

Getting to Youths: Development Programming, Conflict Resolution, and Violence in Niger ^{*}

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Abstract

Can development interventions build peace where conflict resolution institutions are weak or absent? This paper presents a cluster-randomized control trial in the Maradi and Tillabéri regions of Niger, embedded within a youth vocational training and civic engagement program. Villages received either (1) Youth Connect, a standard package of vocational training, civic engagement, and entrepreneurship support; or (2) Youth Connect plus training in Interest-Based Mediation and Negotiation (IBMN). In a follow-up survey of 1,342 youth across 82 villages, those who received the standard package plus IBMN are less likely to support violence than youth who only participated in the standard package, a year after IBMN trainings took place. A difference-in-differences approach using geo-referenced conflict incidents shows that villages which received IBMN in addition to Youth Connect experienced fewer violent events. This research advances scholarship on peacebuilding in fragile states and illustrates the relationship between attitudes towards violence and violence itself.

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Can development interventions build peace where conflict resolution institutions are weak or absent? Where neither the state nor conflict resolution institutions are accessible, addressing individual-levels drivers of violence may not suffice to reduce levels of violence. A plethora of recent interventions by governments, NGOs, and donor organizations have attempted to reduce participation in violence among youth (Bhatt et al. 2024; Blattman and Annan 2016; Pruett et al. 2024). However, the results of such interventions are mixed and few studies attempt to examine how such programs might be enhanced by building local capacity to resolve disputes, particularly in already fragile settings (Dasgupta, Gawande, and Kapur 2017; Premand and Rohner 2024; Sexton and Zürcher 2024). 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 paper answers that question with a randomized controlled trial (RCT) in the Maradi and Tillabéri regions of Niger. We provide causal evidence that training youth in conflict management skills reduces support for violence above and beyond a standard intervention package of economic opportunities and civic engagement activities. Our research took place within a broader USAID-funded program, titled Youth Connect, helped youth enhance their livelihoods through a mixture of vocational training, economic inputs, and civic engagement training. By improving youth’s economic circumstances, Youth Connect aimed to reduce support for violence. Our intervention supplemented Youth Connect with additional training in conflict mediation. Within the 84 villages which participated in Youth Connect, we randomly selected half to also participate in Interest Based Mediation and Negotiation (IBMN) training. IBMN is a conflict management strategy in which participants identify the goals that parties bring to negotiations and try to find win-win scenarios. Youth leaders trained in regional capitals and disseminated their experiences to other young people in their home villages. To capture outcomes, we surveyed 1,342 youth in 41 villages which received only Youth Connect and 41 villages which received Youth Connect alongside the IBMN training.¹

This study expands a growing literature on building peace in fragile contexts by evaluating IBMN, 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).² Previous research deployed

¹We were unable to conduct the follow-up survey in one Youth Connect plus IBMN village and one Youth Connect only village. Appendix B shows results from an additional 36 villages which serve as a non-random pure control group.

²Because of the 2025 withdrawal of US foreign assistance, we cannot identify the precise cost of the IBMN trainings, separate from the broader Youth Connect program. The main costs for IBMN included curriculum development, initial field agent trainings, the training of youth leaders in Maradi and Tillabéri, supplies for the village-level trainings, and some percentage of general management overhead and monitoring and evaluation costs.

IBMN as a stand-alone peacebuilding intervention (Reardon, Wolfe, and Ogbudo 2021); we investigate whether IBMN has a multiplier effect when layered atop a conventional package of vocational training, economic transfers, and civic engagement activities. In applying IBMN training to violence involving organized armed groups, we also take IBMN outside its usual context of inter-communal violence. Our research 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 and conflict has eroded state capacity.

The Youth Connect program specifically targeted villages within Maradi and Tillabéri with high youth vulnerability to violence or participation in armed groups. More broadly, Niger is a “most difficult” case in which to test peacebuilding strategies. Nevertheless, we find a substantial reduction in support for violence among youth in villages which participated IBMN training, relative to those in villages which participated in Youth Connect only. Respondents in Youth Connect villages that participated in Youth Connect and IBMN 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 participated in Youth Connect only. Youth in villages which received both IBMN training and Youth Connect were less likely to agree that “it is justifiable to use violence for a political or religious cause” in a list experiment, as well as slightly less likely to consider responding to a range of scenarios with violence. A difference-in-differences framework, using violent incidents as measured by the Armed Conflict Location and Event Data (ACLED) project, shows that villages selected for IBMN trainings experienced fewer violent incidents in which jihadist groups were the primary actor than villages which only received Youth Connect. These results suggest that IBMN training succeeded in helping villages to resolve conflicts before armed groups could interject themselves into local disputes.

Niger represents a critical and challenging case for building the evidence base around peacebuilding interventions in fragile settings.³ Violence 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 support for violence. For example, a randomized cash transfer program in Niger actually increased the number of violent incidents experienced by villages (Premand and Rohner 2024). Likewise, pro-peace religious messaging led to a backlash towards non-coethnics in neighboring Burkina Faso (Grossman, Nomikos, and Siddiqui 2023).

These results compliment a voluminous body of research which links economic privation and support for violence (Gurr 2011; Weinstein 2007). However, interventions predicated on this the-

³Nigers’ GDP per capita in 2023 was 618 USD, making it the 9th poorest country in the world, according to the World Bank.

oretical linkage have produced mixed results, often yielding null results or even backlash when causally evaluated (Dasgupta, Gawande, and Kapur 2017; Lyall, Zhou, and Imai 2020; Premand and Rohner 2024). Consequently, a growing literature explores the effects of complimentary interventions to reduce support for and participation in 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). Our results build on the growing interest in the ability of complementary interventions to build peace by assessing the extent to which training in conflict management skills may generate stronger peacebuilding effects above and beyond economic support.

More broadly, we engage a long-standing puzzle in both comparative politics and the political economy of conflict: how do you build peace where conflict resolution institutions are weak or inaccessible (Autesserre 2010; Blattman, Hartman, and R. A. Blair 2014; Kalyvas 2003)? Conventional approaches to building peace often function through existing conflict resolution institutions (Autesserre 2010). However, state institutions of conflict resolution in Niger are largely absent, allowing small-scale disputes to metastasize into violence (Blattman, Hartman, and R. A. Blair 2014; Kalyvas 2003). Armed groups in the Sahel exploit these gaps in state capacity by intervening in local disputes to extract resources or garner legitimacy from the individuals and communities on whose behalf they intervene (Benjaminsen and Ba 2024; Molenaar et al. 2019; UNDP 2017). IBMN training provides youth with the toolkit to resolve both their own conflicts and other conflicts within their communities. The fact that villages which received IBMN training experienced a reduction in violent incidents instigated by jihadist groups suggests that improving youth’s ability to mediate conflicts reduced the extent to which violent armed groups were able to leverage local disputes.

The paper proceeds in seven parts. The following section overviews the conflict in Niger and illustrates how violent armed groups leverage pre-existing disputes to advance their agendas. The second section argues that existing interventions, which largely focus on economic drivers of conflict, may not suffice to reduce violence; it then overviews two mechanism by which layering IBMN training atop an economic intervention could reduce violence. Section three introduces both IBMN 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 outlines heterogeneity within those results. Section six provides results from a difference-in-differences analysis which shows how villages in which IBMN trainings took place

experience fewer violence incidents. Section seven concludes the paper.

I Context: Niger

Violence in Niger is concentrated in border regions where state presence is weak. In 2022 alone, ACLED recorded 982 violent incidents across Niger, of which 37 percent took place in Tillabéri and 15 percent took place in Maradi. 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 and occasional incursions by Boko Haram. Figure 1 shows the Maradi and Tillabéri regions of Niger alongside the communes in which Youth Connect took place.

The principal violent organizations in Niger (and its neighbors) are Jama'at Nusrat al-Islam wal-Muslimin (JNIM) and Al Qaeda in the Islamic Maghreb (AQIM).⁴ Of the 2,720 ACLED events observed in Niger between 2020 and 2023, 1,704 list jihadist groups as the primary instigator and 629 list government forces as the primary instigator. A variety of local militia are responsible for much of the remaining violence. Across all events, 68 percent involved zero deaths. The event with the highest fatalities was a January 2020 battle between the Islamic State in the Greater Sahel and the Nigerien military in which 166 soldiers and militants were killed. The second most fatal event took place in March 2021, and involved an Islamic State raid on a Tuareg village in the Tahoua province, which borders both Maradi and Tillabéri.

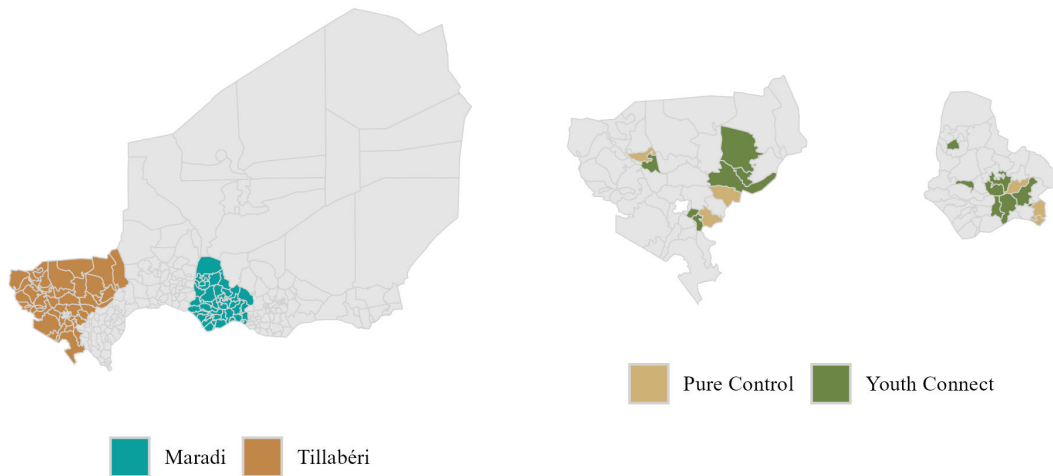
The absence of formal justice institutions is crucial for understanding how violent armed groups within both Niger and the broader Sahel function. In a 2022 survey by Afrobarometer survey, 20 percent of respondents in Niger reported having contacted their elected representative in the national assembly 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.⁵ In a 2022 survey in Maradi and Tillabéri, conducted as a baseline for Youth Connect, 54 percent of youth strongly or somewhat disagreed that their voice was heard by village and administrative authorities when dealing with them.⁶ With a formal state riddled by instability, traditional institutions provide conflict resolution and stability. When asked “If you had a dispute

⁴Boko Haram is also active in Niger's southeast, but is less relevant in Maradi and Tillabéri.

⁵These 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.

