. The second section explores pre-existing strategies to reduce violence while showing how peacebuilding in Niger must also address the bottom-up production of violence by both armed groups and individuals. Section three introduces both IBNM and the broader Youth Connect program. The fourth section enumerates our experimental design, measurement strategies, and estimation strategies. Section five overviews our main results and section six outlines heterogeneity within those results. Section seven provides results from a difference-in-differences analysis which shows how villages in which IBNM trainings took place experience fewer violence incidents. Section eight concludes the paper.

1   Setting: Niger

Figure 1 shows the Maradi and Tillabéri regions of Niger alongside the communes in which Youth Connect took place or will take place. Tillabéri borders both northern Mali and eastern Burkina Faso, where the 2012 outbreak of conflict in Northern Mali precipitated cross-border instability. Maradi abuts northern Nigeria, and suffers from similar patterns of instability, including occasional incursions by Boko Haram. In 2022, the year before the IBNM trainings took place, the Armed Conflict Event Location Database (ACLED) recorded 982 violent incidents across Niger, of which 37 percent took place in Tillabéri and 15 percent took place in Maradi.

Military coups and political upheaval pervade Niger's journey of political liberalization. Most recently, Niger experienced a coup d'etat in 2023, unseating President Bazoum and causing a setback to stabilization efforts in the country. Despite threats of sanctions and military intervention from the Economic Community of West African States (ECOWAS), the coup leaders refused to relinquish power. The political turmoil in the country, alongside climate shocks, ethnic exclusion, and a socioeconomic crisis, raise serious concerns about the proliferation of violence and violent extremism in the country (Lichtenheld and Ogbudu 2021; Raleigh, Nsaibia, and Dowd 2021).

This violence feeds into an ongoing lack of state capacity. Formal institutions of conflict resolution are rare throughout the country, so informal governance predominates. In the 2022

Figure 1. Youth Connect and pure control communes within Niger
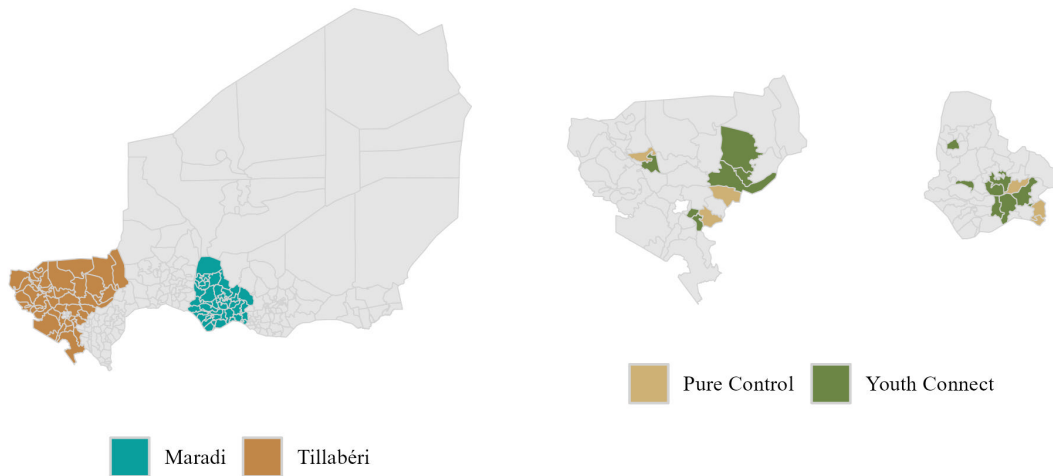
The lefthand panel shows the Maradi and Tillabéri regions within Niger. The righthand panel shows the 14 communes within Maradi and Tillabéri in which Youth Connect took place, as well as the six pure control communes. Assignment to IBNM training was randomized at the village level within the 14 Youth Connect communes.

round of the Afrobarometer survey, 20 percent of respondents in Niger reported having contacted their elected representative in the national assembly in the last 12 months, and 35 percent reported having contacted a local elected leader. In contrast, 46 percent had contacted their traditional chief in the past twelve months. 40 percent of Nigeriens stated that they had no confidence or just a little confidence in the national assembly, and 36 percent had little or no confidence in their municipal or communal councils. In contrast, only 19 percent had little or no confidence in their traditional chiefs.[2]

In Maradi and Tillabéri in particular, 54 percent of youth strongly or somewhat disagreed that their voice was heard by village and administrative authorities when dealing with them.[3] With a formal state riddled by instability, informal and traditional institutions are primary sources of conflict resolution and stability. When asked "If you had a dispute about land/livestock/a business

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[2]These statistics are from wave 9 of Afrobarometer; they use Afrobarometer's survey weights. Across all respondents in wave 9 of Afrobarometer (excluding countries where questions about traditional chiefs were not asked), 26 percent contacted their local elected officials, 23 percent contacted their elected representative, and 25 percent contacted their traditional chief. Similarly, 55 percent of all Afrobarometer respondents had little to no confidence in their national assembly, 57 percent had little to no confidence in their local municipal government, and 40 percent had no confidence in their customary chiefs.

[3]These statistics are from a baseline survey conducted before Youth Connect, administered in the fall of 2022.

transaction, who would you approach to resolve the dispute?" 77 percent of youth said they would approach a traditional leader. Only nine percent said they would approach a government leader. These data illustrate the lack of formal institutions and overall instability which characterizes rural Maradi and Tillabéri.

2   Theoretical framework

Previous research identifies a variety of risk factors for violence. Political disputes, economic factors, and psychosocial causes all drive participation in violence. As a result, scholars have tested a variety of interventions and programs to reduce participation in violence (Bhatt et al. 2024; Blattman, Jamison, and Sheridan 2017; Lyall, Zhou, and Imai 2020). However, the goal of violence prevention programs is not to prevent any one violent act, or any one individual from turning to violence, but to create an environment in which violence is more rare.[4]

However, even when poverty or psychosocial factors do not draw an individual into violence, the absence of strong conflict resolution institutions permits small disputes to escalate into violence. For example, armed groups in Niger such as JNIM and AQIM often intervene in local conflicts between farmers and pastoralist groups in support of Fulani or Tuareg youth, increasing their presence in the community. Similarly, individuals may take advantage of the permissive environment for violence and align with various armed groups to settle pre-existing vendettas or advance their own interests (Kalyvas 2003). The result is a co-production of violence by both armed groups and local populations. We theorize that layering IBNM training—which facilitates conflict resolution at the village level—atop Youth Connect—which addresses individual drivers of violence—will have stronger effects on support for violence than either type of intervention by itself.

2.1   Interventions to reduce violence

Economic motivations have long been the core explanation of support for violent extremism. At the macro level, many of these explanations are predicated upon a view of civil conflict as a competition for resources. Land disputes, for example, drive a large number of recent conflicts on the African continent including those in the eastern Congo and in Côte d'Ivoire (Autesserre 2010; Boone 2003). The presence of transhumant groups like Tuaregs and Fulani within the Sahel

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[4]We focus explicitly on determinants of participation in violence, rather than determinants of violence itself. This approach also speaks to a growing focus within political science on "a broader set of armed actors and their interactions with states and to theoretically focus on ideational variables as a key driver of patterns of order" (Staniland 2023: 197).

means that cattle rustling between farmers and herders and cattle rustling are not uncommon. Specific expenses, such as bride-prices, may also drive youth into the arms of VEOs (Hudson and Matfess 2017). These links are not unique to the developing world.

Within such a framework, joining a violent group is a means to acquiring wealth or resources. Violent groups often pay recruits—which is particular motivation in countries such as Niger where income opportunities are scarce. From the perspective of the violent group, this approach has downsides: militants who join groups for a wage rather than for an ideological conviction are less disciplined and commit more violence against civilians (Weinstein 2007). From the militant's perspective, these wages are often higher than any other employment, which makes participating in political violence appealing from an entirely economic perspective. Where economic privations drive participation in violence, increasing the availability of other employment or income sources should increase the opportunity cost of participating in political violence.

A variety of recent interventions which aim to reduce participation in violence have been predicated on this view of violence as economically motivated. Blattman and Annan (2016), for example, use an employment program among ex-combatants in Liberia to reduce participation in both illegal activity and employment as mercenaries in neighboring conflicts. However, these interventions often have mixed results. In India, the rollout of the National Rural Employment Guarantee Scheme reduced Maoist violence, but only in limited areas with high pre-existing state capacity (Dasgupta, Gawande, and Kapur 2017). In contrast, combining cash transfers and vocational training in Afghanistan increased support for the Afghan government, though neither program was effective by itself and the cash transfers by themselves actually increased Taliban support in the short term (Lyall, Zhou, and Imai 2020). In Niger, a randomized cash transfer program actually increased the count of violent incidents in treated villages in the short term, though another cash transfer program in the Philippines decreased conflict (Crost, Felter, and Johnston 2016; Premand and Rohner 2024).

Risk factors for participation in violence often overlap. Humphreys and Weinstein (2008), for example, point out that multiple risk factors for participation in violence can operate in tandem. To address multiple risk factors, recent research explores multi-armed interventions. For example, Bhatt et al. (2024) combine an 18 month employment training in Chicago with cognitive behavioral therapy; the intervention reduced shootings and homicide arrests among vulnerable youth in Chicago, although not all outcomes saw reductions. In Liberia, reductions in crime and violence ebbed after a year, but when the CBT was combined with USD 200 cash grants, the reduction in violent behaviors lasted (Blattman, Jamison, and Sheridan 2017). Youth Connect advances this literature by combining both economic and civic engagement activities into one

intervention.

Civic engagement interventions can both build the capacity of local conflict resolution institutions and provide alternative pathways to change government policies. In Kenya, participants in a civic engagement training maintained greater confidence in political systems despite subsequent electoral violence (Finkel, Horowitz, and Rojo-Mendoza 2012). Fearon, Humphreys, and Weinstein (2015) show that a community development program increased villages' capacity to cooperate, albeit only in villages who could not rely on traditional institutions to implement the program. In Sudan, a community development program increased both civic engagement and the extent to which local institutions were participatory (Avdeenko and Gilligan 2015). However, there is a paucity of evidence linking such interventions to direct measures of support for violence.

2.2   Co-produced violence

The preponderance of evidence suggests that recent interventions can reduce violence, at least among program participants. However, in particularly insecure contexts such as Niger, such programs are unlikely to have long-term effects. Such areas often have complex but intensely local constellations of power and contestation over limited resources (Bierschenk and De Sardan 1997). In such scenarios, local cleavages, rather than overarching grievances, structure low level violence . Kalyvas (2003: 475–6) points out that "individual and local actors take advantage of the war to settle local or private conflicts often bearing little or no relation to the causes of the war or the goals of the belligerents." In other words, individuals may participate in an ongoing conflict to advance individual goals or vendettas. This dynamic is clear in Niger, where insurgent groups such as JNIM and AQIM recruit largely from Tuareg and Fulani youth who often excluded from village institutions (Raleigh, Nsaibia, and Dowd 2021). In areas of weak state capacity, the lack of conflict resolution institutions may lead these small-scale disputes to simmer, eventually metastasizing into violence when an overarching conflict provides an opening to act on longstanding grievances.

The blurring of political and economic causes contributes to a bottom-up production of violence. In Chad, for example, ongoing but low level violence contributed to a situation where violence became a practical occupation for many young men "periods spent undertaken economic activities performed with or without arms, often in the margins of the state" (Debos 2016: 11). Autesserre (2010: 8) makes a similar point in the context of the eastern Congo: local conflicts "pitted villagers, traditional chiefs, community chiefs, or ethnic leaders against one another over the distribution of land, the exploitation of local mining sites... and the relative social status of

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specific groups and individuals." The absence of economic opportunity combines with a paucity of conflict resolution institutions to permit local disputes to escalate to violence.

While the absence of conflict resolution institutions precipitates violence, building strong institutions of conflict resolution is not a simple task. As such, a variety of recent interventions aim to construct alternative dispute resolution mechanisms, rather than rehabilitate existing institutions. An alternative dispute resolution campaign in Liberia trained participants to negotiate resolutions to their own disputes or to disputes in their communities. After three years, communities which participated in these trainings had lower incidences of violence and promoted violence avoidance norms (Hartman, R. A. Blair, and Blattman 2021). Similarly, Grady et al. (2023) show that community development programs which increased inter-group contract between farmers and herders in Nigeria led to more positive feelings towards out-group members and increased feelings of security. This kind of dispute resolution is especially relevant in Niger, where armed groups often intervene in local disputes to garner support among ethnic groups who feel themselves to be marginalized (Raleigh, Nsaibia, and Dowd 2021).

This evidence illustrates the complexities inherent in fostering peace in areas such as Niger. Economic factors are at play: Niger is one of the poorest countries in the world, and economic opportunities are particularly lacking in rural areas such as Tillabéri and Maradi. At the same time, conflict resolution institutions are also absent, which can lead small-scale disputes to escalate into violence. Interventions which are inattentive to one or more of these drivers of violence. In the remainder of this paper, we explore how layering a conflict resolution training atop a conventional vocational education and civic engagement program can address these multiple factors simultaneously.

3 Interventions: Youth Connect and IBNM Training

We study a multi-pronged intervention, titled Youth Connect, which addresses economic, social, and political drivers of political violence simultaneously.[5] Youth Connect, which was funded by USAID, had three objectives: (1) providing youth with market-relevant skills to help them improve their livelihoods; (2) providing youth and youth groups with better access to resources to facilitate their employment; and (3) training youth in civic engagement skills to help them actively engage with local governance structures and voice their needs. The program targeted both men and women between the ages of 15 and 34.

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[5]Mercy Corps led a consortium of six other NGOs to implement this program: SOS Sahel, Femmes, Action et Développement (FAD), IDEO.org, GeoAnalytics Center, Viamo, and SwissContact.

The program advanced these objectives through a series of specific interventions in 84 program villages. The first objective was to provide vocational training. Youth in program villages had access to training in both agriculture and non-agriculture livelihoods. The agriculture trainings focused on market-demanded skills and links to agricultural value chains.[6] Non-agricultural vocational training was largely delivered via apprenticeship programs. Not all villages received the same training programs; the selection of these vocational skills was tailored to ensure it targeted livelihoods relevant to youth in the village. Women were specifically recruited to participate in these programs to ensure gender balance.

The second objective was to improve youths' access to resources needed to pursue the livelihoods in which they were trained.[7] To this end, youth received a series of 'start-up' kits. For example, some youth received bottles of gas (for cooking) or animals. The third objective was to train youth in civic engagement skills. Mercy Corps delivered training on a variety of soft skills for meaningful civic engagement, including advocacy, principles of good governance, and community mobilization. Local youth groups delivered these trainings within the targeted villages. Beyond its economic activities, the civic engagement elements of Youth Connect aimed to help youth participate in existing institutions for conflict resolution. Considering how economic factors can drive individuals to participate in or support violence, we hypothesize that:

H.1 Respondents in villages which participated in Youth Connect will express less support for violence than respondents in pure control villages.

However, in areas of extremely weak state capacity, such as Maradi and Tillabéri, even enhanced participation in existing institutions may be insufficient to alleviate bottom-up violence. To that end, we implemented an RCT within the broader Youth Connect program to test the effect of training in Interest-Based Negotiation and Mediation (IBNM) as an add-on to Youth Connect's more conventional violence prevention strategy. IBNM is an approach to resolving negotiation adapted from *Getting to Yes*, a how-to guide for negotiation often taught in American business schools (Fisher and Ury 1981). IBNM focuses on creating 'win-win' scenarios by identifying the multiple goals participants bring to negotiations. It aims to build productive agreements by clarifying acceptable outcomes to participants—including walking away from a negotiation

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[6]These trainings were based on a methodology developed by Youth Connect consortium member SwissContact. Trainings took two forms. Short-term, flexible *Formations Initiales Professionnalisantes* trained youth on a variety of specific skills, such as poultry-raising and livestock fattening. Through *Sites Intégrés de Formations Agricoles*, youth practiced agricultural skills on family land under supervision.

[7]In appendix B we show that Youth Connect had positive effects on wealth, employment, and other measures of economic welfare.

if need be. Mercy Corps has previously deployed IBNM training in Afghanistan, Colombia, Ethiopia, Guatemala, Jordan, Kenya, Mali, Myanmar, Nigeria, Iraq, and Tajikistan. In northern Nigeria, training community leaders in IBNM reduced inter-group violence (Reardon, Wolfe, and Ogbudo 2021). Elsewhere, Christensen et al. (2024) show that training village elites in IBN (without the mediation component) decreased exploitation of nearby forest in treated villages. Our research is the first to experimentally evaluate IBNM within the context of a broader intervention to prevent participation in violence and show how IBNM complements more conventional efforts to reduce support for violence. Unlike previous projects, we also explicitly center youth in the intervention, to provide them with non-violent strategies to resolve conflicts and act as a mediator in the their community.

This intervention followed a training of trainers model, in which Mercy Corps' field agents selected four "youth leaders" per village to receive the full IBNM training in consultation with communities and community leaders. Gender was an explicit component of this selection process: two men and two women per village were selected as youth leaders. Of the 183 youth leaders that participated in this initial training, 100 were women and 83 were men. These youth were invited to attend trainings in IBNM techniques which took place in Maradi and Tillabéri, the region-level capitals. The trained youth leaders then led training in IBNM techniques within their home villages.

IBNM training took the form of a three day seminar which centered the "seven elements of negotiation:"interests, alternatives, relationships, options, legitimacy, commitments, and communication (Fisher and Ury 1981). These elements originate in *Getting to Yes*, but were translated to match local interests. For example, participants in the training broke down a hypothetical position of "give me this piece of land" as representing several potential interests: cultivating land, building a health center, to graze livestock, to build a school, or to make a soccer pitch. Participants then explored strategies to advance mutual interest and resolve the underlying conflicts. Specifically, it trained youth to search "to understand the objectives and worries of the two parties and use empathy to demonstrate understanding of their thoughts and worries... to help the conflicted parties to understand each other and respect each others' positions." The training also provided images of and guides to the seven elements of negotiation and mediation to facilitate uptake.

The conflicts resolved by youth leaders take a variety of forms. In one conflict, an IBNM-trained youth leader resolve a conflict caused by a herder's animals destroying a rice paddy. The youth leader reminded both parties that "the little conflict which grows creates insecurity in communities, so it is in everybody's interest to resolve the conflict." The youth leader listened to both

sides separately, and negotiated an acceptable resolution without outside support. In another incident, a youth leader intervened when a phone repair specialist charged a young woman a large amount of money to repair her phone; the young woman was unable to pay so the repair specialist attempted to sell the phone to repay the cost of repair. After negotiating from 10:00am to 6:00pm, "all parties apologized for the excessive remarks and left the scene peacefully."

To measure the immediate effect of this IBNM training on participants, we administered a short questionnaire before and after they participated in the training. Compared to before the IBNM training, participants were 0.78 standard deviations more likely to agree that they "can influence decisions made in community meetings" and 0.8 standard deviations more likely agree that "I have the capacity and the approach to make my community take my point of view into account." They were 0.96 standard deviations more likely to agree that "I'm confident in my ability to resolve conflicts between neighboring villages" and 0.88 standard deviations more likely to agree that "I am confident in my ability to resolve my own conflicts. These results suggest that the youth leaders absorbed the IBNM training.[8]

This intervention aligns well with the co-production of violence we hypothesized earlier. Violent incidents can arise from small-scale dispute. Households may take advantage of the overarching cleavage between the armed group and the state to advance their personal vendettas (Kalyvas 2003). Armed groups may also interject themselves into dispute resolution. Because IBNM training aims to resolve disputes within villages, before armed groups can become involve, we hypothesize that:

H.2 Respondents in villages that received Youth Connect and IBNM training will express less support for violence than respondents in villages which received Youth Connect only.

4 Methodology and Estimation Strategy

4.1 Experimental design

Mercy Corps used a village selection tool to target the program intervention to villages where it had the greatest potential to impact violence.[9] Program staff traveled to communes and met with local stakeholders to arrange meetings, assess the needs of villages, and select villages for Youth Connect. Five villages per commune were selected to receive Youth Connect, in addition to the

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[8]Appendix D explores these data in more detail.

[9]Youth Connect was also implemented in Burkina Faso; IBNM trainings took place only in Niger.

commune centers (*chefs lieux*). The sampling frame for this research is the set of all 84 villages and commune centers which participated in the Youth Connect program.[10]

We randomized assignment to IBNM training within stratas defined by village type and the presence of ACLED events two year period before randomization took place. We stratify on village type because our sampling frame includes both rural villages and commune centers, the latter of which tend to be larger quasi-urban agglomerations. Within Maradi, we stratify on ACLED events to also capture pre-intervention exposure to violence. All but three villages in Tillabéri had at least one ACLED incidents, so we do not stratify on ACLED in Tillabéri.[11] Our sample is not representative of the overall Nigerien population: Youth Connect specifically targeted villages which were particularly vulnerable to violence and recruitment by violent extremist organizations. Our survey weights are representative of this sub-population. As a result, we conceptualize our estimand as a local average treatment effect (LATE) among particularly vulnerable youth (Imbens 2010).

This distinction is particularly relevant for the external validity of this experiment—the participants in Youth Connect and IBNM are are particularly vulnerable to political violence and recruitment by violent organizations. However, violence reduction programs generally center those who are most likely to support violence.[12] These youth are not representative of the super-population of Nigerien youth, but they are nevertheless the key population of interest.

4.2 Measurement strategy and hypotheses

We primarily measure outcomes through a survey of 1,734 youth across 118 villages in Maradi and Tillabéri: 41 villages that received Youth Connect only, 41 villages which received Youth Connect and the IBNM treatment, and 36 pure control villages which received neither Youth Connect nor IBNM treatment. Because of ongoing violence and guidance from local government officials, we did not conduct the survey in one village that received Youth Connect only and one village that received both Youth Connect and IBNM training.

Within these 118 villages, we surveyed two groups of youth. We surveyed ten youth per village selected through a random walk within villages. Once enumerators arrive at households, they asked the household head how many individuals between the ages of 15 and 34 live in the household. The tablet-based survey instrument randomly selected one youth to whom the enu-

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[10] For more details on the village selection process, see: Ribar, Sheely, and Lichtenheld (2023).

[11] The 42 villages which did not receive IBNM training before the midline survey will receive it after. The six pure control communes will receive both IBNM and Youth Connect after the midline survey.

[12] For example, see Blattman, Jamison, and Sheridan (2017: 1166) or Bhatt et al. (2024)

merator administered the survey. Enumerators administered an informed consent procedure at each stage: both household heads and individual youth consented to participate in this survey.[13] This process selected 1,232 young people.

This random walk does not guarantee that sampled youth would have directly participated in Youth Connect. As such, we also directly sampled 532 youth who participated in Youth Connect's vocational training and civic engagement activities. These youth were randomly sampled through Mercy Corps' record of participation in training. Sampling for these 532 youth was independent of IBNM treatment, but 257 came from villages which received Youth Connect only and 245 came from villages which received both Youth Connect and IBNM training. The survey instruments were identical for the two sample groups.

We use multiple strategies to measure support for political violence. Our first priority was to ensure enumerators and respondents felt safe and comfortable answering these questions. We ensured that questions could not directly identify any individual's level of support for violence. Our secondary concern was to minimize social desirability bias (G. Blair, Coppock, and Moor 2020). Where respondents perceive that they may be punished or judged for a response, they are less likely to respond in that way, which biases estimates away from this response. To ensure enumerator safety and minimize social desirability bias we use indirect survey measures to capture support for violence.

Our first measurement strategy is to ask respondents "[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following: defend one's ethnic group, force the government to change their policies, defend one's religion, and retaliate against violence." Answer choices included never justified, rarely justified, and sometimes justified. In addition to the individual responses, we also calculate indices by adding responses to these questions together and by taking the first principal component of these answers.

Our next measurement strategy leverages a series of list experiments. List experiments have become increasingly popular in political science and economics to identify average support for opinions which may suffer from social desirability bias (G. Blair and Imai 2012). Respondents are asked a question and presented with a list of potential responses. The enumerator asks respondents to identify *how many* items they agree with or support but not *which* items. By randomly presenting some respondents with an additional item in the response list, researchers can back out an average level of support for the sensitive item specifically. This strategy mitigates social desirability bias because respondents never admit support for the sensitive item specifically. For

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[13]Consent scripts were available in French, Haoussa, and Zarma.

this survey, enumerators asked respondents:

> I am going to read you a list of statements. I'd like you to tell me how many of these statements you agree with. Please don't tell me which items you agree with, just how many of them you agree with. Now, I will read the statements:

Options included "Western powers should continue to intervene to keep the peace in Niger; All children should go to school; Other people in Niger are not at all trustworthy; and In line with our customs, we should respect our elders." A random 50 percent of respondents also received "It is justifiable to use violence for a political or religious cause."

This list experiment is important to back out attitudes to political violence. However, Westwood et al. (2022) point out that responses to hypothetical questions about political violence change when respondents are presented with more concrete scenarios. We explore this difference through a series of list experiments which present specific scenarios to the survey respondents. More specifically, we asked respondents to

> [i]magine the following scenario... [n]ow I am going to read you a list of ways that some people might respond to this scenario. I do not need to know which of these ways you might consider responding to the scenario, but please tell me how many of them you would consider.

The specific scenarios that respondents considered were:

1. Another person in your village has started encroaching on your land. In previous years, both of you had farmed neighboring parcels. This year, he has started farming your parcel as well as his.

2. A herder from a different ethnic group as you has driven their animals through your land. In the process, many of your crops were destroyed or damaged.

3. An elder has prevented all of the youth from attending a meeting in which the village will discuss the location of a new well or borehole.

4. Somebody from the village has been giving bad advise to the village chief. You think that this advice well make the chief take decisions contrary to the interests of the village youth. For example, this advisor claims another ethnic group is responsible for violence, and so your village should chase them away.

5. Somebody from the village has given bad advice to the chief which will lead him to take decisions which will make it more difficult for your ethnic group to access services.

We designed these scenarios as factorial experiments with multiple treatment arms. Respondents were randomly assigned across three treatment arms: (1) whether they received treatment, in the form of an additional sensitive item; (2) which of two control lists respondents received; and (3) which of two treatments (if any) they received. One list of sensitive control items included: "ask another youth to intervene, complain to family, go to the gendarmerie, contact an NGO, ignore the problem entirely, and chase the other person out of the village." The other list of control items included: "complain to friends, get help from an imam, summon the sous-prefect, wait for the problem to resolve itself, and leave the village." The two treatment items were: (1) "threaten the other person with a weapon" and (2) "chase the other person out of the village."

4.3   Estimation Strategies

All regressions use OLS and within-sample inverse-probability survey weights. We include region and enumerator fixed effects. Enumerator fixed effects control for error induced by different enumerators (Adida et al. 2016). The size of enumerator fixed effects is uncorrelated with assignment into the IBNM training and so comparisons between villages which received Youth Connect only and those that received Youth Connect plus IBNM are unbiased and consistent. However, a different pool of enumerators surveyed pure control villages, which poses potential problems for comparisons between Youth Connect and the pure control villages. Enumerator effects should compensate for these differences, but this assumption cannot be verified empirically.[14] We nevertheless report all comparisons for completeness. We also control for whether the respondent was recruited via the random walk or via the set of direct beneficiaries.

For the first set of survey outcomes (the 'justified' questions), we estimate equations of the form

$$y_i = \beta_1 \text{Pure control} + \beta_2 \text{IBNM} + \psi_1 X_i + \psi_2 X_v + \gamma_r + \epsilon_i$$

Where y is the outcome of interest, X_i is a vector of demographic controls, X_v is a vector of village level controls, γ_r are region-level fixed effects, and ϵ is an error term. Respondents are indexed by i, villages by v, and regions by r. All equations use OLS; robust standard errors are clustered at the village level, because treatment is assigned at the village level (Abadie et al. 2023). Our coefficients of interest here are β_1 and β_2. The baseline level is villages which received Youth Connect but not IBNM training.[15]

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[14] Appendix A.1 explores the rationales for enumerator fixed effects in greater detail.

[15] We randomized within strata but do not use strata fixed effects because they would absorb the pure control group. We include the control variables (ACLED incidents two years before randomization, region, and whether a location

We repeat these models with and without two sets of control variables. At the individual level, control variables are: ethnicity, autochthony, age, age squared, an index of household wealth, and sex.[16] Village level covariates are calculated using a 25 kilometer radius around the village centroid. They include the count of all ACLED incidents in the two years pre-treatment, the count of all deaths recorded by ACLED in the two years pre-treatment, the distance to a river with an average flow of at least one cubic meter per year, distance to an international border, and the count of direct Youth Connect beneficiaries per village.[17]

For the first list experiment, we estimate an equation of the form:

$$y_i = \beta_1 \text{Pure control} + \beta_2 \text{IBNM} + \beta_3 \text{Pure control} \cdot \tau_i + \beta_4 \text{IBNM} \cdot \tau_i$$
$$+ \beta_5 \tau_i + \psi_1 X_i + \psi_2 X_v + \gamma_c + \epsilon_i$$

where τ is the treatment indicator for the survey experiment. Regressing the count of selected outcomes on the treatment indicator by itself provides a population estimate of support for the randomly assigned additional item. Our coefficients of interest are the interaction between the treatment indicator and the respondents' village type (β_3 and β_4), which capture whether respondents in the pure control group or those in villages which received Youth Connect and IBNM respond differently than those in the villages which received Youth Connect only.

For the final set of list experiments, our estimand needs to take into account the factorial design. With three treatment arms assigned independently, a simple OLS estimation struggles to decompose treatment effects of the individual arms (Hainmueller, Hopkins, and Yamamoto 2014). Instead, one can obtain the causal effect of one of these three attributes via an average marginal component effect (AMCE).[18] Our estimand is the average marginal component interaction effect (AMCIE) between a respondent being assigned a treatment item and whether a respondent is in an IBNM treatment village.

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is a commune center) that we used to construct the strata. Our regressions still capture within strata variation because the strata are (nearly) linear combinations of these control variables while also obtaining a coefficient for the effect of being within a pure control village. The sampling strategy only took into account whether a village had one more ACLED incidents, but the coefficients are substantively identical for the effect of being sorted into IBNM training when using strata fixed effects.

[16] Autochthones are the descendants of the initial settlers of the village; while the term is most common in francophone Africa, elsewhere such residents are called 'indigenes' or 'sons of the soil.'

[17] Another threat to inference is interference from neighboring villages. In appendix A.4 we show that spatial proximity to IBNM treatment does not predict our outcomes, so spatial spillover is not a concern.

[18] AMCEs are the appropriate estimand for any factorial experiment, but are most commonly seen as part of conjoint survey experiments. A conjoint survey experiment is a special case of a factorial experiment (Hainmueller, Hopkins, and Yamamoto 2014).