⁶These statistics are from a baseline survey conducted before Youth Connect, administered in the fall of 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 IBMN training was randomized at the village level within the 14 Youth Connect communes.

about land/livestock/a business 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.⁷

The lack of formal justice mechanisms allows small scale disputes to metastasize into broader conflicts. For example, long standing tension between pastoralist and farming communities in Mali and Niger recently escalated due to climate pressure and resource scarcity (Benjaminsen and Ba 2024). Similarly, Molenaar et al. (2019: 68) note that “Malian and Nigerien states have struggled to regulate [such] conflicts over natural resources... As a result, young [Peulh] and Tuareg Daoussak became increasingly involved in the conflict to protect their respective communities.” The absence of state capacity to resolve these conflicts allowed the conflicts to escalate.⁸

Armed groups make use of these lingering disputes. They often intervene in areas where local tensions are high in order to attract recruits, raise resources, or simply raise their standing in the

⁷Chiefs infrequently speak out against the armed groups directly, as many chiefs have been assassinated by both AQIM and JNIM.

⁸When the state does intervene, heavy-handed and repressive actions by state security forces have worsened the underlying conflicts and driven rural households to the violent armed groups for protection (UNDP 2017; Raleigh, Nsaibia, and Dowd 2021).

local community (Raleigh, Nsaibia, and Dowd 2021). Molenaar (2019: 96), for example, notes that “[i]n exchange for weapons, military training and protection, members of [Peulh] communities agreed to support or join the [MUJAO]. [Peulh] militiamen might not have agreed with the religious ideology of MUJAO, but both actors shared the same enemy, Tuareg Daoussak.” Likewise, Benjaminsen and Ba (2024: 5) narrate a long-standing grievances concerning pastoralists’ customary access to riparian pastures on the banks of the Niger. Politicians and local officials blocked access to these pastures and begun to collect bribes but “with Katiba Macina taking control over the delta from 2015, grazing fees to [these] pastures were abolished, which was obviously a popular move among pastoralist.” When Katiba Macina later started to permit fees again, the pastoralist community transferred its support to an alternative group which was later subsumed under the Islamic State in the greater Sahara.⁹

These anecdotes illustrate a common strategy among armed groups in the broader Sahel. The insurgent groups leverage small scale cleavages and long-standing grievances to advance their cause. By taking a side in these conflicts, these armed groups aim to attract supporters or increase their resource base. These narratives—that the relevant armed groups in Niger leverage local conflicts to advance their broader aims—go against the group’s stated religious goals. While the groups maintain jihadist ideologies Kalyvas (2018) argues that it is more productive to conceive of them broadly as a subgroup of violent groups—revolutionary insurgents—rather than as a novel typology. While ideology and religion are no doubt important to these groups, “jihadism has become a kind of ideological focal point around which all kinds of discontented and/or marginalized elements... have coordinated” (Kalyvas 2018: 43).

The above anecdotes suggest that religion acts as a catalyst for joining violent groups, rather than a driver in itself. Ideology may act as a focal point, but local grievances seem to drive mobilization. Indeed, the United Nations Development Program (UNDP) notes in its flagship *Journey to Extremism* report that violent extremist groups in Africa “us[e] religion as a touchstone for other context-based grievances” (UNDP 2017: 5) In a follow-up report, they similarly note that religion is “a potent vector for the mobilization of grievances” (UNDP 2023: 16). Grossman, Nomikos, and Siddiqui (2023) further show that a previously recorded religious appeal for peace and inter-group tolerance from a prominent Burkinabé imam actually led to backlash among respondents; they conclude that the message primed inter-ethnic identity, leading to reduced tolerance for other groups. These results suggest that the role of religion in driving participation complements contextual motivations.¹⁰ The erosion of formal justice and governance mecha-

⁹Katiba Macina itself joined AQIM before Youth Connect took place.

¹⁰Nevertheless, we control for religious education in all our specifications.

nisms, combined with existing grievances, and strategic engagement by armed groups has created an enabling environment for minor disputes to escalate into violence. In the subsequent section, we further underscore the need for building capacity for dispute resolution where formal conflict resolution institutions are absent.

2 Theoretical framework

Previous research identifies a variety of risk factors for violence, including political disputes, economic factors, and psychosocial causes. Consequently, scholars have tested a variety of interventions and programs to reduce participation in violence by addressing these risk factors (Bhatt et al. 2024; Blattman, Jamison, and Sheridan 2017; Lyall, Zhou, and Imai 2020). However, violence prevention programs often have multiple goals—not only to prevent any one violent act or any one individual from turning to violence, but to create an environment in which violence itself is more rare.¹¹

Reducing violence may require a different set of tools than reducing support for violence. 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. Armed groups in Niger such as JNIM and AQIM often intervene in local conflicts between farmers and pastoralist groups in support of Peulh 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 availability of decentralized conflict resolution should reduce these escalations by reducing the extent to which conflicts linger. We theorize that layering IBMN 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 Economic drivers of violence

Economic motivations have long been a core explanation of support for violence (Gurr 2011; Weinstein 2007). At the macro level, many of these explanations are predicated upon a view of

¹¹This 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). While our interest is in violence in general, rather than political violence per se, a substantial fraction of violence in Niger and the broader Sahel is political in nature.

civil conflict as a competition for resources. The role of economic factors in driving support for and participation in violence is well documented within the broader literature and in the Sahel more specifically. 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 within the Sahel means that both farmer-herder conflicts and cattle rustling are not uncommon (Benaminsen and Ba 2024).

Within such a framework, joining a violent group is a means to acquiring wealth or resources. Violent groups often pay recruits—which is a particular motivation in countries such as Niger where income-generating opportunities are scarce (UNDP 2017; UNDP 2017). 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 potential militant's perspective, these wages are often higher than any other employment, which makes participating in violence appealing from an 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 violence (Blattman and Annan 2016).

This view of violence as economically motivated have driven a variety of interventions which aim to reduce participation in violence. However, these interventions often have mixed results. Blattman and Annan (2016), for example, used an employment program among ex-combatants in Liberia to reduce participation in both illegal activity and employment as mercenaries in neighboring conflicts. In India, the roll-out 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 the cash transfers alone increased Taliban support in the short term (Lyall, Zhou, and Imai 2020). In Niger, a randomized cash transfer program increased the count of violent incidents in treated villages in the short term (Premand and Rohner 2024), though another cash transfer program in the Philippines decreased conflict (Croft, Felter, and Johnston 2016).

Risk factors for participation in violence often overlap (Humphreys and Weinstein 2008). To address multiple risk factors, a growing body of evidence explores multi-armed interventions. Bhatt et al. (2024) combine an 18 month employment training in Chicago with cognitive behavioral theory (CBT); 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 caused by a CBT intervention ebbed after a year, but when the CBT was combined with

cash grants, the reduction in violent behaviors endured (Blattman, Jamison, and Sheridan 2017). Similar to the purely economic interventions, these programs show a variety of results, even if the trend is broadly positive.

One explanation for these mixed results is that a variety of political and social factors contribute to poverty, therefore one-off interventions are unlikely to move the needle on economic privation. Across a variety of interviews with former militants and other youth, the UNDP reports that “economic deprivation appears to translate into a generalized sense of hardship and grievances affecting the outlook of all respondents, further underpinned by gender roles and identities affecting the socio-economic incentives and motivations” (2023: 72). These factors may be particularly relevant in contexts such as Niger, where conflicts between pastoralists and farmers largely center access to resources. For example, if pastoralists receive a cash transfer or vocational training, but remain unable to access traditional water points, the economic intervention will have minimal or only short-term effects on violence.

2.2 Violence and conflict resolution institutions

Where state institutions are weak or absent, and formal avenues for conflict resolution are lacking, development interventions may be necessary but are unlikely to be sufficient to reduce violence or support for violence. Such contexts often have complex but intensely local constellations of power and contestation over limited resources (Bierschenk and De Sardan 1997). Given these constraints, how can governments or practitioners reduce violence in areas where the state is weak? If local disputes lead to violence, then peacebuilders could reduce violence through two mechanisms: they could either reduce the extent of small scale disputes through conflict resolution institutions, or they could weaken the magnitude of the relationship between disputes and violence by turning public opinion against violence

Introducing decentralized conflict resolution could reduce the ‘reservoir’ of conflicts available for armed groups to leverage. The Tillabéri and Maradi regions of Niger exist in a state of contestation between rival armed groups and the Nigerien state. None of these groups can exert continuous control over territory. Consequently, both local elites and rural individuals have various incentives to collaborate with one armed group or another (Balcells 2017). A pastoralist may collaborate with JNIM because they believe JNIM would restore their foraging rights or their access to waterways. JNIM may choose to attack an area because they know there are pastoralists whose foraging rights having been taken who may support them. More broadly, armed groups know that siding with an aggrieved local party could buttress their local support. This dynamic is

clear in Niger, where insurgent groups such as JNIM and AQIM recruit largely from Tuareg and Peulh youth who are often excluded from village institutions (Raleigh, Nsaibia, and Dowd 2021). Though JNIM and AQIM are jihadist groups, Kalyvas (2018: 37) notes how such groups often “act in ways that parallel those of nonreligious insurgent actors.” Similarly, in northern Nigeria, Lichtenheld and Ogbudu (2021) show inter-religious conflicts often start as interpersonal conflicts and are later grafted onto religious disputes by either the parties to the conflict or by local elites. 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.” Local cleavages, rather than overarching grievances, often structure low level violence.

Small-scale disputes escalating to large-scale violence is hardly unique to Niger. Persistent violence in Chad contributed to a situation where violence became a viable occupation for young men and “periods spent undertaken economic activities performed with or without arms, often in the margins of the state” became common (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 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.

Reducing the extent to which these disputes linger or metastasize is one strategy to reduce violence in areas where the absence of conflict resolution institutions precipitates violence. 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 contact between farmers and herders in Nigeria led to more positive feelings towards out-group members and increased feelings of security.

The second potential mechanism through which peacebuilders could reduce violence in such areas is to shift public opinion against the acceptability of violence. Reducing support for violence could reduce the extent to which individuals are willing to side with armed groups or turn towards violence themselves. Armed groups’ strategy of involving themselves in local disputes requires selective, rather than indiscriminate, use of violence (Balcells and Stanton 2021). If JNIM used

indiscriminate violence to intervene in a farmer-herder dispute, then they would be just as likely to harm the party to the dispute whose support they aimed to gain. Consequently, these armed groups must rely on local collaborators to provide the qualitative context necessary to intervene selectively.

Reducing support for violence, or the perceived acceptability of violence, may attenuate the relationship between these underlying disputes and the eventual violence. Social norms are important drivers of behavior; social norms against violence will make an individual less likely to participate in violence themselves (Wenzel and Woodyatt 2025). Reducing support for violence will likely reduce the extent to which individuals are willing to share such information with armed groups. It may also make the individual less likely to convey information to the armed groups, knowing that violence will follow. An individual may think they could ‘get away’ with sharing information with an armed group, but when the armed group intervenes on their behalf, the beneficiaries of that violence will be clear. As such, decreasing support could make an individual less likely to share information with an armed group either because they disapprove of the armed groups’ tactics or because they fear the social sanctions which would follow.

These dynamics illustrate 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 absent, which can lead small-scale disputes to escalate into violence. The lack of economic opportunity can increase individual willingness to participate in or support violence, while the lack of institutions creates an environment in which small scale disputes can escalate. Peacebuilding interventions which are inattentive to one or more of these drivers of violence may fail to move the needle on support for 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 IBMN Training

We study a multi-pronged intervention, titled Youth Connect, which addresses economic, social, and political drivers of violence simultaneously.¹² 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

¹²Mercy 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.

employment; and (3) training youth in civic engagement skills to help them actively engage with local governance structures and voice their needs. These components targeted men and women between the ages of 15 and 34.