Table 1. Respondents perceive lower community support for violence in villages which received IBNM training

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Youth Connect + IBNM	−0.042	−0.037	−0.041	−0.069	−0.153**	−0.139**	−0.131*	−0.130*
	(0.055)	(0.055)	(0.051)	(0.048)	(0.050)	(0.052)	(0.053)	(0.051)
Pure control	0.025	0.031	0.075	0.046	−0.035	−0.033	0.018	0.048
	(0.074)	(0.085)	(0.081)	(0.082)	(0.096)	(0.117)	(0.079)	(0.091)
Demographic Controls		X		X		X		X
Demographic Controls		X		X		X		X
Region Fixed Effects	X	X	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X	X	X
Mean of outcome	1.838	1.838	1.546	1.546	1.898	1.898	1.654	1.654
Num.Obs.	1721	1719	1717	1715	1719	1717	1726	1724
R2	0.496	0.508	0.490	0.503	0.520	0.542	0.392	0.402

Note: Data are from the Youth Connect midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following…' The reference level is villages that received Youth Connect only. Demographic controls include ethnicity, autochthony, age, age squared, household wealth, and sex. Village-level controls include the count of all ACLED incidents within a 25 kilometer radius, the count of all ACLED casualties within a 25 kilometer radius, the number of Youth Connect beneficiaries per village, the distance to a permanent source of water, the distance to an international border, and a commune center indicator. Table A5 shows coefficients for control variables. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

5 Results

Table 1 shows the effects of Youth Connect and IBNM treatment on the survey questions about community perceptions of support for violence. We find strong support for H2. Households in villages which received IBNM treatment are less likely to believe their community perceived violence as justified. Respondents which received IBNM training have a 0.16 to 0.18 standard deviation reduction in the perception that violence is justified to defend one's religion and a 0.18 standard deviation reduction in perception that violence is justified to retaliate against other violence. These reductions represent an approximately eight percent decrease in the perceived support for violence from the means. Respondents in IBNM villages were no less likely to report that violence was justified to defend one's ethnic group or to force the government to change its policies. The coefficients on IBNM training are negative for these columns, but they are not statistically significant.

Contra H.1, Table 1 shows no difference among households in the pure control villages and those that received only Youth Connect. These data suggest that the economic interventions and civic engagement activities of Youth Connect did not, by themselves, reduce support for violence. These results are similar with and without control variables, showing that control variables are

Table 2. Respondents perceive lower community support for violence in villages which received IBNM training (combined indices)

	Additive index		1st principal component	
	(1)	(2)	(3)	(4)
Youth Connect + IBNM	−0.325*	−0.334*	−0.169*	−0.168*
	(0.146)	(0.149)	(0.071)	(0.075)
Pure control	0.101	0.139	0.035	0.036
	(0.213)	(0.234)	(0.104)	(0.114)
Demographic Controls		X		X
Geographic Controls		X		X
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Mean of outcome	6.898	6.898	0.084	0.084
Num.Obs.	1734	1732	1694	1692
R2	0.574	0.588	0.607	0.621

Note: Data are from the Youth Connect midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following…' The reference level is villages that received Youth Connect only. Demographic controls include ethnicity, autochthony, age, age squared, household wealth, and sex. Village-level controls include the count of all ACLED incidents within a 25 kilometer radius, the count of all ACLED casualties within a 25 kilometer radius, the number of Youth Connect beneficiaries per village, the distance to a permanent source of water, the distance to an international border, and a commune center indicator. Table A6 shows coefficients for control variables. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

indeed orthogonal to treatment assignment. Table 2 replicates these results using combined indices of the survey questions about community perceptions of support for violence. Respondents within IBNM villages were consistently 0.14 standard deviations less likely to report community support for the use of violence (H.2). As above, however, we see no difference between pure control villages and those that received Youth Connect only (H.1).

Table 3 displays the results of the first list experiment, which backs out whether respondents themselves support the use of violence. A positive coefficient for the list treatment indicator translates to positive support for the controversial item—in this case, the statement 'it is justifiable to use violence for a political or religious cause.' Table 3 shows that this coefficient is consistently positive, although inconsistently statistically significant.

Columns three and four of table 3 show the results of interacting the list treatment indicator with the respondent's IBNM treatment group. These coefficients capture the extent to which different subgroups agree with the controversial item. A positive interaction would suggest that

Table 3. Respondents in IBNM treatment villagers are less likely to support using violence

	(1)	(2)	(3)	(4)
List treatment	0.043	0.058	0.137	0.201*
	(0.058)	(0.059)	(0.086)	(0.083)
List treatment * Pure control village			−0.031	−0.089
			(0.157)	(0.157)
List treatment * IBNM village			−0.224+	−0.322*
			(0.127)	(0.127)
Demographic Controls		X		X
Geographic Controls		X		X
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Mean of outcome	2.425	2.425	2.425	2.425
Num.Obs.	1730	1728	1730	1728
R2	0.307	0.356	0.317	0.366

Note: Data are from the Youth Connect midline survey. Outcome variables are responses to 'how many of these statements you agree with? Please don't tell me which items you agree with, just how many of them you agree with.' The list treatment item is 'it is justifiable to use violence for a political or religious cause.' The reference level is villages that received Youth Connect only. Demographic controls include ethnicity, autochthony, age, age squared, household wealth, and sex. Village-level controls include the count of all ACLED incidents within a 25 kilometer radius, the count of all ACLED casualties within a 25 kilometer radius, the number of Youth Connect beneficiaries per village, the distance to a permanent source of water, the distance to an international border, and a commune center indicator. Table A7 shows coefficients for control variables. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

members of the subgroup are more likely to agree with the treatment item when presented it.

We find that the interaction effect between the list treatment and being in a village that received both IBNM treatment and Youth Connect is negative and statistically significant, which supports H.2. In villages that received Youth Connect only, the marginal effect of being treated with the additional item is associated with a 0.166 point increase in the count of statements with which respondents agreed. Within villages that received both IBNM and Youth Connect, the marginal effect of being assigned the list treatment is statistically indistinguishable from zero. However, the interaction effect between being receiving the list treatment and the pure control indicator is neither statistically not substantively significant. In other words, respondents in pure control and Youth Connect villages agreed that it is sometimes justifiable to use violence; respondents in villages that received both Youth Connect and IBNM treatment did not. Youth Connect by itself did not reduce support for violence, but IBNM training alongside IBNM treatment reduced support for violence.

Finally, figure 2 illustrates the results from the set of factorial list experiments. It shows the

AMCEs and subgroup AMCIEs for being treated with the additional sensitive item. The overall AMCE for receiving any of these treatments, pooled across the different scenarios and IBNM treatment arms, is 0.086 percentage points. In other words, receiving the sensitive item as an option resulted in an 8.6 percent increase in the count of actions respondents said they would consider taking when averaged across the distribution of scenarios and other attributes. Within villages that received Youth Connect only, the AMCIE was 0.108. This means that 10.8 percent of respondents within Youth Connect only villages would consider using violence in response to one of these scenarios. This result is slightly above the conventional threshold for statistical significance ($p = 0.057$). In both the pure control group and the villages which received both Youth Connect and IBNM, treating respondents with the additional sensitive item did not lead respondents to select additional items. This figure shows weak support for H.2, but no support for H.1.

Respondents were more comfortable opting for violence in some of the scenarios but not others. Figure 2 shows that the effect of adding the additional sensitive item was highest when the opening scenario read "an elder has prevented all of the youth from attending a meeting in which the village will discuss the location of a new well or borehole" and "Somebody from the village has given bad advice to the chief which will lead him to take decisions which will make it more difficult for your ethnic group to access services." In these scenarios, the unconditional AMCEs were 0.158 and 0.134 respectively, meaning that approximately 16 and 13 percent of respondents said they could consider an additional action when presented with the option of violence. Overall, the factorial list experiments show that support for using violence is somewhat concentrated within villages that received Youth Connect only. Figure 2 provides suggestive, but not conclusive, evidence that the willingness to use violence is lower in the villages that received IBNM treatment and Youth Connect than in the villages which received Youth Connect only.

Putting together all these results, we can say that the combination of Youth Connect and village-level IBNM participation reduced support for violence among village youth. These results hold for both direct Youth Connect beneficiaries and youth sampled by a random walk, suggesting that IBNM's training-of-trainers model diffused the results of the IBNM training within the villages. These results are strongest when asking whether "people in your community think it is... justified to use violence." This result is consistent with the IBN's training-of-trainers model, which should lead to the effects of the training being diffused among villagers. Survey experiments show similar results. These results support our second hypothesis: respondents in villages that received IBNM alongside Youth Connect had lower support for violence than respondents in villages which received Youth Connect only.

Figure 2. Support for violence is weakly concentrated within villages that received Youth Connect only
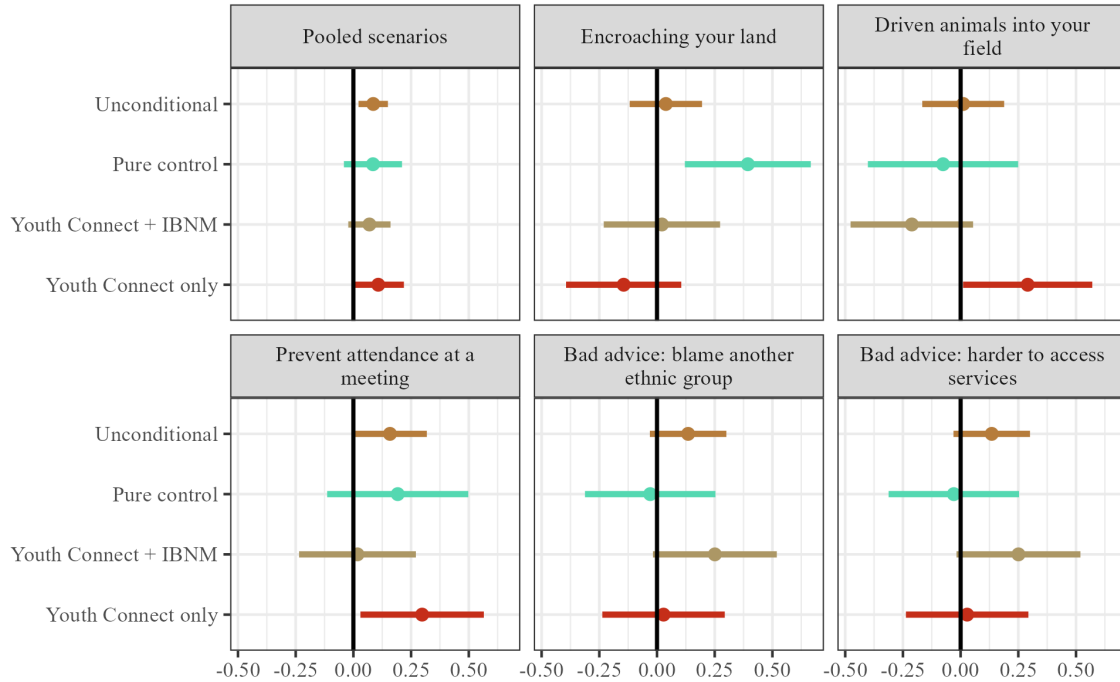
Data are from the Youth Connect midline survey. This figure shows AMCEs and AMCIEs from a series of factorial list experiments that ask respondents to consider a scenario and then say "how many of [the following actions] would you consider?" AMCIEs show the within treatment group effect of adding an additional sensitive item to the list of responses: either "threaten the other person with a weapon" or "chase the other person out of the village." Standard errors are clustered at the respondent level. All regressions use within-sample inverse probability weighting.

Our results accord with a growing body of research which highlights the difference between asking about support for violence in the abstract, and asking about support for specific violent actions (Westwood et al. 2022). When we ask people about general support for "violence," we see stronger results. When we ask people about "threaten[ing] the other person with a weapon" or "chas[ing] the other person out of the village," we see weaker effects. One explanation for this difference could be floor effects: weaker reductions in support for specific violent actions may reflect lower baseline support. Nevertheless, these results are encouraging: IBNM reduces support for violence.

We also explored a variety of heterogeneous treatment effects (HTEs) which we specified

in the pre-analysis plan.[19] First, the effect of receiving Youth Connect and IBNM training is stable across ethnicity, with the exception of respondents belonging to the Zarma ethnicity (also known as Songhay-Zarma). The Zarma are the largest minority within our survey: approximately 52 percent of respondents identified as Haoussa, 29 percent as Zarma, 10 percent as Peul (also known as Fulani), 9 percent as Touareg, and the remainder as 'other' ethnicities. Among Zarma respondents, participation in both IBNM and Youth Connect did not lead to a marginal decrease in support for violence, relative to participation in Youth Connect alone. However, this finding is likely explained by geographic concentration: all Zarma respondents live in Tillabéri, which faces greater exposure to violence due to its proximity to both Burkina Faso and Mali. Table **??** shows no different in treatment effects between autochthones and other respondents. However, relatively few allochthones participated in the survey, so these subgroup effects may be underpowered.

We pre-registered tests for HTEs by responses to a series of questions that ask about participation in community decision making. Respondents provided five-point likert responses to track their agreement with statements such as "I have opportunities to participate in making decisions about my community" and "I feel that my voice is being heard when dealing with my community." In the pre-analysis, we speculated that these intermediate outcomes would show either increased youth participation in decision making or increased satisfaction with the decisions made by community. However, table A10 in the appendices shows that that treatment does not vary based on these intermediate outcomes.

We also pre-registered a non-parametric, machine-learning based strategy to uncover sources of treatment effect heterogeneity within the data. Specifically, we estimate conditional average treatment effects (CATEs) using a data-driven approach to select variables, which means our pre-analysis plan did not enumerate the specific moderators we explore here. Following Athey and Wager (2018), we estimate CATEs using a series of causal forests. Calculating variable importance then shows that that adding IBNM training to Youth Connect was more effective in reducing support for violence among respondents who were further from water, who were closer to market towns, and who had greater availability of water within 10 kilometers of their village. The fact that Youth Connect had greater effects among villages which were further from water and those that had less access to water within 10 kilometers reinforces the idea that the economic activities under the umbrella of Youth Connect did, in fact, matter. Villages with greater access to water are likely to be more economically successful, and so treatment seems to have been more effective

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[19]We focus on HTEs for the direct outcomes (the 'justified') questions, because adding an interaction to the survey experiments adds computational complexity and reduces statistical power.

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in poorer villages.[20]

6   Difference-in-difference with conflict data

The previous section shows that the combination of Youth Connect and IBNM training reduces support for violence. Does this reduced support for violence translate into a reduction in violence? We used data from the ACLED project to show that villages which received both IBNM training and Youth Connect experienced fewer violent incidents than those which received only Youth Connect. ACLED data are available over time, so we leverage a difference-in-differences design (Raleigh et al. 2010). This analysis is particularly important given the worsening security situation in Niger: even if the count of violent events increased in both villages that received only Youth Connect and those that received Youth Connect alongside IBNM, the latter group may have experienced a smaller increase in the former. This difference-in-differences analysis using ACLED data was not pre-registered, so the following results should be treated as exploratory rather than confirmatory.

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[20]The mean household wealth index in villages with below median distance to water is 0.16 standard deviations higher than the mean household wealth index in villages with distance to water above the median.

Figure 3. Average count of ACLED incidents within 25 kilometers of program villages
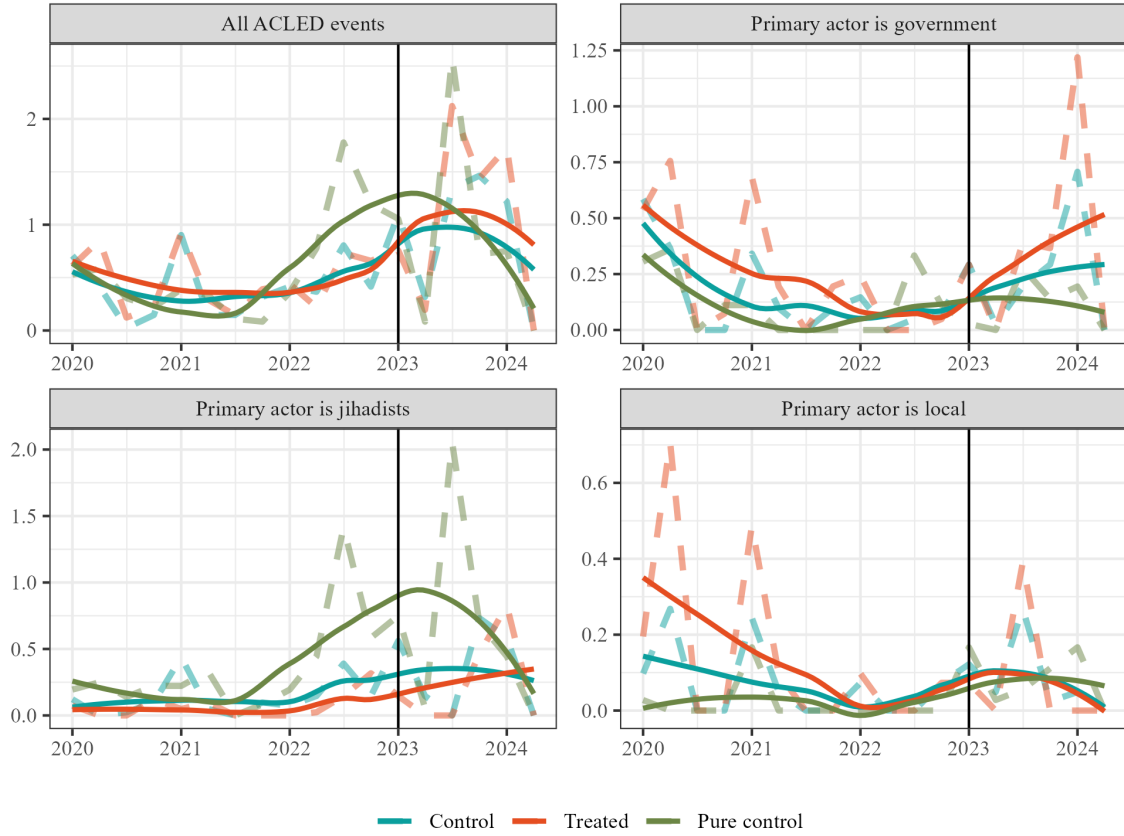
This figure shows the average count of ACLED events within a 25 kilometer radius of program villages, across the three treatment groups. The dashed line shows the raw counts; the smooth line uses LOESS to calculate a moving average. Black lines indicate the date of treatment.

For the difference-in-differences estimator to be valid, villages which received both Youth Connect and IBNM would have had similar outcomes to those that received only Youth Connect, in the counterfactual state where IBNM training did not take place (i.e. the parallel trends assumption). Figure 3 displays the quarterly sum of ACLED incidents across the Youth Connect only, Youth Connect plus IBNM, and pure control villages. We also break down ACLED incidents by whether the primary actor recorded in the ACLED database was a government (or its military), a jihadist organization (Katiba Macina, Boko Haram, or Jama'at Nusrat al-Islam wal-Muslimin), or a local actor such as a village or ethnic militia.

Table 4. Difference in difference coefficients for ACLED incidents six months before and after IBN

	All incidents	Government	Jihadist group	Local groups
D-in-D: IBNM	−0.528+	0.836	−1.429*	−0.847
	(0.280)	(1.054)	(0.708)	(1.155)
Village + period FEs	X	X	X	X
Num.Obs.	174	52	78	44
R2	0.456	0.272	0.528	0.156

Note: This figure reports the TWFE difference-in-difference coefficient comparing villages which received IBNM and Youth Connect and villages which only received Youth Connect. Outcome variables are calculated from ACLED incidents in the six months before and after treatment within a 25km radius of the village. All models use a Poisson regression; standard errors are clustered at the village level.

Figure 3 provides visual support for the parallel trends assumption between villages which received only Youth Connect and villages that received Youth Connect and IBNM. The average counts for different incidents move in parallel for extended periods of time before treatment (the date at which IBNM training took place, are indicated by the black line). This assumption does not hold for the pure control group. While assignment into IBNM training was fully randomized at the village level, pure control villages are clustered within distinct communes, which were selected non-randomly. The geographic and demographic differences between Youth Connect and Youth Connect plus IBNM villages are small; those between Youth Connect communes and pure control communes are greater. As a result, I report only the difference-in-difference coefficients for the treatment versus control communes.

In our case, a two-way fixed effects (TWFE) design recovers our the LATE of IBNM training, because all units received treatment at the same time and the parallel treatment assumption holds (Roth et al. 2023: 2220). One important caveat is that many villages did not experience a single ACLED event during this period of time—which means these villages drop out of the estimation, leading the low numbers of observations. Unique among this paper's models, table 4 uses Poisson regressions rather than OLS to account for the count structure of the data.

Table 4 shows an overall decrease in violent events in villages that received the IBNM treatment, relative to a control group. While the coefficient is weakly positive for the overall count of ACLED incidents ($p = 0.059$), it is clear that these treatment results are concentrated in the count of incidents where jihadist groups were the primary actor. Moving from the treatment to the control group is associated a marginal decrease of 1.16 incidents instigated by jihadist organi-

zations, a decrease of 0.55 standard deviation against a mean of 0.75 events per village.

Table 4 includes ACLED events six months before and six months after IBNM training took place. Figure E2 in the appendix reinforces these conclusions by showing the results are largely invariant to the choice of window. When the outcome variable is the count of all ACLED events, the difference-in-difference coefficient is statistically significant using windows between two and six months. However, for the count of events for which a jihadist organization is the primary actor, this coefficient is significant for any window between three and 10 months. Consistent with table 4, there is no window for which IBNM treatment reduced the count of incidents for which the primary actor is either local militia or the Nigerien government.[21] These results using external data on conflict incidents from ACLED provide promising and robust evidence that villages whose experience with Youth Connect was supplemented by IBNM training experienced a reduction in violent events, relative to villages that received Youth Connect only.

What explains these reductions? Jihadist groups are often mobile, which suggests that decreased recruitment in IBNM treatment villages is unlikely to decrease jihadist activity. However, as Raleigh, Nsaibia, and David (2021: 123) point out, these groups "have exploited local ethnic cleavages while self-defense groups, vigilantes, and community-based militias have become increasingly embroiled in counter-terrorism efforts." In such a context, it seems more likely that treatment with IBNM has helped youth to resolve their own conflicts as well as other conflicts in the community before jihadist organizations can intervene. Where small scale conflicts are resolved early, there exist fewer cleavages onto which external violent groups can agglomerate, and thus fewer violent incidents involving these groups (Kalyvas 2003).

7 Discussion and Conclusion

This article presents results from an RCT in the Maradi and Tillabéri regions of Niger which randomized assignment into IBNM training—a conflict management program—within an economic and civic engagement intervention called Youth Connect. We test the effects of this intervention using a survey of 1,734 youth across 118 villages, including 41 villages that received Youth Control, 41 villages that received Youth Control and IBNM training, and 36 villages which serve as a pure control group.

Youth who participated in Youth Connect were no less likely to support violent extremism or perceive violence to be justified than youth in villages that did not receive Youth Connect. Youth

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[21]Results are similar when using 10 kilometer radii rather than 25 kilometer radii as in table 4. However, the sample size further dwindles. We show these results in figure E1.

that received both Youth Connect and IBNM perceived less community support for violence than youth in villages that received Youth Connect only. In addition, youth that participated in IBNM were less likely to themselves support the use of political violence, as demonstrated by a series of survey experiments. Youth Connect by itself did not reduce support for political violence, but combining Youth Connect with IBNM training made significant progress in reducing support for violence. However, this is not to say that the economic effects were unimportant: our non-parametric strategy for identifying heterogeneous treatment effects shows that IBNM training was most effective in reducing support for violence in economically deprived areas.

Beyond support for violence, adding IBNM to Youth Connect reduced the incidence of violence. Using a difference-in-differences framework, we show that villages which received IBNM experienced fewer violent incidents than villages which received Youth Connect only. This reduction in violence is concentrated among events instigated by jihadist groups such as JNIM or AQIM, which accords well with a bottom-up production function of violence (Kalyvas 2006). By helping villages to resolve disputes before armed groups can interject themselves or villagers can appeal to armed groups, Youth Connect successfully prevented the jihadist groups from instigating violent events in treatment villages. Our survey data also show that the lessons of IBNM diffused beyond the initial program participants: survey respondents selected as part of a random walk also expressed reduced support for violence.

IBNM training is a light-touch, low cost intervention which supplements existing programming on countering violence and reducing vulnerability to violent extremism. We apply IBNM, a tested intervention, to a novel context by targeting youth rather than village elites (Christensen et al. 2024; Reardon, Wolfe, and Ogbudo 2021). IBNM expands a growing toolkit of policy interventions, and further explores how different interventions to reduce support from violence complement each other (Bhatt et al. 2024; Blattman, Jamison, and Sheridan 2017). IBNM also takes a bottom-up strategy to build peace, but giving youth the skills they need to peacefully resolve their own conflicts and to mediate other conflicts within this community.

Niger is a particularly difficult context in which to move the needle on support for violence. It is a 'complex emergency' in which multiple risk factors for violence overlap. It is one of the poorest countries in the world and has suffered from both political instability and climate-driven insecurity. Porous borders permit neighboring conflicts in Mali, Burkina Faso, and Nigeria to percolate into Niger and give insurgents a path to retreat from the Nigerien armed forces. That IBNM and Youth Connect managed to reduce violence in this complicated situation suggests that layering IBNM atop conventional violence reduction programs may be effective in other fragile environments.

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Appendices

Table of Contents

A   Estimation details

A.1   Tests for enumerator effects

While the pre-analysis plan did not include enumerator fixed-effects, this section shows they are necessary to explain a significant portion of the variation seen in the outcome measures. The median village was visited by four different enumerators. The median enumerator visited 16 different villages. This survey used two sets of enumerators. An external survey firm, Appui Conseil Internationale pour le Développement (ACID) engaged one group of enumerators. These enumerators administered the midline survey to all respondents in Tillaberi and the direct program beneficiaries in Maradi. The decision to hire an external survey firm was undertaken by USAID, who wanted an independent firm to conduct a performance evaluation using survey responses from the direct beneficiaries. Mercy Corps' Niger country office directly hired enumerators to administer the random sample in Maradi.

Enumerator effects refer to enumerator characteristics altering the responses of survey participants. In African contexts, enumerator effects are often driven by mismatches in sex or ethnicity (Adida et al. 2016). Di Maio and Fiala (2020) show that enumerator effects can explain up to 30 percent of variation in respondents' answers to questions about political preference. In less sensitive questions, however, this amount can be much lower. In the context of this field survey, we expect sensitive questions about violence to have relatively higher rates of enumerator effects.

Figure A1 shows the amount of variation explained by adding additional variables to a pair of regressions. The lefthand panel of A1 shows a regression where the outcome is an index of responses to "Do people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following" summed across the various scenarios. The righthand panel of figure A1 shows the same breakdown, but for a regression where the outcome is the sum of items a respondent reports their household as owning—a non sensitive survey item (this is the wealth index we use as a control variable elsewhere in the paper, and an outcome in appendix B).

Simply regressing the outcome on a region indicator (Maradi or Tillabéri) explains almost no variation. Adding commune indicators explains six to nine percent of variation, and adding villages explains an additional 14-19 percent of variation. If enumerator effects are strong, they will explain large percentages of additional variation. Enumerator effects explain 44 percent of variation for questions about support for violence. In contrast, enumerator effects explain only four percent of variation for the non-sensitive survey item. This figure clearly shows that enumer-

Figure A1. Enumerator fixed effects explain a meaningful fraction of total variance
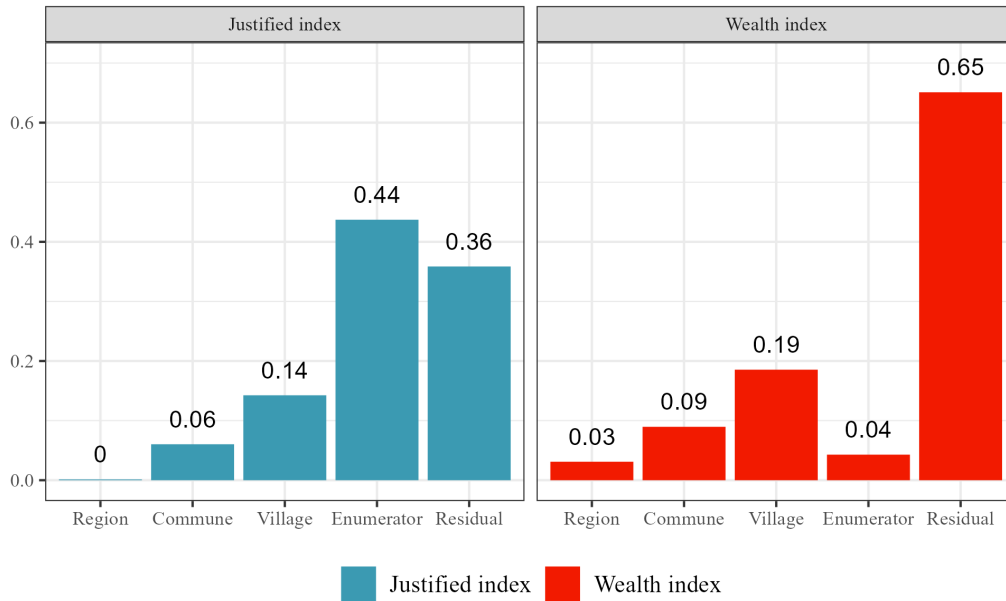
This figure shows the marginal percentage of additional variation explained by geographic variables, the sample type (direct beneficiaries versus randomly sampled youth), and the enumerator fixed effects in a pair of regressions.

ator fixed effects explain much of the variation in survey responses, but only for the questions about violence.

Enumerator effects error are a source of measurement error which affects the outcome variables of our research (because the sensitive items in out survey are the outcome). There is no measurement error in the treatment assignment, since we know exactly which villages have been treated. Given measurement error in the outcome variable, OLS remains unbiased and consistent (under the usual OLS assumptions) **if and only if** the error component is uncorrelated with treatment assignment.

We can directly explore the consistency of OLS by varying the model specification. Table A1 shows the effect of IBN treatment at the village level on the additive justified index. Overall, the coefficient on the binary indicator for the participant being in village treated with IBN is relatively consistent. While the level of statistical significance bounces around, the magnitude and direction of the coefficient is similar across specifications. This treatment effect naturally attenuates when further controls are added, but remains negative. In particular, adding commune

indicators decreases the magnitude of results, but this is unsurprising given the small number of villages per commune (six). This table suggests that enumerator effects are orthogonal to treatment assignment, at least for the comparison between villages that received Youth Connect only and villages that received Youth Connect plus IBNM. Enumerator effects explain additional variation, and in so doing reduce standard errors, but they are uncorrelated with the effect of treatment.