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.¹³ 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.

The second objective was to improve youths' access to resources needed to pursue the livelihoods in which they were trained.¹⁴ To this end, youth received a series of 'start-up' kits relevant to the specific trainings they received. 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.

However, in areas of extremely weak state capacity, such as Maradi and Tillabéri, enhanced livelihoods and increased participation may not suffice to alleviate violence. To that end, we implemented an RCT within the broader Youth Connect program to test the effect of IBMN training as an add-on to Youth Connect's conventional violence prevention strategy. IBMN is an approach to negotiation adapted from *Getting to Yes*, a how-to guide for negotiation often taught in American business schools (Fisher and Ury 1981). IBMN 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 if need be. Mercy Corps has previously deployed IBMN training in peace-building programs in Afghanistan, Colombia, Ethiopia, Guatemala, Jordan, Kenya, Mali, Myanmar, Nigeria, Iraq, and Tajikistan. In northern Nigeria, training community leaders in IBMN

¹³Youth Connect consortium member SwissContact developed these trainings. 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.

¹⁴In appendix B we show that Youth Connect had positive effects on wealth, employment, and other measures of economic welfare.

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 IBMN within the context of a broader intervention to prevent participation in violence and show how IBMN complements more conventional efforts to reduce support for violence. Unlike previous projects, this research also explicitly centers youth in the intervention, to provide them with non-violent strategies to resolve conflicts and act as a mediator in the the 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 IBMN training in consultation with communities and community leaders. Of the 198 youth leaders that participated in this initial training, 100 were women and 83 were men.¹⁵ These youth, aged 16 to 34, attended trainings in IBMN techniques which took place in the eponymous region-level capitals of Maradi and Tillabéri. The trained youth leaders then led training in IBMN techniques within their home villages.

IBMN training for youth leaders 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 other's positions." The training also provided images of and guides to the seven elements of negotiation and mediation.

Youth leaders resolved a variety of conflicts. In one instance, an IBMN-trained youth leader resolved 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 at-

¹⁵The remaining 15 did not fill out the questionnaire associated with the training.

tempted 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.”¹⁶

To measure the immediate effect of this IBMN training on participants, we administered a short questionnaire before and after they participated in the training. Compared to before the IBMN 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 IBMN training.”¹⁷

Within Youth Connect, the IBMN intervention should increase the local availability of conflict resolution in areas where the state is scarce. By making it easier to resolve conflicts before armed groups arrive—and by decreasing public acceptance of violence—IBMN should be well-positioned to counteract the co-production of violence by communities and armed groups described in the qualitative literature on conflict in Niger. More specifically, we hypothesize that:¹⁸

H.1 Respondents in villages that received Youth Connect and IBMN 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 Youth Connect to villages where it had the greatest potential to impact violence.¹⁹ 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

¹⁶These citations come from qualitative monitoring and evaluation conducted by the Mercy Corps team.

¹⁷Appendix D explores these data and presents the results in greater detail.

¹⁸In our pre-analysis plan, we also hypothesized that respondents villages which received neither Youth Connect nor IBMN would show greater support for violence than respondents in the Youth Connect only villages. Appendix B shows no support for this hypotheses. We exclude discussion of the pure control from the main results because selection into the pure control was assigned non-randomly at the commune level.

¹⁹Youth Connect was also implemented in Burkina Faso; IBMN trainings took place only in Niger. Communes were chosen non-randomly to maximize the potential impact of Youth Connect; we discuss the implications of this selection for external validity below.

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.²⁰

We randomized assignment to IBMN training within stratas defined by village type and the presence of ACLED events two year period before randomization took place. Stratification generally leads to efficiency gains because partitioning the covariate space over which the treatment is randomized can rule out uninformative comparisons while preserving randomization (Athey and Imbens 2017). 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 exposure ACLED events to also capture pre-intervention exposure to violence. All but three villages in Tillabéri had at least one ACLED event within 25 kilometers, so we do not stratify on ACLED in Tillabéri.²¹

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 IBMN are unusually vulnerable to violence and recruitment by violent organizations. However, violence reduction programs generally center those who are most likely to support violence, so our focus aligns with the general literature.²² These youth are not representative of the super-population of Nigerien youth, but they are nevertheless the key population of interest.

Table A3 shows that respondents in Youth Connect only and Youth Connect plus IBMN villages are balanced across all village-level variables and all but two individual-level variables. Respondents in Youth Connect plus IBMN villages are more likely to be ethnically Tuareg and less likely to have completed secondary education or higher. Tables A5 and A6 show that Tuaregs are weakly less likely to support violence, and table A8 suggests that treatment might be more effective among Tuaregs. However, Tuaregs make up only six percent of the weighted sample, so this imbalance is unlikely to shift our results. In fact, table A8 shows that IBMN treatment effects are stronger when controlling for heterogeneous treatment effects by ethnicity. Tables A5 and A6 show no relationship between education and our outcome variables. Together these

²⁰For more details on the village selection process, see: Ribar, Sheely, and Lichtenheld (2023).

²¹Initially, all 84 villages plus the pure control villages were to receive both Youth Connect and IBMN after the midline survey, but all implementation was canceled as part of the January 2025 foreign aid freeze.

²²For example, see Blattman, Jamison, and Sheridan (2017: 1166) or Bhatt et al. (2024).

results suggest that imbalance is not a problem for the paper’s findings.

4.2 Measurement strategy and hypotheses

We primarily measure outcomes through a survey of 1,342 youth across 82 villages in Maradi and Tillabéri: 41 villages that received Youth Connect only and 41 villages which received Youth Connect and the IBMN treatment.²³ 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 IBMN training.²⁴

Within each of the 82 villages, we surveyed two groups. We surveyed ten youth per village selected through a random walk within villages. Once enumerators arrived at households, they asked the household head how many individuals between the ages of 15 and 34 lived in the household. The tablet-based survey instrument randomly selected one of these youth to whom the enumerator administered the survey. Enumerators administered an informed consent procedure at each stage: both household heads and individual youth consented to participate in this survey.²⁵ This process selected 840 young people across the 82 villages.²⁶

A random walk does not guarantee that sampled youth would have directly participated in Youth Connect’s economic activities. As such, we also directly sampled 502 youth who participated in Youth Connect’s vocational training and civic engagement activities. We randomly sampled these youths through Mercy Corps’ record of participation in training. Sampling for these 532 youth was independent of IBMN treatment, but 257 came from villages which received Youth Connect only and 245 came from villages which received both Youth Connect and IBMN training. The survey instruments were identical for the randomly selected sample and the sample selected through Youth Connect records.

We use multiple strategies to measure support for using violence. Our first priority was to ensure enumerators and respondents felt safe and comfortable answering these questions. Niger is a particularly fragile environment, with a history of crackdowns on dissent (Amnesty Inter-

²³One important caveat is that our sample size was driven by budget considerations, rather than power calculations. The pre-analysis plan for the paper also did not include power calculations. We can use the collected data to estimate post-hoc power, which gives us a minimum detectable effect of approximately 0.245 in the additive index for the direct questions, which suggests we were adequately powered to detect our true effect sizes.

²⁴In addition, we survey 392 additional youth across 36 pure control villages which were slated to receive Youth Connect as well as IBMN training after the midline survey took place. Because assignment into the pure control was non-random, we present these results in appendix B rather than in the body of the paper.

²⁵Consent scripts were available in French, Haoussa, and Zarma. All respondents consented to participate in the survey.

²⁶The discrepancy is due to the two villages in which we were unable to implement the follow-up survey.

national 2024). As such, we coordinated with Mercy Corps' local country office, who advised that questions which directly identify any individual's level of support for violence would cause discomfort to respondents and potentially put both respondents and enumerators at risk.²⁷

Our secondary concern was to minimize social desirability bias. 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. Blair, Coppock, and Moore (2020) note that the risk of social desirability bias is often overstated. However, direct questions about support for violence fit all four of their risk criteria for social desirability bias: respondents know (from the informed consent) that a donor organization is asking questions, they would know the questions could infer their support directly, they would likely suspect that the donor would not want them to support violence, and they would suspect that answering with true preferences could create risk for them (G. Blair, Coppock, and Moor 2020: 1311).

We use multiple indirect survey measures to capture support for violence in order to ensure enumerator safety, ensure respondent safety, and minimize social desirability bias. 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. These questions are indirect because they ask respondents about the community's support for violence, not the respondent's.

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 sensitive item in the response list, researchers can back out an average level of support for the sensitive item. This strategy mitigates social desirability bias because respondents never admit support for the sensitive item specifically. For this survey, enumerators asked respondents:

²⁷For example, if we had asked direct questions about an individual's support for violence, and the Nigerien state or by non-state armed groups confiscated the tablets, survey respondents could be at risk of violence or repression.

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.”

Westwood et al. (2022) point out that responses to hypothetical questions about violence change when respondents are presented with more concrete scenarios. It is easier to support violence in the abstract than in a specific circumstance. We explore this difference through a series of list experiments which present specific scenarios to the survey respondents. More specifically, we asked respondents to

Imagine 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.

We consulted with Mercy Corps' country office to ensure these scenarios applied to Niger. 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 advice to the village chief. You think that this advice will 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 (i.e. sensitive items) they received, if any. 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 ordinary least squares (OLS) and within-sample inverse-probability survey weights.²⁸ 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 IBMN training and so comparisons between villages which received Youth Connect only and those that received Youth Connect plus IBMN are unbiased and consistent. 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.²⁹

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

$$y_i = \beta_1 \text{IBMN} + \psi_1 X_i + \psi_2 X_v + \gamma_r + \epsilon_i$$

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 . 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 is β_1 . The baseline level is villages which received Youth Connect but not IBMN training.

We repeat these models with and without two sets of control variables. Demographic control

²⁸Appendix A.2 describes our weighting in greater detail.

²⁹Appendix A.1 explores the rationales for enumerator fixed effects in greater detail. However, we note that our pre-analysis plan did not pre-specify the use of enumerator fixed effects. Nevertheless, table A.1 shows that our coefficients of interest remain reasonably stable with and without enumerator fixed effects, across different specifications.

variables are: ethnicity, education (none, madrassa or Koranic school,³⁰ primary, or secondary or higher) autochthony, age, age squared, an index of household wealth, and sex.³¹ We calculate village-level covariates using a 25 kilometer radius around the village centroid. They include the count of all ACLED incidents in the two years pre-treatment,³² 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.³³

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

$$y_i = \beta_1 \text{IBMN} + \beta_2 \tau_i + \beta_3 \text{IBMN} \cdot \tau_i + \psi_1 X_i + \psi_2 X_v + \gamma_c + \epsilon_i$$

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 coefficient of interest is the interaction between the survey experiment treatment indicator and whether the village received IBMN (β_3), which captures whether respondents in villages which received Youth Connect and IBMN 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).³⁴ 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 IBMN treatment village.