However, this table casts doubt on comparisons between pure control villages and villages that received Youth Connect only, as these estimates are not consistent. Both the magnitude and significance of this coefficient disappear when adding enumerator fixed effects. The pure control coefficients cannot be calculated with commune fixed effects because the pure control assignment took place at the commune level, so any coefficient would be entirely collinear with commune fixed effects and thus mechanically dropped from the regression.

Table A2 shows the average fitted values obtained by regression the justified index on enumerators, controlling only for region. This table confirms that enumerator effects are uncorrelated with IBN treatment/control villages, but it does suggest that enumerator effects could affect the results for pure control villages. The result is qualitatively similar if one residualized the outcome variable over commune, in a Frisch-Waugh-Lovell-style regression.

If enumerator effects are positively correlated with being in a pure control commune, the we would expect to see the coefficient of interest being upward biased (and consistently significant). This is consistent with the results that we observe. To summarise: comparisons between villages that receive IBNM alongside the Youth Connect program and villages that received Youth Connect by itself remain valid. Comparisons between Youth Connect-only villages and the pure control villages are likely correct, because we control for this source of error through enumerator fixed effects, but this cannot be verified.

A.2 Survey weights

In this paper, we weight our regressions to be representative of youth within the villages and commune centers selected for IBNM and Youth Connect, not to be representative of the broader population of youth in Niger. In other words, we weight to uncover the LATE for villages selected for Youth Connect. We weight in this manner because the non-random selection of villages into Youth Connect (via the village selection tool) means that we are not be able to generalize our results to the broader population (or at least not with further argument).

For our inverse probability sampling weights, we take the sample as given, and then calculate

Table A1. Effect of IBN treatment on direct support for violence across model specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control village	0.826*	0.101	0.745*	0.139				
	(0.347)	(0.213)	(0.323)	(0.234)				
Treatment village	−0.230	−0.325*	−0.398	−0.334*	−0.217	−0.204	−0.117	−0.155
	(0.272)	(0.146)	(0.220)	(0.149)	(0.267)	(0.164)	(0.281)	(0.173)
Direct YC recipient	0.926**	−0.980*	0.939**	−0.990*	1.033**	−1.027*	1.073**	−0.877*
	(0.339)	(0.395)	(0.327)	(0.451)	(0.312)	(0.410)	(0.319)	(0.397)
Ethnicity: Peul			−0.453	−0.495*			−0.318	−0.469*
			(0.295)	(0.198)			(0.257)	(0.213)
Ethnicity: Tuareg			0.568	−0.334			0.573	−0.129
			(0.457)	(0.174)			(0.424)	(0.207)
Ethnicity: Zarma			0.760*	−0.259			0.375	−0.413**
			(0.343)	(0.174)			(0.451)	(0.157)
Age			−0.148	−0.094			−0.158	−0.094
			(0.133)	(0.088)			(0.141)	(0.089)
Age squared			0.002	0.002			0.003	0.002
			(0.003)	(0.002)			(0.003)	(0.002)
Female			−0.026	−0.196			−0.026	−0.197
			(0.145)	(0.115)			(0.141)	(0.112)
ACLED events			−0.011	−0.013**			0.020	0.002
			(0.008)	(0.004)			(0.046)	(0.029)
ACLED deaths			−0.001	0.000			−0.035	−0.009
			(0.007)	(0.004)			(0.023)	(0.013)
N. of YC beneficiaries			0.003	0.000			0.001	−0.001
			(0.003)	(0.002)			(0.003)	(0.002)
Distance to a river			−0.054*	−0.015			−0.048	−0.046*
			(0.025)	(0.017)			(0.027)	(0.021)
Distance to the border			−0.010**	−0.006*			0.008	0.009
			(0.004)	(0.003)			(0.015)	(0.008)
Commune seat			0.181	0.078			0.154	0.079
			(0.238)	(0.154)			(0.220)	(0.152)
Region Fixed Effects	X	X	X	X				
Commune Fixed Effects					X	X	X	X
Enumerator Fixed Effects		X		X		X		X
Num.Obs.	1734	1734	1732	1732	1734	1734	1732	1732
R2	0.041	0.574	0.092	0.588	0.110	0.597	0.144	0.607

Note: Data are from the Youth Connect midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following…' The reference level is villages that received Youth Connect only. Demographic controls include ethnicity, autochthony, age, age squared, household wealth, and sex. Village-level controls include the count of all ACLED incidents within a 25 kilometer radius, the count of all ACLED casualties within a 25 kilometer radius, the number of Youth Connect beneficiaries per village, the distance to a permanent source of water, the distance to an international border, and a commune center indicator. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

Table A2. Fitted values from enumerator fixed effects by treatment status

Treatment status	Sample type	Mean	Std.Err.
Control	Random sample	6.94	0.50
Control	Beneficiary	6.67	0.44
Treated	Random sample	6.65	0.67
Treated	Beneficiary	6.78	0.61
Pure control	Random sample	7.25	0.71

Note:
This table shows the average fitted values obtained by regressing the additive index of the 'justified' questions on enumerator fixed effects, controlling for region.

a youth's probability of being chosen. For the beneficiary sample, the probability is even—every youth in the list of beneficiaries had an equal probability of being chosen for the sample. For the random sample of youths, we calculate two numbers. First, using the 2014 Nigerien census (Répertoire des localités), I divide the number of households we sampled in the village by the total number of households per village. Second, we use the number of youth per household, which we ask as part of the two-stage random selection of youth. I multiple these numbers together and take the inverse.[22]

We add two more post-processing steps. First, we winsorize the within-sample weights for the random sample. This means any weights below the 5th percentile are set to the 5th percentile, and any weights above the 95th percentile are set to the 95th percentile. This process means our results are less vulnerable due to outliers. Second, we balance the relative weights of the random sample and the beneficiary sample match their frequency in the data. In other words, the sum of the weights of the random sample divided the sum of the weights of the beneficiary sample are equal to the count of observations for randomly sampled youth in our survey divided the count of observations for beneficiaries in the survey. Direct program beneficiaries make up 29 percent of our sample and 29 percent of the total weights.

For survey weights, the important criteria is that the weights are balanced across treatment groups. Figure A2 shows the density of these weights. These weights are indeed balanced—which means that we can proceeded confidently using these within-sample survey weights.

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[22]The Répertoire des localités is a rough measure of village population, both because of its age and because of potential survey errors. This presents another reason to winsorize, to prevent errors in the census from proliferating into dramatic over or under weighting.

Figure A2. Within-sample survey weights are balanced across groups
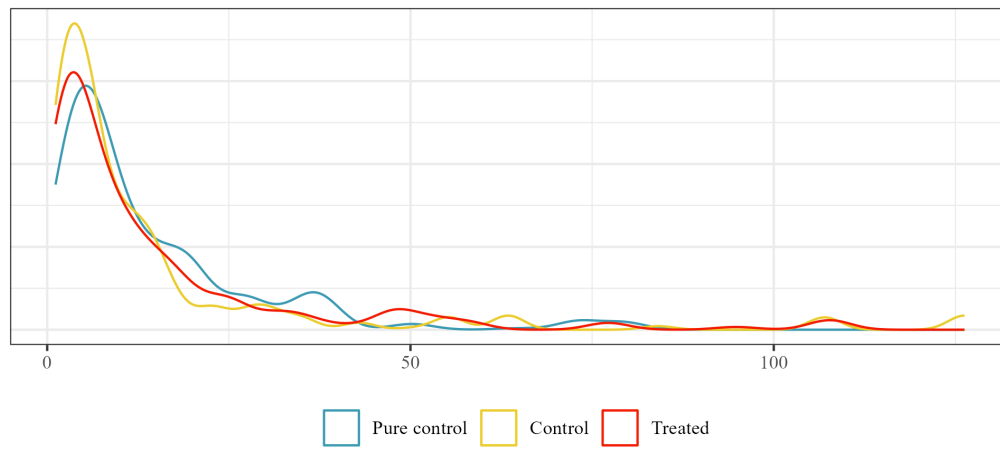
This figure shows the density of the within-sample inverse probability survey weights across all three IBNM and Youth Connect treatment groups.

A.3 Balance table

Table A3. Youth Connect and the IBNM treatment group are balanced across observable characteristics

	IBNM + Youth Connect		Youth Connect only		Difference
	Mean	Std.Err.	Mean	Std.Err.	T-value
Individual-level covariates					
Age	23.04	5.57	22.49	5.59	1.43
Female	0.41	0.49	0.39	0.49	0.70
Wealth index	1.03	1.37	1.15	1.40	-1.33
Ethnicity: Haoussa	0.63	0.48	0.68	0.47	-1.56
Ethnicity: Zarma	0.22	0.41	0.24	0.43	-0.79
Ethnicity: Peul	0.04	0.20	0.04	0.19	0.26
Ethnicity: Tuareg	0.09	0.28	0.02	0.15	4.21
Education: madrassa	0.19	0.39	0.14	0.35	2.00
Education: none	0.14	0.34	0.12	0.33	0.68
Education: primary	0.26	0.44	0.23	0.42	1.01
Education: secondary/higher	0.35	0.48	0.49	0.50	-4.16
Village-level covariates					
Count of all ACLED events	10.02	19.11	8.95	14.82	0.91
Count of all ACLED deaths	3.39	5.36	3.37	4.76	0.07
Count of Youth Connect beneficiaries	37.66	50.94	34.59	50.14	0.88
Distance to water	5.00	4.51	4.46	4.06	1.81
Distance to a border	73.80	33.18	71.87	35.32	0.82

Note: Individual level covariates are from the 2024 IBNM and Youth Connect midline survey. Statistics from survey data use within-sample inverse probability weights; village-level statistics are unweighted.

Table A4. Spatial proximity to IBNM treated villages does not predict support for violence

Outcome	Estimate	Std.Err.	T-statistic	P value
Defend your ethnic group	0.003	0.002	1.325	0.193
Force the government to change their policies	-0.002	0.002	-0.828	0.412
Defend your religion	0.002	0.003	0.931	0.357
Retaliate against violence	0.002	0.003	0.812	0.422
Index (1st principal component)	0.004	0.003	1.082	0.286

Note: This table estimates the spatial spillover of IBNM treatment; the sample is restricted to villages that received Youth Connect treatment only. All regressions use OLS with region and enumerator fixed effects. Standard errors are clustered at the village level.

A.4 Spatial spillover

This appendix explores the potential for spatial spillover from IBNM treatment. In other words, did a nearby village being treated with IBNM in addition to Youth Connect affect outcomes in villages that only received Youth Connect?

The mean distance between villages that received Youth Connect only and the nearest village that also received IBNM training is 9.45 kilometers. The median distance is 6.98 kilometers. Between villages in the pure control communes and villages treated with IBNM, those statistics are 20.3 kliometers and 17.4 kilometers respectively. Table A4 tests for spillover by regressing the direct measures of support for violence on the distance to a village treated with IBNM, subsetting to villages that received Youth Connect only. The table shows that spatial proximity to villages treated with IBNM does not predict attitudes towards violence. In other words, spatial spillover does not appear to have driven our results.

A.5 Village selection details

Mercy Corps selected twenty communes to receive Youth Connect when the project was conceived in 2020. Within these 20 communes, Mercy Corps had the capacity to implement Youth Connect in 14 communes during the program's first phase. Mercy Corps' country office identified 10 of these communes as priorities to receive Youth Connect; within the other ten, four were randomly selected to received Youth Connect in the first phase, and the remaining six were selected to receive Youth Connect in the second phase. These six communes form the pure control in this sample.

Within communes, Mercy Corps selected five villages to receive Youth Connect, in addition to the commune centers. These five villages were selected through Mercy Corps' 'Village

Selection Tool." Mercy Corps' field staff convened meetings at the commune level where village elites ranked their villages across 28 different criteria. The categories of criteria included: education and training, ideological beliefs, socio-economic conditions, politics and governance, psychosocial factors, the presence of multiple ethnic groups, and dialogue, and conflict resolution institutions.[23] Villages were ranked from zero to three across each of these criteria. At the end of the meeting, the scores were added together, and the five villages with the highest total scores were selected for Youth Connect.

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[23]For more details on village selection, see Ribar, Sheely, and Lichtenheld 2023.

Table A5. Coefficient values for table 1

	Defend ethnic group	Change policies	Defend religion	Retaliate
	(1)	(2)	(3)	(4)
Direct YC recipient	−0.182+	−0.234+	−0.305*	−0.170
	(0.106)	(0.129)	(0.127)	(0.129)
Ethnicity: Peul	−0.131+	−0.073	−0.111	−0.094
	(0.070)	(0.048)	(0.068)	(0.070)
Ethnicity: Tuareg	−0.074	0.031	−0.272**	0.016
	(0.099)	(0.060)	(0.086)	(0.076)
Ethnicity: Zarma	−0.041	−0.026	−0.188*	0.057
	(0.090)	(0.068)	(0.085)	(0.063)
Autochthone	−0.092	0.031	−0.026	−0.050
	(0.099)	(0.050)	(0.053)	(0.074)
Age	−0.054	−0.048+	−0.042	0.030
	(0.040)	(0.028)	(0.034)	(0.044)
Age squared	0.001	0.001	0.001	−0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Female	−0.091*	−0.077*	0.007	−0.013
	(0.041)	(0.039)	(0.047)	(0.037)
ACLED events	−0.002	−0.003*	−0.005**	−0.003
	(0.002)	(0.001)	(0.001)	(0.002)
ACLED deaths	−0.001	0.002+	−0.001	0.000
	(0.001)	(0.001)	(0.002)	(0.001)
N. of YC beneficiaries	0.000	0.001	−0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
Distance to a river	0.007	−0.010**	−0.002	−0.003
	(0.005)	(0.004)	(0.006)	(0.007)
Distance to the border	−0.002+	−0.001*	−0.002+	−0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Commune seat	−0.057	0.080+	0.076	−0.001
	(0.051)	(0.043)	(0.060)	(0.066)
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Num.Obs.	1719	1715	1717	1724
R2	0.508	0.503	0.542	0.402

Note: This table shows coefficients or covariates from table 1. Data are from the Youth Connect midline survey. Outcome variables are responses to '[d]o people in your community think it is some-times, rarely, or never justified to use violence to do each of the following…' The reference level is villages that received Youth Connect only. All regressions use OLS with within-sample inverse proba-bility weights; standard errors are clustered at the village level.