³⁰While madrassa/Koranic education is a weak proxy for religiosity, more precise proxies such as mosque attendance or the amount of the Koran memorized are post-treatment measures, which would introduce potential bias into results.

³¹Autochthones are the descendants of the initial settlers of the village; while the term is most common in franco-phone Africa, elsewhere such residents are called ‘indigènes’ or ‘sons of the soil.’

³²The sampling strategy only took into account whether a village had one more ACLED incidents, but the results are substantively identical for the effect of being randomized into IBMN training when using strata fixed effects.

³³Another threat to inference is interference from neighboring villages. In appendix A.4 we show that spatial proximity to IBMN treatment does not predict our outcomes, so spatial spillover is not a concern.

³⁴AMCEs 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 IBMN training

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IBMN Treatment	-0.026 (0.055)	-0.027 (0.055)	-0.056 (0.046)	-0.047 (0.044)	-0.123** (0.046)	-0.099+ (0.050)	-0.121* (0.052)	-0.165** (0.054)
Demographic Controls		X		X		X		X
Geographic 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	1.808	1.808	1.532	1.532	1.867	1.867	1.593	1.593
Num.Obs.	1333	1332	1328	1327	1331	1330	1335	1334
R ²	0.557	0.568	0.524	0.544	0.576	0.593	0.405	0.431

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, education (including Islamic), 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 IBMN treatment on the survey questions about community perceptions of support for violence. Households in villages which received IBMN treatment are less likely to believe their community perceived violence as justified. Respondents which received IBMN training have a 0.12 to 0.14 standard deviation reduction in the perception that violence is justified to defend one's religion and a 0.14 to 0.23 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 overall mean levels. Respondents in IBMN 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 IBMN training are negative for these columns, but they are not statistically significant. These results are similar with and without control variables, suggesting that control variables are 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 IBMN villages were consistently 0.12 standard deviations less likely to report community support for the use of violence.

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

	Additive index		1st principal component	
	(1)	(2)	(3)	(4)
Youth Connect + IBMN	-0.285* (0.143)	-0.292+ (0.153)	-0.145* (0.069)	-0.141+ (0.074)
Demographic Controls		X		X
Geographic Controls		X		X
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Num.Obs.	1342	1341	1314	1313
R ₂	0.598	0.609	0.644	0.653
Mean of outcome	6.763	6.763	0.019	0.019

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, education (including Islamic) 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.

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

	(1)	(2)	(3)	(4)
List treatment	0.018 (0.067)	0.039 (0.065)	0.127 (0.088)	0.201* (0.082)
List treatment * IBMN village			−0.215+ (0.129)	−0.327* (0.124)
Demographic Controls		X		X
Geographic Controls		X		X
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Num.Obs.	1339	1338	1339	1338
R ²	0.312	0.376	0.322	0.387
Mean of outcome	2.401	2.401	2.401	2.401

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.

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.’ 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 whether the respondent’s village received IBMN treatment. A positive interaction would suggest that members of the subgroup are more likely to agree with the treatment item when presented it.

The interaction effect between the list treatment and being in a village that received both IBMN treatment and Youth Connect is negative and statistically significant, which supports our hypothesis. In villages that received Youth Connect only, the marginal effect of being assigned the additional item is a 0.201 point increase in the count of statements with which respondents agreed. Within villages that received both Youth Connect and IBMN, the marginal effect of being assigned the list treatment is statistically indistinguishable from zero. In other words, respondents in Youth Connect only villages agreed that it is sometimes justifiable to use violence;

respondents in villages that received both Youth Connect and IBMN treatment did not. The combination of Youth Connect and IBMN training reduced support for violence more than Youth Connect by itself.

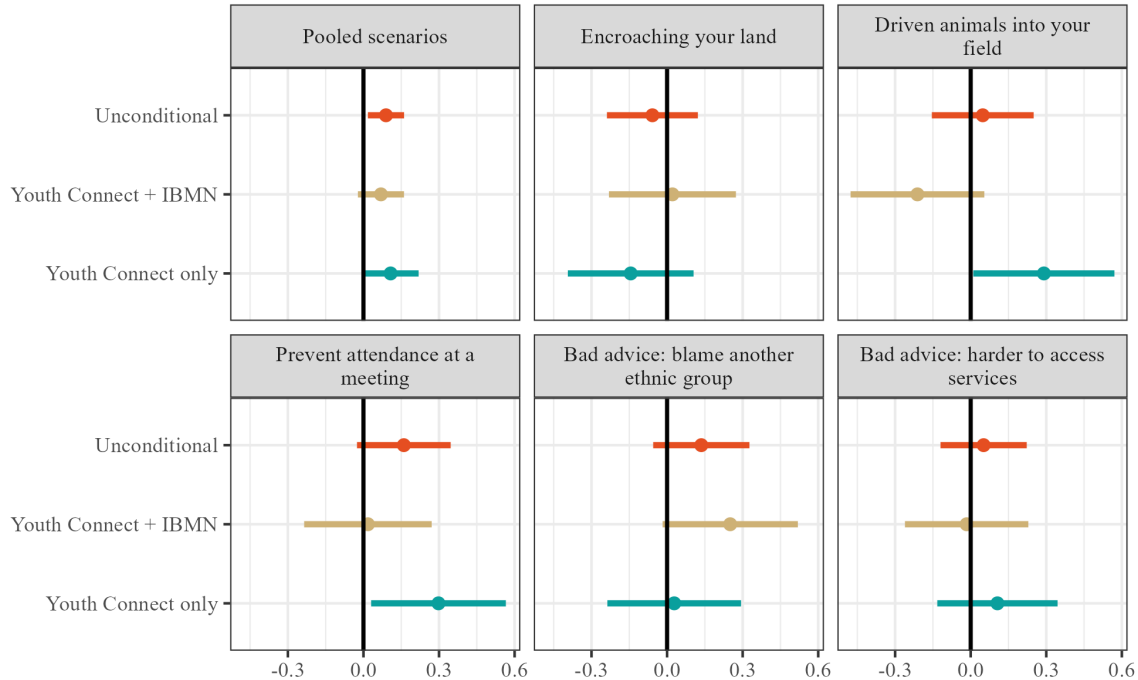
Finally, figure 2 illustrates the AMCEs and subgroup AMCIEs from the set of factorial list experiments. The overall AMCE for receiving any of these treatments, pooled across the different scenarios and IBMN treatment arms, is 0.086 percentage points. In other words, receiving the sensitive item as an 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 the villages which received both Youth Connect and IBMN, treating respondents with the additional sensitive item did not lead respondents to select additional items.

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” or “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, approximately 16 and 13 percent of respondents said they could consider an additional action when presented with the option of violence. In contrast to our other results, figure 2 provides suggestive, but not conclusive, evidence that the willingness to use violence is lower in the villages that received IBMN 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 IBMN participation reduced support for violence among village youth. These results are strongest when asking whether “people in your community think it is... justified to use violence.” Survey experiments show similar results. These results support our hypothesis: respondents in villages that received IBMN alongside Youth Connect had lower support for violence than respondents in villages which received Youth Connect only.

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”

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.

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: IBMN reduces support for violence.

We also explored a variety of heterogeneous treatment effects (HTEs) which we specified in the pre-analysis plan.³⁵ First, the effect of receiving Youth Connect and IBMN 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 per-

³⁵We focus on HTEs for the direct outcomes (the 'justified') questions, because adding a triple interaction to the survey experiments adds computational complexity and reduces statistical power.

cent of respondents identified as Haoussa, 29 percent as Zarma, 10 percent as Peulh, and 9 percent as Touareg. Among Zarma respondents, participation in both Youth Connect and IBMN did not lead to a marginal decrease in support for violence, relative to participation in Youth Connect alone. This finding is potentially 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. We also test for HTE by autochthony status, but find no difference in treatment effects between autochthones and other respondents (table A9). However, relatively few non-autochthones participated in the survey, so these subgroup effects are 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-scaled 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 plan, 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 see no difference in these outcomes between villages which did and did not receive IBMN treatment.

We also deploy 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.³⁶ Following Athey and Wager (2018), we estimate CATEs using a series of causal forests. We then calculate variable importance by the number of times the causal forest split on the variable, weighted by the depth of the split. This procedure highlights that adding IBMN 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 in poorer villages.³⁷ These findings complement appendix B, which suggests that Youth

³⁶We specified this procedure in our pre-analysis plan, but because of the data-driven procedure, we did not specify which variables would be included.

³⁷The 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.

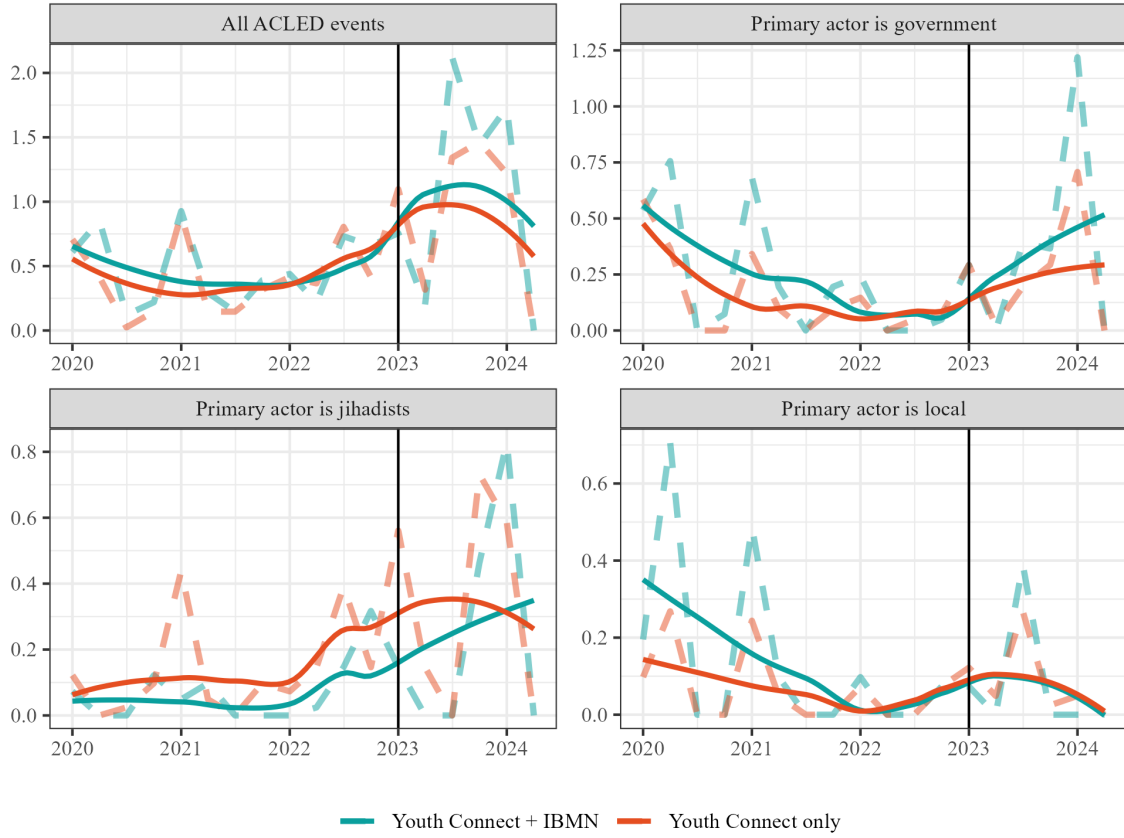
Connect by itself did have positive economic effects.

6 Difference-in-difference with conflict data

Does reduced support for violence translate into a reduction in violence? We use data from the ACLED project to show that villages which received both IBMN 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 IBMN, 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.

For the difference-in-differences estimator to be valid, villages which received both Youth Connect and IBMN would have had similar outcomes to those that received only Youth Connect, in the counterfactual state where IBMN 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 and Youth Connect plus IBMN 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, AQIM, or JNIM), or a local actor such as a village or ethnic militia. Figure 3 provides visual support for the parallel trends assumption between villages which received only Youth Connect and villages that received Youth Connect and IBMN. The average counts for different incidents move in parallel for extended periods of time before treatment.

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 both 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.

In our case, a two-way fixed effects (TWFE) design recovers our the LATE of IBMN 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 accommodate the count structure of the data.

Table 4 shows an overall decrease in violent events in villages that received the IBMN treatment, relative to villages that only participated in Youth Connect. While the coefficient is weakly

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

	All incidents	Government	Jihadist group	Local groups
D-in-D: IBMN	-0.528+ (0.280)	0.836 (1.066)	-1.429* (0.712)	-0.847 (1.167)
Village + period FEs	X	X	X	X
Num.Obs.	120	34	54	30
R ²	0.288	0.213	0.240	0.044

Note: This figure reports the TWFE difference-in-difference coefficient comparing villages which received IBMN 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.

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. Adding IBMN to Youth Connect is associated a marginal decrease of 1.16 incidents instigated by jihadist organizations, a decrease of 0.55 standard deviations against a mean of 0.75 events per village. Violence increased in both sets of villages, but that increase was smaller in villages that received IBMN.

Table 4 includes ACLED events six months before and six months after IBMN 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. 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 IBMN treatment reduced the count of incidents for which the primary actor is either local militia or the Nigerien government.³⁸ 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 IBMN training experienced a reduction in violent events, relative to villages that received Youth Connect only.

These results show that IBMN training not only reduced support for violence in the target population, but moved the needle on violence itself. The RCT shows that layering negotiation

³⁸Results are similar when using 10 kilometer radii rather than 25 kilometer radii as in table 4. However, the sample size further dwindles. Figure E1 displays these results.

training atop a peacebuilding intervention which focuses on economic drivers of conflict acts as a ‘force multiplier.’ Despite positive results, however, we remain unable to decisively adjudicate between alternative mechanisms. Qualitative and policy-focused research shows that armed groups in both Niger and the broader West Africa leverage small-scale disputes and pre-existing cleavages to recruit collaborators and extract resources (Benjaminsen and Ba 2024; Molenaar et al. 2019). We identified two mechanisms through which IBMN training could disrupt this process. First, by improving the availability of conflict resolution, IBMN could reduce the ‘reservoir’ of disputes available for armed groups to intervene. Second, by turning popular opinion against violence, IBMN could decrease people’s willingness to either bring their dispute to the armed group or accept the armed group’s help.

Ultimately, we find limited evidence for both mechanisms. We were unable to monitor the increase in dispute resolution: after the 2023 coup, Mercy Corps redistributed in-country resources to focus on delivering the core interventions, rather than conducting additional monitoring and evaluation. However, the anecdotal data we collected suggested that the youth leaders themselves resolved most conflicts. Similarly, we find no difference between villages that received IBMN and villages which received Youth Connect only across a variety of secondary outcomes, including social cohesion questions and questions about youth agency. Tables A11 and A12 show these null results in greater detail. However, appendix D shows that the youth leaders themselves do perceive themselves to be more efficacious as mediators after IBMN treatment, though these estimates suffer from both a small sample and probable demand effects on the respondent. Together, these results suggest that the lessons of IBMN training did not fully diffuse to the broader population, but that youth leaders themselves were effective mediators. While the results from our survey and from the difference-in-differences using ACLED data illustrate that the combination of Youth Connect and IBMN training decreased both violence and support for violence, we are not able to adjudicate between alternative mechanisms.

7 Discussion and Conclusion

This article presents results from a cluster-randomized experiment in the Maradi and Tillabéri regions of Niger which randomized assignment into IBMN 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 1,342 youth across 118 villages, including 41 villages that received Youth Connect and 41 villages that received Youth Connect alongside IBMN training.

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

Youth that received both Youth Connect and IBMN perceived less community support for violence than youth in villages that received Youth Connect only. Youth in villages that participated in IBMN were less likely to themselves support the use of violence, as demonstrated by a series of survey experiments. Youth Connect by itself did not reduce 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 IBMN training was most effective in reducing support for violence in economically deprived areas.

Beyond reducing support for violence, adding IBMN to Youth Connect reduced the incidence of violence. Using a difference-in-differences framework, we show that villages which received IBMN 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. 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.

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: extreme poverty, political instability, climate-driven shocks, and porous borders that channel conflict spillovers from Mali, Burkina Faso, and Nigeria. These circumstances, however, need not inherently constrain the external validity or potential replicability of the findings presented here. Our results suggest that in contexts where formal dispute resolution is weak and where armed actors routinely leverage local disputes and cleavages, layering training that builds conflict resolution capacity onto youth-focused development programming can meaningfully reduce support for, and in some cases the incidence of, violence. These conditions are not unique to Niger; political instability, weak states, and extreme poverty are found in many fragile and conflict-affected settings across Africa and the Middle East. Even in countries with functioning national courts, peripheral or border regions often face thin institutional reach and limited supply of timely conflict resolution (Boone

2003). In such settings, the light-touch IBMN complement tested here is likely to be most valuable. Future research should more precisely delineate other scope conditions under which these effects hold and examine how variation in access to dispute resolution, youth network structures, and the strategic behavior of armed groups shapes the effectiveness of combining development interventions with conflict resolution training.

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Appendices

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A Estimation details

A.1 Tests for enumerator effects

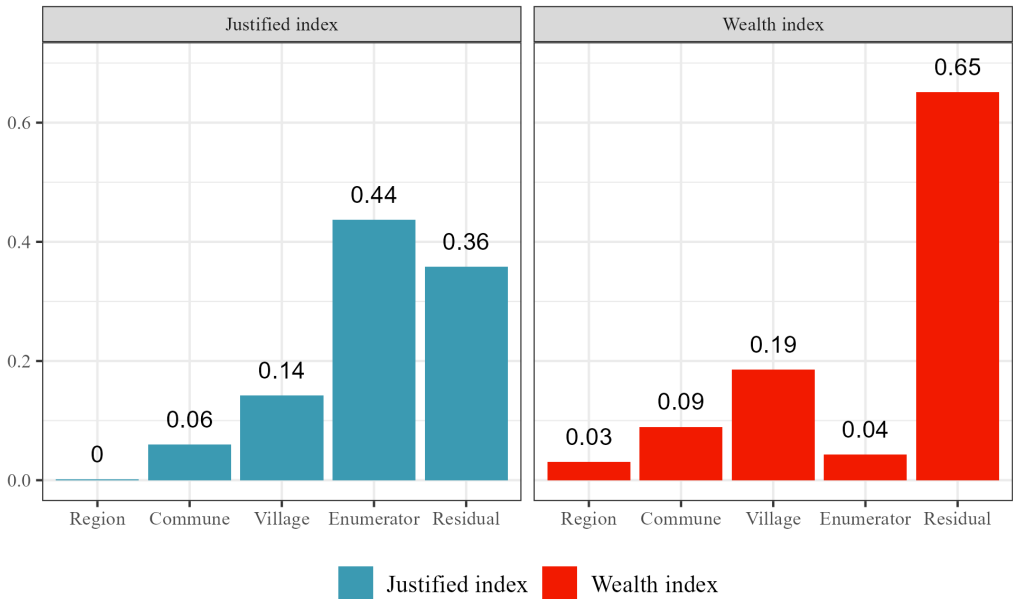
While the pre-analysis plan did not include enumerator fixed-effects, this section shows they are necessary to absorb a significant portion of the variation seen in the outcome measures. The median village was visited by four different enumerators. The median enumerator visited 16 different villages. 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 Tillabéri 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. Mismatches in sex or ethnicity often drive enumerator effects (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, 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 shows that enumerator fixed effects explain much of the variation in survey responses, but only for the questions about

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.

violence.

Enumerator effects are a source of measurement error which affects the outcome variables of our research. 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 IBMN 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 IBMN is reasonably consistent. While the level of statistical significance oscillates, 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 assign-

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control	0.824*	0.105	1.008*	0.282				
	(0.348)	(0.213)	(0.426)	(0.306)				
Youth Connect + IBMN	-0.232	-0.323*	-0.362	-0.318*	-0.217	-0.204	-0.089	-0.166
	(0.272)	(0.147)	(0.223)	(0.154)	(0.267)	(0.163)	(0.301)	(0.194)
Direct YC recipient	0.924**	-0.977*	0.909**	-0.993*	1.035**	-1.027*	1.063**	-0.886*
	(0.340)	(0.396)	(0.328)	(0.449)	(0.313)	(0.411)	(0.320)	(0.400)
Ethnicity: Peul			-0.465	-0.479*			-0.302	-0.463*
			(0.296)	(0.195)			(0.263)	(0.207)
Ethnicity: Tuareg			0.584	-0.342			0.580	-0.141
			(0.477)	(0.173)			(0.420)	(0.204)
Ethnicity: Zarma			0.766*	-0.230			0.384	-0.407**
			(0.348)	(0.179)			(0.457)	(0.153)
Age			-0.131	-0.089			-0.155	-0.094
			(0.132)	(0.089)			(0.142)	(0.090)
Age squared			0.002	0.002			0.003	0.002
			(0.003)	(0.002)			(0.003)	(0.002)
Female			-0.037	-0.201			-0.027	-0.198
			(0.145)	(0.116)			(0.141)	(0.112)
ACLED events			-0.010	-0.014**			0.023	0.001
			(0.008)	(0.004)			(0.047)	(0.030)
ACLED deaths			-0.002	0.000			-0.036	-0.008
			(0.007)	(0.004)			(0.023)	(0.014)
N. of YC beneficiaries			0.002	0.001			0.002	0.000
			(0.002)	(0.001)			(0.002)	(0.002)
Distance to a river			-0.060*	-0.017			-0.048	-0.046*
			(0.025)	(0.018)			(0.027)	(0.021)
Distance to the border			-0.010**	-0.007*			0.008	0.008
			(0.003)	(0.003)			(0.014)	(0.009)
Commune seat			-0.067	0.026			0.042	0.126
			(0.263)	(0.173)			(0.282)	(0.194)
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
R ²	0.041	0.574	0.093	0.589	0.110	0.597	0.145	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.
Youth Connect only	Random sample	6.94	0.50
Youth Connect only	Beneficiary	6.67	0.44
Youth Connect + IBMN	Random sample	6.65	0.67
Youth Connect + IBMN	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.

ment. 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. 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 IBMN treatment, 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. These results reinforce our decision not to include the pure control groups in the main body of the paper.