Table A6. Coefficient values for table 2

	Additive index	1st principal component
	(1)	(2)
Direct YC recipient	−0.990*	−0.436*
	(0.451)	(0.214)
Ethnicity: Peul	−0.495*	−0.213*
	(0.198)	(0.088)
Ethnicity: Tuareg	−0.334+	−0.192*
	(0.174)	(0.093)
Ethnicity: Zarma	−0.259	−0.137
	(0.174)	(0.090)
Autochthone	−0.157	−0.072
	(0.171)	(0.088)
Age	−0.094	−0.066
	(0.088)	(0.044)
Age squared	0.002	0.001
	(0.002)	(0.001)
Female	−0.196+	−0.086
	(0.115)	(0.056)
ACLED events	−0.013**	−0.006*
	(0.004)	(0.002)
ACLED deaths	0.000	−0.001
	(0.004)	(0.002)
N. of YC beneficiaries	0.000	−0.001
	(0.002)	(0.001)
Distance to a river	−0.015	−0.002
	(0.017)	(0.008)
Distance to the border	−0.006*	−0.003*
	(0.003)	(0.001)
Commune seat	0.078	0.031
	(0.154)	(0.075)
Region Fixed Effects	X	X
Enumerator Fixed Effects	X	X
Num.Obs.	1732	1692
R2	0.588	0.621

Note: This table shows coefficients or covariates from table 2. Data are from the Youth Connect midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following…' The reference level is villages that received Youth Connect only. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

Table A7. Coefficient values for table 3

	Additive index	1st principal component
	(1)	(2)
Direct YC recipient	1.099***	1.107***
	(0.183)	(0.185)
Ethnicity: Peul	−0.069	−0.051
	(0.099)	(0.093)
Ethnicity: Tuareg	0.230+	0.202
	(0.130)	(0.127)
Ethnicity: Zarma	0.109	0.110
	(0.132)	(0.125)
Autochthone	−0.133	−0.144
	(0.095)	(0.095)
Age	−0.040	−0.037
	(0.041)	(0.039)
Age squared	0.001	0.001
	(0.001)	(0.001)
Female	0.035	0.046
	(0.053)	(0.053)
ACLED events	−0.004**	−0.005**
	(0.001)	(0.002)
ACLED deaths	0.004**	0.005**
	(0.001)	(0.002)
N. of YC beneficiaries	0.001	0.001
	(0.001)	(0.001)
Distance to a river	−0.004	−0.003
	(0.008)	(0.008)
Distance to the border	0.001	0.001
	(0.001)	(0.001)
Num.Obs.	1728	1728
Region Fixed Effects	X	X
NA	NA	NA
Enumerator Fixed Effects	X	X
R2	0.356	0.366

Note: This table shows coefficients or covariates from table 3. Outcome variables are responses to 'how many of these statements you agree with? Please don't tell me which items you agree with, just how many of them you agree with.' The list treatment item is 'it is justifiable to use violence for a political or religious cause.' The reference level is villages that received Youth Connect only. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

A.6 Heterogenous treatment effects

Table A9 shows the effects of Youth Connect and IBNM treatments broken out by ethnicity.[24] The effect of receiving Youth Connect and IBNM training is stable across ethnicity, with the exception of respondents belonging to the Zarma ethnicity (also known as Songhay-Zarma). The Zarma are the largest minority within our survey: approximately 52 percent of respondents identified as Haoussa, 29 percent as Zarma, 10 percent as Peul (also known as Fulani), 9 percent as Touareg, and the remainder as 'other' ethnicities. Among Zarma respondents, participation in both IBNM and Youth Connect did not lead to a marginal decrease in support for violence, relative to participation in Youth Connect alone. However, this finding is likely explained by geographic concentration: all Zarma respondents live in Tillabéri, which faces greater exposure to violence due to its proximity to both Burkina Faso and Mali. IBNM training had weaker effects in Tillabéri overall, which suggests the HTEs for Zarma respondents reflects greater exposure to violence.

Autochthones are villagers who consider themselves descendants of the first families to settle a village."[25]. However, table A8 shows no different in treatment effects between autochthones and other respondents. Relatively few allochthones participated in the survey, so these subgroup effects may be underpowered. This table shows heterogenous treatment effects for the additive index of the direct survey questions about support for violence extremism.

We pre-registered tests for HTEs by responses to a series of questions that ask about participation in community decision making. Respondents provided five-point likert responses to track their agreement with:

- I have **opportunities** to participate in making decisions about my community

- I feel that my voice is **heard** when dealing with my community

- I feel that my voice is **heard by the** village and **administrative authorities** when dealing with them

- I feel like an **active** member of my village.

- I can **influence** the decisions made in community meetings.

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[24]We focus on HTEs for the direct outcomes (the 'justified') questions, because adding an interaction to the survey experiments adds computational complexity and reduces statistical power.

[25]Conflicts between autochthones and allochthones, or relative strangers, was a primary driver of land conflict in Côte d'Ivoire (Boone 2003).

Table A8. Heterogenous treatment effects for Youth Connect and IBNM by autochthony

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control village	0.016	0.031	0.118	0.086	−0.180	−0.242	−0.176	−0.104
	(0.232)	(0.230)	(0.132)	(0.139)	(0.118)	(0.138)	(0.171)	(0.163)
Treatment village	0.057	0.075	0.034	−0.005	−0.110	−0.067	−0.513***	−0.509***
	(0.223)	(0.213)	(0.128)	(0.133)	(0.114)	(0.123)	(0.133)	(0.129)
Pure control village * autochthone	0.009	0.001	−0.052	−0.047	0.170	0.251	0.229	0.182
	(0.253)	(0.237)	(0.136)	(0.133)	(0.122)	(0.132)	(0.177)	(0.167)
Treatment village * autochthone	−0.107	−0.122	−0.084	−0.071	−0.043	−0.071	0.426**	0.420**
	(0.225)	(0.217)	(0.131)	(0.134)	(0.112)	(0.126)	(0.134)	(0.134)
Direct YC recipient	−0.178	−0.179	−0.256*	−0.234	−0.294**	−0.298*	−0.153	−0.176
	(0.095)	(0.106)	(0.124)	(0.129)	(0.095)	(0.127)	(0.124)	(0.131)
Ethnicity: Peul		−0.129		−0.072		−0.108		−0.101
		(0.071)		(0.047)		(0.072)		(0.068)
Ethnicity: Tuareg		−0.079		0.031		−0.287***		0.026
		(0.098)		(0.060)		(0.083)		(0.078)
Ethnicity: Zarma		−0.045		−0.025		−0.208*		0.056
		(0.087)		(0.069)		(0.086)		(0.060)
Autochthone	−0.027	−0.054	0.093	0.065	−0.073	−0.067	−0.233*	−0.228*
	(0.205)	(0.195)	(0.099)	(0.089)	(0.048)	(0.057)	(0.100)	(0.090)
Age		−0.054		−0.047		−0.045		0.027
		(0.040)		(0.028)		(0.034)		(0.042)
Age squared		0.001		0.001		0.001		−0.001
		(0.001)		(0.001)		(0.001)		(0.001)
Female		−0.092*		−0.078*		0.008		−0.009
		(0.041)		(0.039)		(0.047)		(0.036)
ACLED events		−0.002		−0.003*		−0.005**		−0.003
		(0.002)		(0.001)		(0.001)		(0.002)
ACLED deaths		−0.001		0.002		−0.002		0.000
		(0.001)		(0.001)		(0.002)		(0.001)
N. of YC beneficiaries		0.000		0.001		−0.001		0.000
		(0.001)		(0.001)		(0.001)		(0.001)
Distance to a river		0.006		−0.010**		−0.003		−0.003
		(0.005)		(0.004)		(0.006)		(0.007)
Distance to the border		−0.002		−0.001*		−0.002		−0.001
		(0.001)		(0.001)		(0.001)		(0.001)
Commune seat		−0.056		0.080		0.081		−0.002
		(0.051)		(0.043)		(0.059)		(0.066)
Region Fixed Effects	X	X	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X	X	X
Num.Obs.	1721	1719	1717	1715	1719	1717	1726	1724
R2	0.497	0.508	0.490	0.503	0.521	0.544	0.398	0.407

Note: This table uses data from the YC midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following... ' The reference level is villages that received Youth Connect only. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

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Table A9. Heterogenous treatment effects for direct questions by ethnicity

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control village	0.008	−0.087	−0.043	−0.105	0.083	−0.027	0.069	0.034
	(0.123)	(0.121)	(0.080)	(0.074)	(0.116)	(0.120)	(0.113)	(0.119)
Treatment village	−0.126*	−0.156*	−0.104*	−0.156***	−0.171**	−0.223***	−0.182**	−0.207**
	(0.059)	(0.061)	(0.048)	(0.042)	(0.054)	(0.060)	(0.065)	(0.063)
Treatment village * Peul	−0.003	−0.008	0.199*	0.207*	0.046	0.051	0.122	0.131
	(0.195)	(0.189)	(0.091)	(0.095)	(0.154)	(0.143)	(0.192)	(0.183)
Pure control village * Peul	−0.184	−0.046	0.174+	0.205+	−0.223	−0.092	−0.296	−0.255
	(0.207)	(0.190)	(0.100)	(0.110)	(0.170)	(0.155)	(0.197)	(0.195)
Treatment village * Touareg	0.165	0.175	0.037	0.094	−0.007	0.085	0.418**	0.446***
	(0.160)	(0.167)	(0.141)	(0.126)	(0.216)	(0.204)	(0.130)	(0.128)
Pure control village * Touareg	−0.322+	−0.246	0.094	0.233+	−0.343	−0.200	0.055	0.154
	(0.191)	(0.211)	(0.143)	(0.139)	(0.216)	(0.217)	(0.136)	(0.145)
Treatment village * Zarma	0.329**	0.438***	0.189	0.266*	0.197+	0.339***	0.130	0.226+
	(0.125)	(0.125)	(0.119)	(0.124)	(0.106)	(0.098)	(0.140)	(0.131)
Pure control village * Zarma	0.227	0.383*	0.264	0.306+	−0.088	0.041	0.034	0.118
	(0.177)	(0.179)	(0.160)	(0.161)	(0.181)	(0.189)	(0.156)	(0.173)
Direct YC recipient	−0.161+	−0.150	−0.230+	−0.226+	−0.324**	−0.309*	−0.143	−0.158
	(0.088)	(0.101)	(0.124)	(0.130)	(0.103)	(0.131)	(0.121)	(0.130)
Ethnicity: Peul	−0.024	−0.110	−0.198**	−0.189*	−0.066	−0.118	−0.001	−0.008
	(0.164)	(0.150)	(0.060)	(0.074)	(0.116)	(0.105)	(0.163)	(0.159)
Ethnicity: Tuareg	−0.090	−0.087	0.012	−0.029	−0.258+	−0.288*	−0.192**	−0.203**
	(0.117)	(0.127)	(0.070)	(0.061)	(0.132)	(0.142)	(0.066)	(0.071)
Ethnicity: Zarma	−0.263*	−0.321*	−0.167	−0.178+	−0.290**	−0.351***	−0.024	−0.045
	(0.117)	(0.124)	(0.109)	(0.107)	(0.092)	(0.093)	(0.110)	(0.108)
Autochthone		−0.122		0.012		−0.037		−0.085
		(0.097)		(0.051)		(0.058)		(0.074)
Age		−0.052		−0.046		−0.040		0.033
		(0.039)		(0.028)		(0.033)		(0.042)
Age squared		0.001		0.001		0.001		−0.001
		(0.001)		(0.001)		(0.001)		(0.001)
Female		−0.093*		−0.081*		0.010		−0.012
		(0.039)		(0.039)		(0.046)		(0.037)
ACLED events		−0.003*		−0.003*		−0.006***		−0.004+
		(0.002)		(0.001)		(0.002)		(0.002)
ACLED deaths		−0.001		0.002		0.000		0.001
		(0.001)		(0.001)		(0.002)		(0.001)
N. of YC beneficiaries		−0.001		0.000		−0.001+		0.000
		(0.001)		(0.001)		(0.001)		(0.001)
Distance to a river		0.005		−0.012**		−0.004		−0.006
		(0.005)		(0.004)		(0.006)		(0.007)
Distance to the border		−0.002+		−0.002**		−0.002		−0.001
		(0.001)		(0.001)		(0.001)		(0.001)
Commune seat		−0.089+		0.073+		0.055		−0.010
		(0.051)		(0.044)		(0.059)		(0.063)
Region Fixed Effects	X	X	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X	X	X
Num.Obs.	1721	1719	1717	1715	1719	1717	1726	1724
R2	0.506	0.519	0.495	0.509	0.531	0.547	0.404	0.413

Note: This table uses data from the YC midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following...' The reference level is villages that received Youth Connect only. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

All responses were measured using a five point likert scale. Table A10 shows that that treatment does not vary based on these intermediate outcomes. Among respondents who feel that they are able to participate in community decision making, IBNM treatment had neither greater nor lesser effects on perceived support for violence. However, these intermediate responses are also measured post-treatment, which opens a space for potential bias in capturing the effect of these moderators.

Figure A3. Geospatial concentration of IBNM Treatment effects
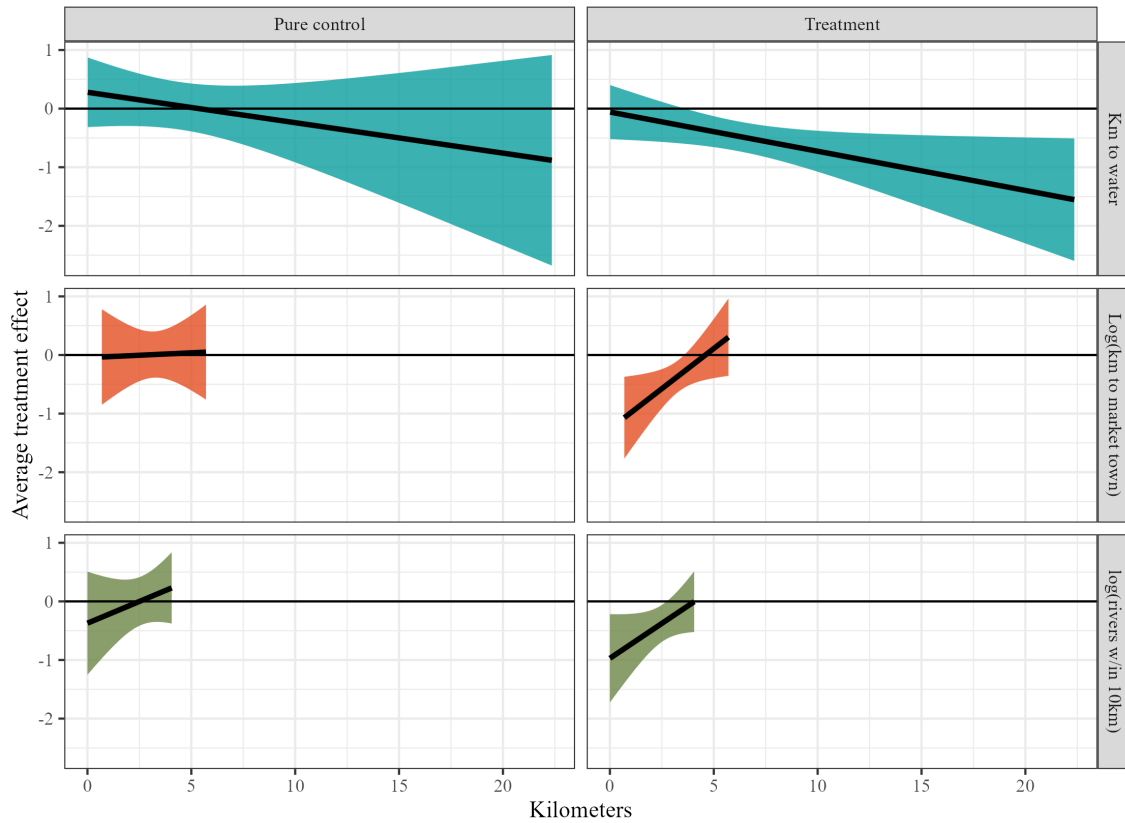
This figure displays the marginal effects for IBNM treatment, by the distance between a village and a flowing source of water, the (log of) the respondents' self-reported distance to a market town, and the (log of) the total length of rivers within a 10 kilometer radius. Rivers are defined as permanent flows of at least one cubic meter of water per day. Regressions are estimated using OLS with region and enumerator fixed effects and with within sample survey weights. Standard errors are clustered at the village level.