If enumerator effects are positively correlated with being in a pure control commune, then 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 IBMN 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

We weight our regressions to be representative of youth within the villages and commune centers selected for IBMN 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 of the non-random selection of villages into Youth Connect (via Mercy Corps’ village selection tool).

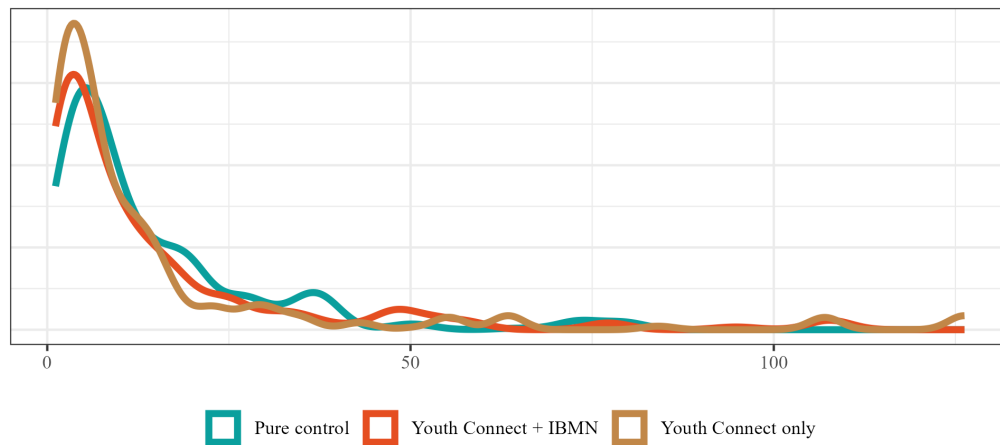
For our inverse probability sampling weights, we take the sample of villages and communes as given, and then calculate a youth’s probability of being chosen. For the beneficiary sample, the probability is even—every youth in the list of beneficiaries had an equal probability of being chosen for the sample. For the random sample of youths, we calculate two numbers. First, using the 2014 Nigerien census (Répertoire des localités), we 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. We multiply these numbers together and take the inverse.¹

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 weight.

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 proceed confidently using these within-sample survey weights.

¹The Répertoire des localités is a rough measure of village population, both because of its age and because of potential survey errors. These errors present 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 IBMN and Youth Connect treatment groups.

A.3 Balance table

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

	IBMN + Youth Connect		Youth Connect only		Difference
	Mean	Std.Err.	Mean	Std.Err.	T-value
Individual-level covariates					
Age	23.04	5.57	22.46	5.57	1.50
Female	0.41	0.49	0.39	0.49	0.66
Wealth index	1.03	1.37	1.15	1.39	-1.27
Ethnicity: Haoussa	0.63	0.48	0.68	0.47	-1.52
Ethnicity: Zarma	0.22	0.41	0.24	0.43	-0.83
Ethnicity: Peul	0.04	0.20	0.04	0.19	0.25
Ethnicity: Tuareg	0.09	0.28	0.02	0.15	4.20
Education: madrassa	0.19	0.39	0.14	0.35	1.88
Education: none	0.14	0.34	0.12	0.33	0.70
Education: primary	0.27	0.45	0.23	0.42	1.38
Education: secondary/higher	0.40	0.49	0.50	0.50	-3.11
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	104.73	48.30	104.63	56.57	0.03
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 IBMN 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 IBMN treated villages does not predict support for violence

Outcome	Estimate	Std.Err.	T-statistic	P value
Defend your ethnic group	0.002	0.004	0.371	0.713
Force the government to change their policies	0.004	0.003	1.127	0.267
Defend your religion	0.006	0.004	1.387	0.173
Retaliate against violence	0.002	0.004	0.428	0.671
Index (1st principal component)	0.008	0.006	1.311	0.197

Note: This table estimates the spatial spillover of IBMN treatment among 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 IBMN treatment. Did a nearby village being treated with IBMN 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 IBMN training is 9.45 kilometers. The median distance is 6.98 kilometers. Between villages in the pure control communes and villages treated with IBMN, those statistics are 20.3 kilometers 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 IBMN, subsetting to villages that received Youth Connect only. The table shows that spatial proximity to villages treated with IBMN 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 receive 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.² 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 (plus the commune centers) were selected for Youth Connect.

In phase two of the project, villages in the six pure control communes were to receive both Youth Connect and IBMN. Likewise, villages that received only Youth Connect in phase one were to receive IBMN in phase two. However, Youth Connect was canceled in January 2025, before phase two could begin.

²For more details on village selection, see: Ribar, Sheely, and Lichtenheld [2023](#).

A.6 Coefficient values for main tables

Table A5. Coefficient values for table 1

	Defend ethnic group	Change policies	Defend religion	Retaliate
	(1)	(2)	(3)	(4)
Direct YC recipient	−0.100 (0.103)	−0.121 (0.107)	−0.238* (0.107)	−0.165 (0.133)
Ethnicity: Peul	−0.094 (0.098)	−0.123* (0.050)	−0.071 (0.086)	0.025 (0.091)
Ethnicity: Tuareg	−0.020 (0.117)	−0.028 (0.060)	−0.307** (0.097)	0.157 (0.101)
Ethnicity: Zarma	−0.101 (0.096)	−0.070 (0.070)	−0.195* (0.087)	0.076 (0.085)
Autochthone	−0.103 (0.106)	0.004 (0.057)	−0.084 (0.055)	−0.046 (0.087)
Age	−0.067+ (0.037)	−0.045+ (0.027)	−0.044 (0.033)	0.036 (0.046)
Age squared	0.001+ (0.001)	0.001 (0.001)	0.001 (0.001)	−0.001 (0.001)
Female	−0.095* (0.046)	−0.040 (0.040)	0.026 (0.047)	−0.007 (0.042)
Education: Islamic	0.013 (0.075)	0.092 (0.101)	0.093 (0.091)	−0.025 (0.073)
Education: Primary	0.053 (0.069)	−0.047 (0.065)	−0.006 (0.067)	0.115* (0.056)
Education: Secondary/higher	0.093 (0.068)	0.074 (0.068)	−0.001 (0.067)	−0.017 (0.066)
ACLED events	−0.003 (0.003)	−0.006*** (0.002)	−0.006** (0.002)	0.001 (0.002)
ACLED deaths	0.008 (0.016)	0.019*** (0.005)	0.011 (0.011)	−0.025* (0.009)
N. of YC beneficiaries	0.000 (0.000)	0.001+ (0.000)	0.000+ (0.000)	−0.001+ (0.000)
Distance to a river	0.009 (0.006)	−0.007 (0.004)	0.000 (0.009)	−0.004 (0.008)
Distance to the border	−0.001 (0.001)	−0.001+ (0.001)	−0.001 (0.001)	−0.002 (0.001)
Commune seat	−0.056 (0.069)	−0.022 (0.064)	0.076 (0.057)	0.108 (0.067)
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Num.Obs.	1332	1327	1330	1334
R2	0.568	0.544	0.593	0.431

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 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 A6. Coefficient values for table 2

	Additive index	1st principal component
	(1)	(2)
Direct YC recipient	−0.695+ (0.385)	−0.298 (0.190)
Ethnicity: Peul	−0.436 (0.314)	−0.143 (0.124)
Ethnicity: Tuareg	−0.246 (0.224)	−0.162 (0.110)
Ethnicity: Zarma	−0.351+ (0.180)	−0.185* (0.086)
Autochthone	−0.262 (0.164)	−0.119 (0.081)
Age	−0.094 (0.099)	−0.074 (0.048)
Age squared	0.002 (0.002)	0.001 (0.001)
Female	−0.136 (0.129)	−0.065 (0.058)
Education: Islamic	0.144 (0.268)	0.072 (0.125)
Education: Primary	0.082 (0.199)	0.022 (0.085)
Education: Secondary/higher	0.120 (0.192)	0.044 (0.082)
ACLED events	−0.015* (0.006)	−0.007* (0.003)
ACLED deaths	0.017 (0.024)	0.012 (0.012)
N. of YC beneficiaries	0.000 (0.001)	0.000 (0.001)
Distance to a river	−0.007 (0.023)	0.002 (0.010)
Distance to the border	−0.006+ (0.003)	−0.003+ (0.002)
Commune seat	0.043 (0.182)	0.027 (0.089)
Region Fixed Effects	X	X
Enumerator Fixed Effects	X	X
Num.Obs.	1341	1313
R ²	0.609	0.653

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	1.031*** (0.208)	1.064*** (0.210)
Ethnicity: Peul	0.083 (0.084)	0.087 (0.086)
Ethnicity: Tuareg	0.285* (0.139)	0.248+ (0.127)
Ethnicity: Zarma	0.211 (0.135)	0.203 (0.125)
Autochthone	−0.025 (0.079)	−0.024 (0.079)
Age	−0.056 (0.050)	−0.045 (0.047)
Age squared	0.001 (0.001)	0.001 (0.001)
Female	0.076 (0.060)	0.096 (0.060)
Education: Islamic	−0.076 (0.093)	−0.071 (0.094)
Education: Primary	0.098 (0.074)	0.087 (0.073)
Education: Secondary/higher	−0.016 (0.070)	−0.033 (0.068)
ACLED events	−0.006** (0.002)	−0.007*** (0.002)
ACLED deaths	0.013 (0.010)	0.016+ (0.010)
N. of YC beneficiaries	0.000 (0.000)	0.000 (0.000)
Distance to a river	0.009 (0.007)	0.009 (0.007)
Distance to the border	0.000 (0.001)	0.000 (0.001)
Num.Obs.	1338	1338
Region Fixed Effects	X	X
NA	NA	NA
Enumerator Fixed Effects	X	X
R ₂	0.376	0.387

Note: This table shows coefficients on 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.7 Heterogenous treatment effects

Table A8 shows the effects of Youth Connect and IBMN treatments broken out by ethnicity.³ The effect of receiving Youth Connect and IBMN 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 Peulh (also known as Fulani), 9 percent as Touareg, and the remainder as 'other' ethnicities. Among Zarma respondents, participation in both IBMN 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. IBMN 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 village⁴. Table A9 shows no difference in treatment effects between autochthones and other respondents. Relatively few allochthones participated in the survey, so these subgroup effects may be underpowered. Because of the plethora of potentially moderating variables in table A9, here we only show the results using the additive index of support for the direct questions on support for violence.

We also 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.

³We 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.

⁴Conflicts 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 direct questions by ethnicity

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control	0.011 (0.123)	-0.071 (0.147)	-0.046 (0.080)	-0.007 (0.088)	0.082 (0.116)	0.139 (0.149)	0.070 (0.113)	-0.078 (0.146)
Youth Connect + IBMN	-0.124* (0.059)	-0.154* (0.065)	-0.106* (0.048)	-0.142*** (0.041)	-0.172** (0.055)	-0.203** (0.064)	-0.181** (0.065)	-0.236*** (0.055)
Youth Connect + IBMN * Peul	-0.005 (0.196)	-0.004 (0.189)	0.200* (0.091)	0.191* (0.086)	0.046 (0.154)	0.048 (0.144)	0.122 (0.192)	0.136 (0.183)
Pure control * Peul	-0.186 (0.208)	-0.076 (0.205)	0.175+ (0.101)	0.182+ (0.106)	-0.223 (0.170)	-0.156 (0.172)	-0.296 (0.197)	-0.289 (0.187)
Youth Connect + IBMN * Touareg	0.166 (0.160)	0.162 (0.171)	0.037 (0.141)	0.115 (0.122)	-0.007 (0.216)	0.070 (0.202)	0.418** (0.130)	0.459*** (0.125)
Pure control * Touareg	-0.318+ (0.191)	-0.214 (0.220)	0.092 (0.143)	0.158 (0.140)	-0.344 (0.217)	-0.183 (0.213)	0.056 (0.136)	0.179 (0.144)
Youth Connect + IBMN * Zarma	0.325* (0.126)	0.427*** (0.126)	0.191 (0.119)	0.258* (0.125)	0.198+ (0.106)	0.307** (0.096)	0.129 (0.141)	0.249+ (0.131)
Pure control * Zarma	0.227 (0.177)	0.373* (0.187)	0.265 (0.160)	0.262+ (0.155)	-0.087 (0.181)	-0.007 (0.210)	0.034 (0.156)	0.131 (0.174)
Direct YC recipient	-0.158+ (0.088)	-0.165 (0.101)	-0.231+ (0.124)	-0.209 (0.132)	-0.324** (0.103)	-0.327* (0.126)	-0.143 (0.121)	-0.195 (0.131)
Ethnicity: Peul	-0.020 (0.165)	-0.086 (0.152)	-0.200** (0.061)	-0.190** (0.064)	-0.067 (0.116)	-0.071 (0.116)	0.000 (0.163)	-0.009 (0.152)
Ethnicity: Tuareg	-0.083 (0.114)	-0.091 (0.126)	0.008 (0.070)	-0.037 (0.064)	-0.260+ (0.131)	-0.319* (0.125)	-0.190** (0.066)	-0.199** (0.071)
Ethnicity: Zarma	-0.253* (0.117)	-0.299* (0.126)	-0.173 (0.108)	-0.182+ (0.107)	-0.292** (0.093)	-0.309** (0.097)	-0.021 (0.110)	-0.068 (0.105)
Autochthone		-0.134 (0.097)		0.002 (0.053)		-0.054 (0.059)		-0.098 (0.073)
Age		-0.053 (0.040)		-0.044 (0.028)		-0.041 (0.034)		0.032 (0.041)
Age squared		0.001 (0.001)		0.001 (0.001)		0.001 (0.001)		-0.001 (0.001)
Female		-0.091* (0.040)		-0.082* (0.039)		0.007 (0.047)		0.000 (0.035)
Education: Islamic		-0.013 (0.072)		0.132 (0.088)		0.097 (0.077)		-0.034 (0.064)
Education: Primary		0.045 (0.064)		0.012 (0.062)		0.045 (0.063)		0.127* (0.051)
Education: Secondary/higher		0.026 (0.068)		0.061 (0.058)		0.037 (0.060)		-0.038 (0.058)
ACLED events		-0.004* (0.002)		-0.004* (0.002)		-0.007*** (0.001)		-0.003+ (0.002)
ACLED deaths		-0.001 (0.001)		0.002 (0.001)		0.000 (0.002)		0.001 (0.001)
N. of YC beneficiaries		0.000 (0.000)		0.000 (0.000)		0.001+ (0.000)		0.000 (0.000)
Distance to a river		0.005 (0.005)		-0.013*** (0.003)		-0.006 (0.006)		-0.005 (0.007)
Distance to the border		-0.002+ (0.001)		-0.002** (0.001)		-0.002 (0.001)		-0.001 (0.001)
Commune seat		-0.067 (0.050)		0.028 (0.055)		0.052 (0.057)		0.020 (0.065)
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	1714	1717	1710	1719	1712	1726	1719
R ₂	0.507	0.519	0.495	0.513	0.531	0.547	0.404	0.421

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.

Table A9. Heterogenous treatment effects for Youth Connect and IBMN by autochthony

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Pure control	0.020 (0.232)	0.056 (0.238)	0.116 (0.133)	0.189 (0.160)	-0.180 (0.118)	-0.102 (0.149)	-0.176 (0.171)	-0.204 (0.168)
Youth Connect + IBMN	0.057 (0.224)	0.073 (0.216)	0.033 (0.129)	0.002 (0.138)	-0.110 (0.114)	-0.070 (0.122)	-0.513*** (0.133)	-0.511*** (0.127)
Pure control * autochthone	0.010 (0.254)	-0.012 (0.244)	-0.051 (0.136)	-0.074 (0.136)	0.171 (0.122)	0.247 (0.138)	0.231 (0.177)	0.170 (0.166)
Youth Connect + IBMN * autochthone	-0.105 (0.225)	-0.120 (0.222)	-0.085 (0.131)	-0.065 (0.135)	-0.043 (0.112)	-0.056 (0.128)	0.427** (0.134)	0.399** (0.132)
Direct YC recipient	-0.175 (0.095)	-0.187 (0.106)	-0.257* (0.124)	-0.214 (0.130)	-0.294** (0.095)	-0.309* (0.120)	-0.153 (0.124)	-0.205 (0.133)
Ethnicity: Peul		-0.126 (0.072)		-0.083 (0.043)		-0.089 (0.071)		-0.120 (0.065)
Ethnicity: Tuareg		-0.077 (0.099)		0.022 (0.062)		-0.309*** (0.085)		0.044 (0.080)
Ethnicity: Zarma		-0.035 (0.088)		-0.041 (0.070)		-0.186* (0.086)		0.043 (0.056)
Autochthone	-0.030 (0.206)	-0.063 (0.197)	0.094 (0.099)	0.056 (0.087)	-0.073 (0.048)	-0.086 (0.060)	-0.234* (0.100)	-0.227* (0.088)
Age		-0.054 (0.041)		-0.044 (0.028)		-0.043 (0.034)		0.028 (0.040)
Age squared		0.001 (0.001)		0.001 (0.001)		0.001 (0.001)		-0.001 (0.001)
Female		-0.090* (0.042)		-0.080* (0.039)		0.003 (0.049)		0.002 (0.035)
Education: Islamic		-0.013 (0.072)		0.137 (0.089)		0.093 (0.077)		-0.029 (0.064)
Education: Primary		0.033 (0.061)		0.010 (0.062)		0.032 (0.061)		0.107* (0.049)
Education: Secondary/higher		0.013 (0.068)		0.062 (0.060)		0.023 (0.060)		-0.048 (0.059)
ACLED events		-0.003 (0.002)		-0.003* (0.001)		-0.005*** (0.001)		-0.003 (0.002)
ACLED deaths		-0.001 (0.001)		0.002 (0.001)		-0.001 (0.002)		0.000 (0.001)
N. of YC beneficiaries		0.000 (0.000)		0.000 (0.000)		0.001* (0.000)		0.000 (0.000)
Distance to a river		0.007 (0.005)		-0.011*** (0.003)		-0.005 (0.007)		-0.003 (0.007)
Distance to the border		-0.002 (0.001)		-0.001* (0.001)		-0.002 (0.001)		-0.001 (0.001)
Commune seat		-0.052 (0.056)		0.032 (0.058)		0.065 (0.061)		0.021 (0.070)
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	1714	1717	1710	1719	1712	1726	1719
R2	0.498	0.508	0.490	0.508	0.522	0.544	0.398	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.