We also pre-registered a non-parametric, machine-learning based strategy to uncover sources of treatment effect heterogeneity within the data. Specifically, we estimate conditional average

Table A10. Treatment effects are homogenous across intermediate outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Pure control village	−0.243	−0.396	−0.751	−0.487	−0.553	−0.264
	(0.303)	(0.524)	(0.474)	(0.474)	(0.697)	(0.408)
Treatment village	−0.341	−0.833*	−0.989*	−0.691+	−1.011*	−0.368
	(0.229)	(0.401)	(0.389)	(0.377)	(0.436)	(0.321)
Treatment village * opportunities		0.148				
		(0.119)				
Pure control village * opportunities		0.161				
		(0.167)				
Treatment village * heard			0.190+			
			(0.109)			
Pure control village * heard			0.271+			
			(0.139)			
Treatment village * heard by authority				0.118		
				(0.126)		
Pure control village * heard by authority				0.210		
				(0.187)		
Treatment village * active					0.171	
					(0.115)	
Pure control village * active					0.168	
					(0.176)	
Treatment village * affect						0.012
						(0.107)
Pure control village * affect						0.094
						(0.152)
Direct YC recipient	−1.020*	−0.923*	−0.924*	−0.954*	−0.947*	−0.923*
	(0.470)	(0.440)	(0.422)	(0.435)	(0.438)	(0.424)
Ethnicity: Peul	−0.455*	−0.494*	−0.504**	−0.434*	−0.473*	−0.493*
	(0.189)	(0.194)	(0.191)	(0.193)	(0.202)	(0.202)
Ethnicity: Tuareg	−0.289+	−0.310+	−0.301+	−0.259	−0.302+	−0.306
	(0.170)	(0.175)	(0.172)	(0.172)	(0.177)	(0.185)
Ethnicity: Zarma	−0.258	−0.218	−0.218	−0.212	−0.228	−0.241
	(0.175)	(0.171)	(0.174)	(0.187)	(0.176)	(0.178)
Autochthone	−0.184	−0.125	−0.184	−0.174	−0.172	−0.147
	(0.167)	(0.171)	(0.164)	(0.172)	(0.167)	(0.169)
Age	−0.098	−0.072	−0.074	−0.083	−0.103	−0.040
	(0.089)	(0.087)	(0.088)	(0.090)	(0.086)	(0.083)
Age squared	0.002	0.001	0.001	0.001	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Female	−0.173	−0.197	−0.211+	−0.200+	−0.212+	−0.168
	(0.111)	(0.119)	(0.116)	(0.118)	(0.124)	(0.117)
ACLED events	−0.013**	−0.015**	−0.014**	−0.014**	−0.013*	−0.012*
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
ACLED deaths	0.001	0.000	−0.001	−0.001	0.000	−0.001
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)
N. of YC beneficiaries	0.000	0.000	−0.001	−0.001	−0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Distance to a river	−0.013	−0.017	−0.014	−0.014	−0.015	−0.011
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Distance to the border	−0.006*	−0.007*	−0.007*	−0.007*	−0.007*	−0.007*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Commune seat	0.081	0.085	0.079	0.111	0.068	0.019
	(0.150)	(0.154)	(0.151)	(0.150)	(0.155)	(0.154)
Region Fixed Effects	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X
Num.Obs.	1732	1725	1721	1707	1691	1621
R2	0.592	0.592	0.596	0.595	0.594	0.606

Note: This table uses data from the YC midline survey. Outcome variables are responses to '[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following... ' The reference level is villages that received Youth Connect only. Demographic controls include ethnicity, autochthony, age, age squared, wealth, and sex; geographic controls include the count of all ACLED events within 25 km of the village, the sum of ACLED deaths in the village, the count of total YC beneficiaries per village, the distance to permanent water, distance to the nearest international border, and a commune center indicator. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

treatment effects (CATEs) using a data-driven approach to select variables, which means our pre-analysis plan did not enumerate the specific moderators we explore here. The overall estimand for the experiment LATE, τ, which estimates $\tau = \mathbb{E}[Y^{(1)} - Y^{(0)}]$ where τ denotes the estimand, $Y^{(1)}$ denotes the outcome given assignment into treatment, and $Y^{(0)}$ denotes the outcome given assignment into the control group. In contrast the CATE estimates a individual level effect τ_i defined as $\tau(x_i) = \mathbb{E}[Y^{(1)} - Y^{(0)}|X = x_i]$, where x_i denotes a vector of control variables.

Following Athey and Wager (2018), we estimate CATEs using a series of causal forests. We then calculate variable importance for control variable v as the number of times that the forest split on v, weighted by the depth of the split. This measure captures the relative importance of v in explaining treatment effect heterogeneity. We identified three variables which explained the greatest fraction of treatment effect heterogeneity through this approach: the distance between a village and a flowing source of water, the (log of) the respondents' self-reported distance to a market town, and the (log of) the total length of rivers within a 10 kilometer radius.[26] We then re-estimate a series of OLS regressions in which we interact the variables indicated as important in the causal forests with the IBNM treatment indicator and the pure control group indicator.[27] Figure A3 shows the marginal effect of Youth Connect and IBNM training by these non-parametrically selected variables. The marginal effect of being in the pure control group relative to being in a village which received only Youth Connect is at no point statistically distinguishable from zero. Absent supplemental IBNM treatment, Youth Connect by itself did not impact support for political violence at any levels of these moderating variables. In contrast, figure A3 shows that adding IBNM training to Youth Connect was more effective in reducing support for violence among respondents who were further from water, who were closer to market towns, and who had greater availability of water within 10 kilometers of their village.

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[26]We define rivers as a permanent water source with more than a cubic meter of flow per year.

[27]We consider this process as analogous to using a post-LASSO procedure, in which researchers use a LASSO to select control variables, and then include these variables in a non-shrinkage adjusted OLS regression (Belloni and Chernozhukov 2013). In our case, we use causal forests to identify drivers of heterogeneous treatment effects, and include these variables in a standard OLS regression as interaction variables.

B   Economic effects of Youth Connect

It is worth noting that elsewhere in this paper we are skeptical of comparisons between the pure control group and the two groups treated by Youth Connect (Youth Connect only and Youth Connect + IBNM). This is because a separate pool of enumerators surveyed the pure control villages, and so it is impossible to verify that enumerator fixed effects absorb all enumerator errors. However, as appendix A.1 shows, enumerator effects are more prevalent for sensitive questions, such as those on the subject of violence, rather than questions about asset ownership and employment. As such, we feel reasonably confident that tables B1 and B2 do not suffer from error induced by different survey enumerators.

This section shows the effect of Youth Connect on a variety of economic outcomes. Table B1 shows the effect of Youth Connect on a wealth index and on self-reported employment. Table B2 shows the effect of Youth Connect on self reported access to opportunity and training. Across all economic outcomes, there is a negative and statistically significant coefficient on the indicator for the pure control group. This means that economic outcomes are worse in villages which did not receive Youth Connect—an important confirmation of the program's effectiveness. There is no difference within Youth Connect between households that did and did not receive IBNM, which makes sense because IBNM training had no economic components.

Table B1. Wealth and employment are higher in villages treated with Youth Connect

	Wealth index		Employment	
	(1)	(2)	(3)	(4)
Youth Connect + IBNM	−0.229	−0.188	0.000	−0.012
	(0.222)	(0.134)	(0.031)	(0.029)
Pure control	−0.723**	−0.659**	−0.200***	−0.142**
	(0.271)	(0.242)	(0.056)	(0.052)
Direct YC recipient	0.245	0.287	−0.482***	−0.488***
	(0.241)	(0.202)	(0.062)	(0.067)
Ethnicity: Peul		−0.105		−0.160***
		(0.144)		(0.047)
Ethnicity: Tuareg		−0.627***		−0.075
		(0.178)		(0.051)
Ethnicity: Zarma		−0.135		−0.142**
		(0.212)		(0.043)
Autochthone		−0.270		0.006
		(0.206)		(0.041)
Age		−0.065		−0.023
		(0.078)		(0.024)
Age squared		0.001		0.001
		(0.002)		(0.000)
Female		−0.043		−0.154***
		(0.094)		(0.028)
ACLED events		−0.008+		−0.001
		(0.004)		(0.001)
ACLED deaths		0.002		−0.002
		(0.003)		(0.001)
N. of YC beneficiaries		0.001		0.000
		(0.001)		(0.000)
Distance to a river		0.006		−0.001
		(0.011)		(0.002)
Distance to the border		0.000		−0.001+
		(0.002)		(0.000)
Commune seat		0.759***		0.078**
		(0.117)		(0.029)
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Mean of outcome	0.974	0.974	0.298	0.298
Num.Obs.	1734	1732	1734	1732
R2	0.135	0.239	0.243	0.307

Note: Data are from the Youth Connect midline survey. Outcome variables are an index of household wealth and whether a respondent reports being employed. The independent variable is treatment status; the reference level is villages that received Youth Connect only. ACLED data are calculated using a 25 kilometer radius from the village centroid. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

Table B2. Youth Connect villages have greater access to training

	Technical training		Life skills training		Perceived opportunity	
	(1)	(2)	(3)	(4)	(5)	(6)
Youth Connect + IBNM	−0.032	−0.041	−0.028	−0.047+	−0.050	−0.058
	(0.032)	(0.030)	(0.033)	(0.028)	(0.083)	(0.078)
Pure control	−0.190***	−0.136*	−0.104**	−0.086*	−0.417*	−0.509**
	(0.047)	(0.054)	(0.033)	(0.035)	(0.163)	(0.175)
Direct YC recipient	0.285**	0.267**	0.556***	0.532***	0.222	0.266
	(0.090)	(0.093)	(0.076)	(0.077)	(0.220)	(0.214)
Ethnicity: Peul		−0.070		0.003		−0.279*
		(0.043)		(0.032)		(0.138)
Ethnicity: Tuareg		−0.089+		0.100*		−0.126
		(0.045)		(0.043)		(0.137)
Ethnicity: Zarma		−0.071		0.099*		−0.165
		(0.043)		(0.040)		(0.119)
Autochthone		0.057		0.068*		−0.249+
		(0.035)		(0.031)		(0.128)
Age		0.053*		0.027+		0.160*
		(0.025)		(0.015)		(0.073)
Age squared		−0.001+		0.000		−0.003*
		(0.001)		(0.000)		(0.002)
Female		−0.027		0.015		0.121+
		(0.022)		(0.024)		(0.072)
ACLED events		−0.002*		0.001		−0.005*
		(0.001)		(0.001)		(0.002)
ACLED deaths		0.001		0.000		0.007*
		(0.001)		(0.001)		(0.003)
N. of YC beneficiaries		0.001**		0.000		−0.001
		(0.000)		(0.000)		(0.001)
Distance to a river		−0.005*		−0.007*		−0.009
		(0.002)		(0.003)		(0.008)
Distance to the border		0.000		0.000		−0.001
		(0.000)		(0.000)		(0.002)
Commune seat		0.062*		0.026		0.159+
		(0.029)		(0.025)		(0.082)
Region Fixed Effects	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X
Mean of outcome	0.369	0.369	0.325	0.325	3.755	3.755
Num.Obs.	1733	1731	1733	1731	1729	1727
R2	0.307	0.329	0.465	0.479	0.319	0.350

Note: Data are from the Youth Connect midline survey. Outcome variables are: responses to (1) Have you received technical training; (2) Have you ever received training in life skills, such as management skills, leadership skills, social skills, or civic engagement; and (3) To what extent do you agree or disagree with the following statement: I have access to trainings and educational opportunities to learn new skills. The independent variable is treatment status; the reference level is villages that received Youth Connect only. ACLED data are calculated using a 25 kilometer radius from the village centroid. All regressions use OLS with within-sample inverse probability weights; standard errors are clustered at the village level.

Table C1. Overlap in sampled villages between baseline and endline

	Treated	Control	Pure control
Baseline only	0	1	0
Both	11	21	14
Midline only	30	14	22

Note: 118 villages were sampled at endline; of the 120 originally included, two villages were no longer accessible to enumerators because of insecurity.

C   Difference-in-differences with baseline data

We also implemented a baseline survey in advance of both Youth Connect and IBNM training. Table C1 shows the overlap between villages in which the baseline and midline took place. Because we sampled more youth per village in the baseline (in fewer villages), we have significantly more observations at baseline than at midline in this limited overlap. The baseline survey was implemented in 48 villages, of which 47 were surveyed again at midline. These 47 overlapping villages yield a repeated cross section with 1,186 observations at baseline and 416 observations at midline. This discrepancy is largely due to the baseline survey enumerating a similar number of individuals within a smaller set of villages. We also restrict the midline sample to the households recruited via a random walk to ensure the two samples are comparable. Our within-sample survey weights account for this difference, as we include the probability of a household being sampled.

Tables C2 and C3 show the results of estimating a difference-in-differences design using a two-way fixed effects specification. For each of the four 'justified' questions, as well as an additive index of all four questions, we estimate the TWFE model with and without control variables. One important note is that the structure of the data preclude me from adding enumerator fixed effects. Because each period had distinct enumerators, enumerator fixed effects are collinear with the period fixed effects. However, all regressions do include controls for each enumerator's median duration, which I take as a proxy measure for enumerator quality. I also include each enumerators 10th and 90th percentile of duration, to capture both low and high areas of enumerator quality. However, these measures cannot capture other enumerator effects, like gender or shared ethnicity.

Each of these specifications uses villages which received only Youth Connect treatment as the reference level. In other words, these models show the extent to which support for violence increased or decreased in these villages since baseline, compared to the extent to which support

Table C2. Difference in difference coefficients for 'retaliate against violence,' 'defend one's ethnic group,' and 'defend one's religion'

	Retaliate against violence		Defend ethnic group		Defend religion	
	(1)	(2)	(3)	(4)	(5)	(6)
D-in-D: YC + IBNM	0.275	0.278	0.072	0.104	0.027	0.065
	(0.190)	(0.191)	(0.179)	(0.179)	(0.243)	(0.226)
D-in-D: Pure control	0.436*	0.437*	0.490*	0.456*	0.346	0.333
	(0.208)	(0.206)	(0.212)	(0.221)	(0.280)	(0.270)
Ethnicity: Peul		0.056		0.087		−0.146
		(0.100)		(0.148)		(0.200)
Ethnicity: Tuareg		0.061		−0.183		−0.483**
		(0.085)		(0.182)		(0.164)
Ethnicity: Zarma		0.144+		0.068		−0.374*
		(0.083)		(0.221)		(0.145)
Age		0.040		−0.003		0.003
		(0.041)		(0.068)		(0.053)
Age squared		−0.001		0.000		0.000
		(0.001)		(0.002)		(0.001)
Wealth index		−0.016		0.000		−0.023
		(0.026)		(0.036)		(0.033)
Female		0.017		0.129		0.139*
		(0.063)		(0.091)		(0.059)
Enumerator: 10th percentile	0.029**	0.030**	0.026**	0.026**	0.072***	0.072***
	(0.011)	(0.011)	(0.009)	(0.009)	(0.014)	(0.014)
Enumerator: 50th percentile	0.002	0.002	−0.006	−0.006	−0.040**	−0.040**
	(0.004)	(0.004)	(0.008)	(0.009)	(0.011)	(0.012)
Enumerator: 90th percentile	0.000	0.000	0.000	0.000+	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Village + wave FEs	X	X	X	X	X	X
Num.Obs.	1598	1597	1595	1594	1580	1579
R2	0.173	0.178	0.119	0.129	0.219	0.239

Note: This table reports the TWFE difference-in-difference coefficient using YC baseline and mid-line data in a repeated cross-section. The outcome variables of this regression are responses to a five point likert scale. All regressions use OLS and within-sample inverse probability weights. Standard errors are clustered at the village level.

Table C3. Difference in difference coefficients for 'force the government to chance its policies' and an overall index

	Defend one's religion		Index	
	(1)	(2)	(3)	(4)
D-in-D: YC + IBNM	0.283+	0.324*	0.813	0.927+
	(0.159)	(0.140)	(0.505)	(0.493)
D-in-D: Pure control	0.374*	0.329*	1.634*	1.560*
	(0.164)	(0.150)	(0.615)	(0.627)
Ethnicity: Peul		−0.063		−0.073
		(0.165)		(0.340)
Ethnicity: Tuareg		−0.087		−0.678*
		(0.150)		(0.274)
Ethnicity: Zarma		−0.072		−0.283
		(0.145)		(0.301)
Age		−0.026		0.025
		(0.065)		(0.126)
Age squared		0.000		−0.001
		(0.001)		(0.003)
Wealth index		−0.055+		−0.090
		(0.028)		(0.078)
Female		0.125		0.457*
		(0.079)		(0.195)
Enumerator: 10th percentile	0.015+	0.016*	0.146***	0.148***
	(0.008)	(0.007)	(0.024)	(0.025)
Enumerator: 50th percentile	−0.025***	−0.025***	−0.070**	−0.072**
	(0.006)	(0.005)	(0.023)	(0.023)
Enumerator: 90th percentile	0.000	0.000	0.001	0.001+
	(0.000)	(0.000)	(0.000)	(0.000)
Village + wave FEs	X	X	X	X
Num.Obs.	1595	1594	1602	1601
R2	0.145	0.171	0.149	0.166

Note: This table reports the TWFE difference-in-difference coefficient using YC baseline and midline data in a repeated cross-section. The outcome variables of this regression are responses to a five point likert scale. All regressions use OLS and within-sample inverse probability weights. Standard errors are clustered at the village level.

for violence increased or decreased in the Youth-Connect only group. These results show minimal differences between treatment and control groups, but that the pure control had a statistically significant increase in support for violence, relative to the control groups. These data weakly support the conclusion that support for violence grew in the absence of Youth Connect/IBNM trainings. However, these regressions are underpowered and we do not include them in the main paper.

D Pre-post questionnaire of IBNM participants

In addition to the midline survey, we administered a short questionnaire to the 183 direct participants in IBNM training immediately before and after the training. Table D1 shows the differences in five questions between the survey administered before the training and the survey administered after the training. All five questions were posed as "to what extent do you agree with the following statement," with answer options as five point likert scales ranging from strongly agree (5) to strongly disagree (1). Results are from an OLS regression of an indicator for post-treatment on the outcome variables, with respondent fixed effects. In other words, these coefficients measure the average change from pre-treatment to post-treatment.

Table D1 shows sizable increases between pre and post treatment on a variety of indicators. It shows no change in agreement that "sometimes, violence is the only way to change things." However, this lack of change is largely a function of floor effects: respondents were already unlikely to agree with this statement, so there was minimal movement possible after treatment.[28] These results may also be the result of social desirability bias, so we do not strongly rely on them.

Table D1. After the training, IBNM direct participants are more confidence they can influence their community and resolve conflicts.

Statement	Coefficient	Std.Err.	P-value	Baseline mean
I can influence decisions taken during community meetings.	1.04	0.12	0.00	3.82
I have the capacity and the abilities to make community leaders see my point of view.	1.04	0.10	0.00	3.84
Sometimes, violence is the only way to change things.	-0.08	0.12	0.95	1.43
I am confident in my capacity to resolve conflicts between different villages.	1.37	0.12	0.00	3.60
I am confident in my ability to resolve conflicts for myself.	1.00	0.09	0.00	4.15

Note: Data are from a survey administered to 183 IBNM participants; respondents were asked "to what extent do you agree with the following statements." Answers were on a five point likert scale. Surveys were administered to IBN participants immediately before and after the training. All regressions use respondent fixed effects; standard errors are clustered by respondent. Coefficients are for a binary indicator that a survey was in the second wave.

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[28]These floor effects are another reason to prefer the various measurement strategies like indirect questions or list experiments adopted in the midline survey over a simpler measure.

The broader IBNM randomized control trial followed a training of trainers model. A select few respondents, titled "young leaders," participated in these direct and regionally managed IBNM trainings. They then disseminated these results to the remainder of their communities through village assemblies. In that sense, these results serve as a "first-stage" results: they show that participants did indeed take-up the lessons of IBNM. This result may not have broad implications by itself, but it is an important confirmatory test for the remainder of the project. If youth leaders had not absorbed the lessons of IBNM, it would be cause for concern.

E   ACLED Difference-in-Difference: robustness

Figures E1 and E2 show that the difference-in-differences design using ACLED data is robust to alternative specifications. Both figures manipulate the windows during which the pre and post periods are calculated. Figure E1 restricts to ACLED events within 10 kilometers of village centroids; figure E2 maintains the 25 kilometer radii as in the main text. Incidents where the government is the primary actor and incidents where local militia are the primary actors drop out of figure E1 because of the very small number of observations.

A variety of critiques exist of ACLED and similar datasets—their coverage is uneven, and they can also exclude a variety of conflict events Demarest and Langer 2018; Eck 2012. However, for these data issues to negate our analysis, it's not enough for ACLED to be missing events, for this to bias our results, the missingness of events would have to be systematically different between IBNM and Youth Connect villages and villages which received only Youth Connect. We found no reason to suggest that such a bias exists.

Figure E1. Difference-in-difference coefficients are robust measuring ACLED events within 10 kilometer radii
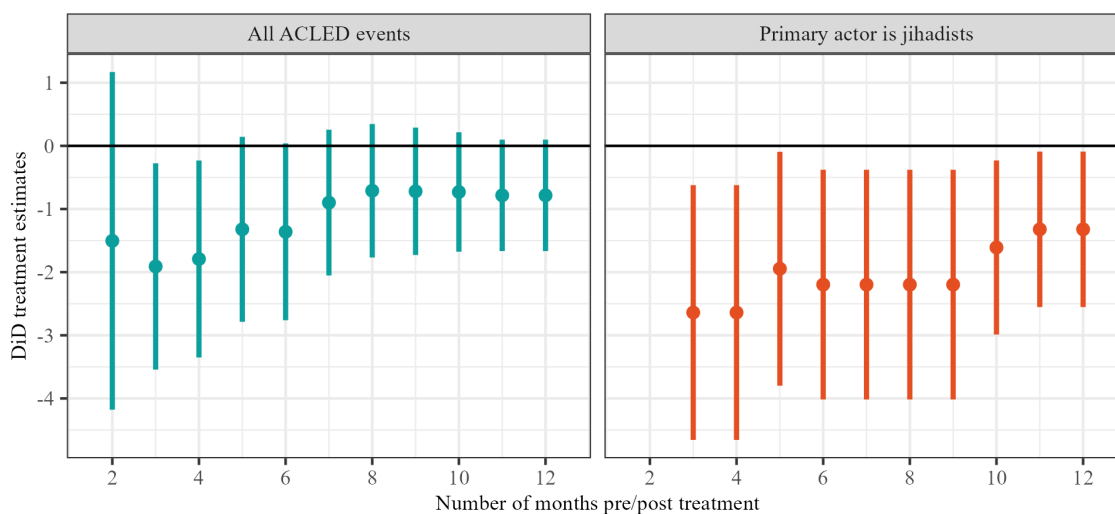
This figure shows how DiD coefficients from a two-way fixed-effects (village and period) Poisson regression change based on how the window of treatment is calculated. For example, when the number of months pre/post treatment is two, the pre-treatment period includes November and December of 2022 and the post-treatment period includes January and February of 2023. The count of ACLED incidents is calculated using 10 kilometer radii around each village center. Each point represents the coefficient of interest from a difference-in-differences regression, and the vertical lines represent confidence intervals.

Figure E2. Difference-in-difference coefficients are robust to moving the window of months in which ACLED incidents are measured
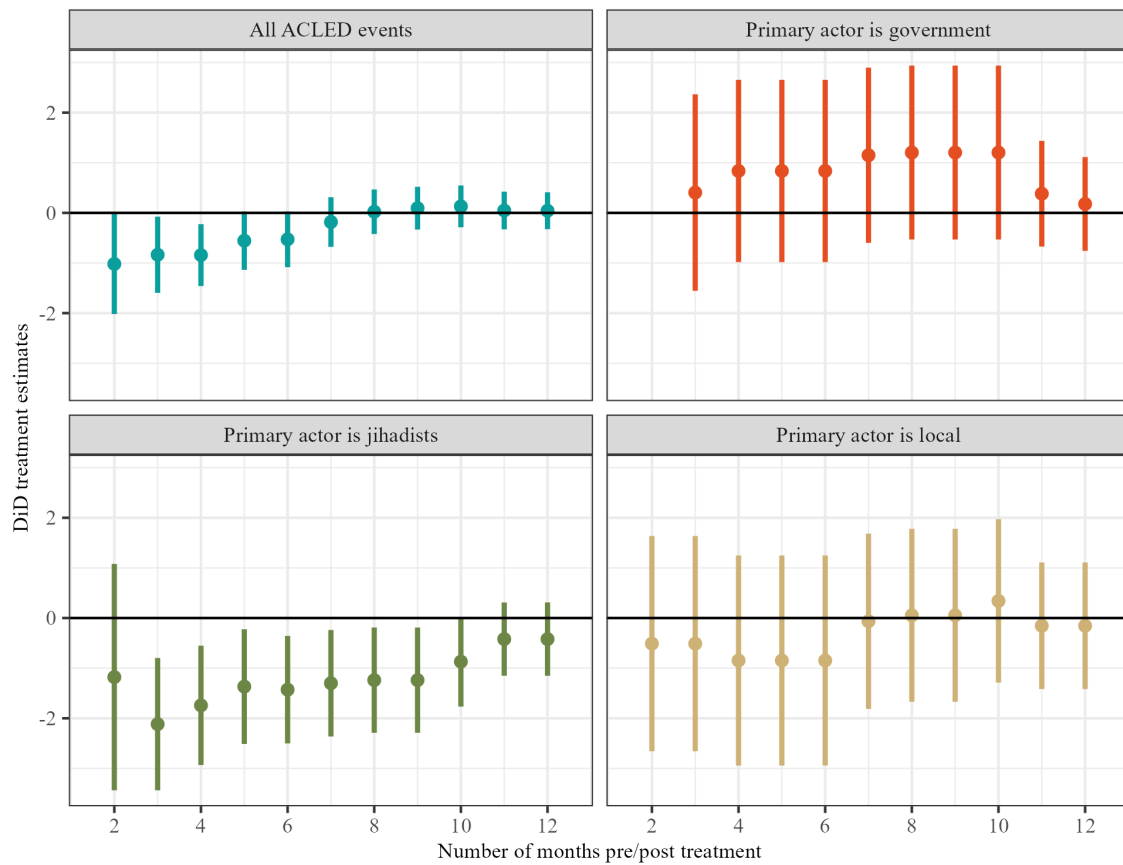
This figure shows how DiD coefficients from a two-way fixed-effects (village and period) Poisson regression change based on how the window of treatment is calculated. For example, when the number of months pre/post treatment is two, the pre-treatment period includes November and December of 2022 and the post-treatment period includes January and February of 2023. The count of ACLED incidents is calculated using 25 kilometer radii around each village center. Each point represents the coefficient of interest from a difference-in-differences regression, and the vertical lines represent confidence intervals.

F   Ethics and Informed Consent

Two separate institutional review boards (IRBs) reviewed this research. The Stanford University IRB reviewed it under protocol number IRB-63802. The Nigerien national ethics committee (Comité Nationale d'Ethique de Niger), which serves as an IRB for all research undertaken in Niger, reviewed and approved this research under deliberation number 23/2024/CNERS. The baseline survey, which we leverage in appendix C was separately reviewed by the Comité d'Ethique, under protocol number 071/2021/CNERS.

We administered a two step consent/assent procedure for this research. After a household was selected, we first asked the household head (or their spouse) for consent to administer the survey instrument to one member of their household between the age of 15 and 35. We then asked the household head for the names of all members of the household that fit these criteria, and randomly selected a survey participant from that list. This randomization was performed by the survey software (ONA). The survey software also randomly selected a back-up respondent if the first was then unavailable. We then separately administered a consent statement—technically an assent statement for respondents between 15 and 17—to the survey participant. Consent scripts were available in French Haoussa, and Zarma.

There are three important ethical considerations to this research. The first potential ethical concern regards the ethics of withholding treatment from households. Particularly in fragile contexts like Niger, withholding a potentially advantageous treatment for purely research reasons is ethically difficult to justify. In the case of IBNM training however, we follow a randomized rollout design: villages that did not receive IBNM before the (midline) follow-up survey will receive it after. The sequencing of receiving IBNM treatment was due to Mercy Corps's capacity to implement these trainings—villages would have been delayed in receiving IBNM treatment anyways.

The second ethical consideration to this research has to do with the age of respondents: 15 to 35. Of the 1,734 participants, 352 were between the ages of 15 and 17. This Youth Connect program was specifically targeted towards youth, who are a marginalized community within village politics of Niger. To ensure that all participants were able to give informed consent, we administered consent statements to both households heads, who were consistently capable of giving legal consent, as well as to the participants—some of whom were able to assent, rather than consent. We do not believe this research posed a particular risk to participants under the age of 18.

Finally, it is important to note that this research takes place in extremely vulnerable settings. Both the Maradi and Tillabéri regions of Niger suffer from instability and incursions by violent

non-state actors. Our goal with the design of the research questionnaire was to ensure that youth never answered direct questions about their own support for violence, so that any potential data leaks would not implicate individual research participants. To this end, we use two broad sets of measures to capture support for violence:

1. A series of questions which asked "[d]o people in your community think it is sometimes, rarely, or never justified to use violence to do each of the following: retaliate against violence, defend one's ethnic group, defend one's religion, and force the government to change their policies." This question asks respondents about general support within their community, rather than their own support.

2. A series of list experiments, which all follow a similar strategy in asking respondents "how many items do you agree with/how many actions would you consider taking." In each case, we explicitly ask respondents not to tell the enumerator which items they agree with specifically, and in any case do not record any additional details the respondent provides.

Ultimately, the sensitive questions of the survey are the ones which concern violence—through these measurement strategies, we ensure youth never directly state their own perceptions of or support for violence. In so doing, we give an element of "plausible deniability" to any participants.

We took two additional steps to minimize risk to participants. First, all survey instruments were designed in extensive consultation with Mercy Corps' Niger and Burkina Faso country offices, who implemented both Youth Connect and the IBNM program. In addition to deep country knowledge, these offices have experiences with Monitoring and Evaluation within Niger and Burkina Faso, which poses many similar challenges as field surveys for research purposes. Every question on our field survey was reviewed and approved by these country offices, to ensure both respondents and enumerators were comfortable with the language.

Finally, the field team liaised extensively with regional authorities in both the Maradi and Tillabéri regions. In Tillabéri, where the security situation is especially fragile, Mercy Corps' field staff informed local officials such as prefects, sous-prefects, and gendarmerie officers of their movements in advance to ensure no security guidelines were violated and enumerators were safe.

G   Appendix References

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