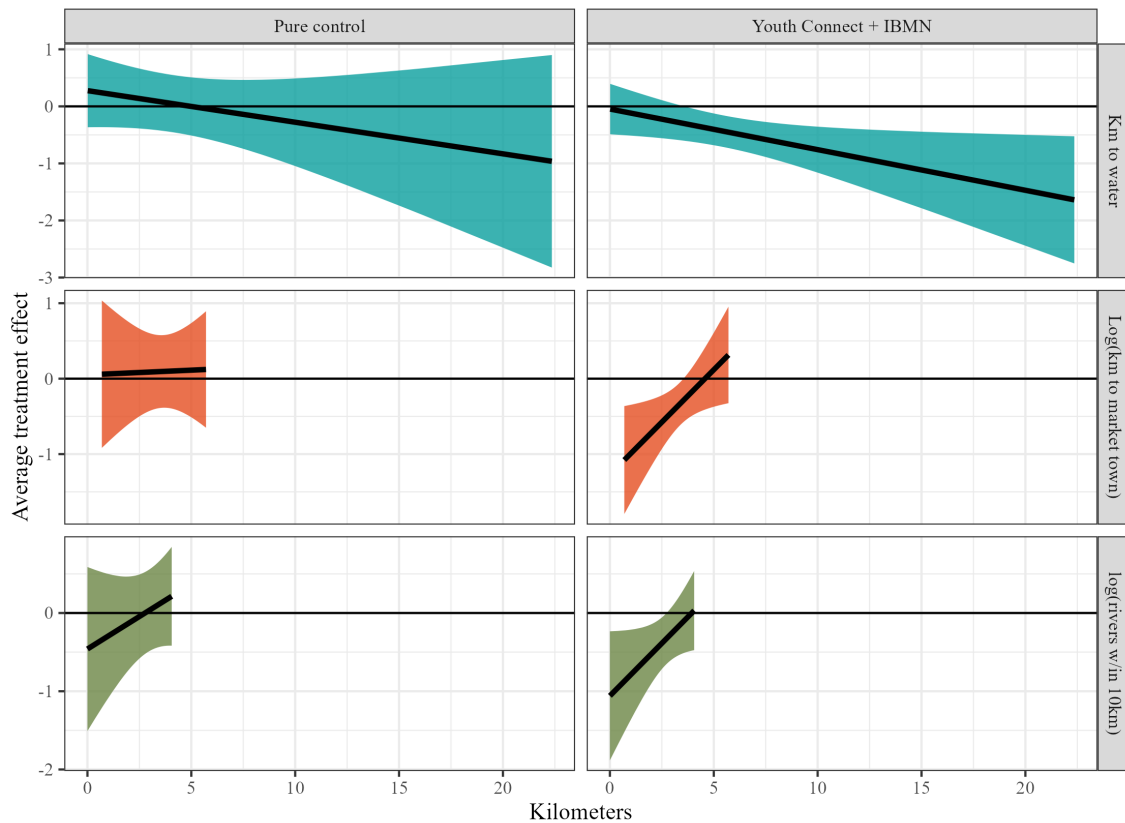
Table A10. Treatment effects are homogenous across intermediate outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
Pure control village	0.771 (0.903)	-0.339 (0.539)	-0.736 (0.524)	-0.444 (0.486)	-0.472 (0.705)	-0.161 (0.472)
Youth Connect + IBMN	0.039 (0.572)	-0.821+ (0.418)	-0.991* (0.408)	-0.668+ (0.395)	-0.997* (0.448)	-0.304 (0.338)
Youth Connect + IBMN * useful	-0.084 (0.140)					
Pure control * useful	-0.115 (0.218)					
Youth Connect + IBMN * opportunities		0.145 (0.121)				
Pure control * opportunities		0.170 (0.161)				
Youth Connect + IBMN * heard			0.192+ (0.113)			
Pure control * heard			0.307* (0.143)			
Youth Connect + IBMN * heard by authority				0.113 (0.128)		
Pure control * heard by authority				0.246 (0.175)		
Treatment village * active					0.169 (0.115)	
Pure control * active					0.176 (0.174)	
Youth Connect + IBMN * affect						-0.005 (0.110)
Pure control * affect						0.122 (0.149)
Direct YC recipient	-0.930* (0.454)	-0.949* (0.440)	-0.944* (0.416)	-0.964* (0.428)	-0.974* (0.437)	-0.928* (0.419)
Ethnicity: Peul	-0.512* (0.196)	-0.500** (0.190)	-0.510** (0.186)	-0.442* (0.187)	-0.474* (0.199)	-0.480* (0.198)
Ethnicity: Tuareg	-0.366* (0.179)	-0.323+ (0.178)	-0.324+ (0.173)	-0.291 (0.180)	-0.324+ (0.181)	-0.338+ (0.191)
Ethnicity: Zarma	-0.293 (0.182)	-0.222 (0.175)	-0.226 (0.179)	-0.229 (0.189)	-0.229 (0.184)	-0.235 (0.185)
Autochthone	-0.213 (0.172)	-0.175 (0.168)	-0.249 (0.160)	-0.242 (0.170)	-0.225 (0.164)	-0.204 (0.162)
Age	-0.097 (0.085)	-0.072 (0.084)	-0.077 (0.084)	-0.084 (0.087)	-0.106 (0.083)	-0.045 (0.083)
Age squared	0.002 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.002 (0.002)	0.001 (0.002)
Female	-0.204+ (0.113)	-0.185 (0.122)	-0.199+ (0.119)	-0.189 (0.121)	-0.200 (0.129)	-0.162 (0.121)
Education: Islamic	0.160 (0.226)	0.166 (0.232)	0.164 (0.232)	0.161 (0.224)	0.153 (0.225)	0.218 (0.235)
Education: Primary	0.160 (0.164)	0.190 (0.162)	0.196 (0.166)	0.168 (0.165)	0.188 (0.161)	0.184 (0.167)
Education: Secondary/higher	0.061 (0.172)	0.062 (0.178)	0.082 (0.179)	0.044 (0.175)	0.088 (0.174)	0.147 (0.173)
ACLEd events	-0.014** (0.005)	-0.015** (0.005)	-0.014** (0.005)	-0.015** (0.005)	-0.013* (0.005)	-0.013* (0.005)
ACLEd deaths	0.000 (0.004)	0.000 (0.004)	-0.001 (0.004)	-0.001 (0.004)	0.000 (0.004)	-0.001 (0.004)
N. of YC beneficiaries	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Distance to a river	-0.018 (0.018)	-0.019 (0.018)	-0.016 (0.018)	-0.017 (0.018)	-0.018 (0.018)	-0.015 (0.018)
Distance to the border	-0.007* (0.003)	-0.007* (0.003)	-0.007* (0.003)	-0.007* (0.003)	-0.007* (0.003)	-0.007* (0.003)
Commune seat	0.018 (0.178)	0.068 (0.173)	0.054 (0.168)	0.088 (0.165)	0.049 (0.176)	-0.017 (0.180)
Region Fixed Effects	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X
Num.Obs.	1721	1720	1716	1702	1686	1616
R2	0.594	0.592	0.596	0.596	0.593	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.

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, IBMN 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. Marginal effects of Youth Connect and IBMN by non-parametrically selected moderators



This figure displays the marginal effects for IBMN 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 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.⁵ We then re-estimate a series of OLS regressions in which we interact the variables indicated as important in the causal forests with the IBMN treatment indicator and the pure control group indicator.⁶ Figure A3 shows the marginal effect of Youth Connect and IBMN 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 IBMN treatment, Youth Connect by itself did not impact support for violence at any levels of these moderating variables. In contrast, figure A3 shows that adding IBMN 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. These results—along with the evidence for the positive economic effects of Youth Connect in appendix B—suggest that the economic support provided by Youth Connect was a necessary but insufficient condition for violence prevention.

⁵We define rivers as a permanent water source with more than a cubic meter of flow per year.

⁶We 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.

A.8 Social cohesion and youth agency

Table A11. IBMN training does not affect social cohesion outcomes

	Help across lines		Do not share values		Working together	
	(1)	(2)	(3)	(4)	(5)	(6)
Youth Connect + IBMN	0.064 (0.073)	0.015 (0.074)	0.065 (0.083)	0.008 (0.077)	0.065 (0.093)	0.026 (0.073)
Direct YC recipient	0.373* (0.181)	0.330+ (0.186)	0.497* (0.206)	0.554* (0.218)	0.417* (0.203)	0.332 (0.204)
Ethnicity: Peul		-0.017 (0.163)		0.027 (0.150)		0.038 (0.099)
Ethnicity: Tuareg		0.284+ (0.152)		0.009 (0.129)		-0.207 (0.138)
Ethnicity: Zarma		0.150 (0.135)		0.402*** (0.118)		0.039 (0.109)
Autochthone		0.278* (0.114)		0.177 (0.135)		0.374** (0.114)
Age		0.096* (0.039)		0.011 (0.056)		0.075 (0.064)
Age squared		-0.002* (0.001)		0.000 (0.001)		-0.001 (0.001)
Female		-0.035 (0.092)		-0.089 (0.076)		-0.009 (0.074)
Education: Islamic		-0.124 (0.103)		-0.156 (0.131)		-0.229* (0.098)
Education: Primary		-0.051 (0.085)		-0.133 (0.103)		-0.144+ (0.077)
Education: Secondary/higher		-0.074 (0.094)		-0.187+ (0.105)		-0.130+ (0.077)
ACLED events		0.001 (0.002)		-0.006 (0.004)		0.002 (0.002)
ACLED deaths		-0.001 (0.001)		0.009*** (0.003)		0.000 (0.002)
N. of YC beneficiaries		-0.001 (0.000)		0.001 (0.001)		-0.001 (0.001)
Distance to a river		-0.005 (0.006)		-0.016 (0.010)		-0.013 (0.008)
Distance to the border		0.000 (0.001)		0.000 (0.001)		-0.003 (0.002)
Commune seat		-0.026 (0.066)		-0.004 (0.097)		-0.073 (0.078)
Demographic Controls		X		X		X
Demographic Controls		X		X		X
Region Fixed Effects	X	X	X	X	X	X
Enumerator Fixed Effects	X	X	X	X	X	X
Num.Obs.	1731	1724	1727	1720	1729	1722
R ²	0.410	0.424	0.469	0.488	0.290	0.325
Mean of outcome	4.056	4.056	3.157	3.157	4.256	4.256

Note: Data are from the Youth Connect midline survey. Outcome variables are responses to “[t]o what extent do you agree or disagree with the following statements: [p]eople around here are willing to help their neighbors across ethnic lines; [p]eople in this area do not share the same values; and [p]eople in this area see the benefits of working together to achieve common goals. 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 A12. IBMN training does not affect youth agency outcomes

	Contribute		Peaceful members		Leadership		Valued	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Youth Connect + IBMN	0.022 (0.042)	0.008 (0.040)	0.127* (0.049)	0.123* (0.054)	0.145* (0.066)	0.141* (0.062)	−0.003 (0.045)	0.033 (0.040)
Direct YC recipient	0.264* (0.104)	0.228* (0.105)	0.403*** (0.102)	0.380*** (0.103)	0.219* (0.087)	0.199+ (0.101)	0.211* (0.088)	0.181 (0.111)
Ethnicity: Peul		0.028 (0.088)		0.041 (0.072)		0.010 (0.080)		0.056 (0.079)
Ethnicity: Tuareg		0.010 (0.071)		−0.028 (0.079)		−0.151 (0.113)		0.071 (0.081)
Ethnicity: Zarma		−0.005 (0.084)		0.011 (0.061)		−0.007 (0.085)		0.171* (0.077)
Autochthone		0.088 (0.095)		−0.006 (0.070)		0.167+ (0.091)		0.010 (0.061)
Age		−0.024 (0.031)		0.015 (0.028)		0.118*** (0.031)		0.033 (0.029)
Age squared		0.001 (0.001)		0.000 (0.001)		−0.002*** (0.001)		−0.001 (0.001)
Female		−0.049 (0.048)		0.051 (0.040)		−0.051 (0.050)		0.069+ (0.042)
Education: Islamic		−0.060 (0.056)		−0.007 (0.059)		0.046 (0.049)		0.108+ (0.058)
Education: Primary		0.056 (0.055)		−0.007 (0.060)		0.031 (0.063)		0.005 (0.059)
Education: Secondary/higher		0.046 (0.056)		0.047 (0.056)		0.051 (0.050)		0.092+ (0.054)
ACLED events		0.001 (0.001)		0.001 (0.002)		0.003* (0.001)		0.001 (0.001)
ACLED deaths		−0.003* (0.001)		0.000 (0.001)		−0.002* (0.001)		−0.002+ (0.001)
N. of YC beneficiaries		0.000 (0.000)		0.000 (0.000)		0.000 (0.000)		0.001** (0.000)
Distance to a river		−0.004 (0.004)		−0.004 (0.004)		−0.004 (0.005)		0.008+ (0.004)
Distance to the border		0.001 (0.001)		−0.001 (0.001)		0.002** (0.001)		0.001 (0.001)
Commune seat		−0.027 (0.048)		−0.045 (0.054)		0.041 (0.060)		−0.203*** (0.045)
Demographic Controls		X		X		X		X
Geographic 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
Num.Obs.	1731	1724	1732	1725	1731	1725	1730	1723
R ²	0.248	0.261	0.314	0.321	0.248	0.270	0.236	0.263
Mean of outcome	4.339	4.339	4.34	4.34	4.31	4.31	4.326	4.326

Note: Data are from the Youth Connect midline survey. Outcome variables are responses to “[t]o what extent do you agree or disagree with the following statements: [y]outh contribute positively to peace in our community; [y]outh from my ethnic group are peaceful members of the community; [y]outh should take more leadership roles in the community; and [y]outh are valued for their contributions by elders. 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.

B Pure control group

In addition to the 82 Youth Connect villages, we surveyed 36 villages that received neither Youth Connect nor IBMN training. We call this set of villages the pure control. However, it is important to note that assignment into these 36 villages was not random. IBMN assignment was randomized at the village level within the 14 communes selected to receive Youth Connect. However, another six communes were slated to receive Youth Connect in a second tranche of treatment. These six communes were assigned non-randomly, based on Mercy Corps' logistical considerations. Our pure control group consists of the 36 villages and communes centers within these six communes that were slated to receive Youth Connect.⁷

We do not include these 36 villages as part of our main analysis. However, they can nevertheless provide suggestive (not causal) evidence that Youth Connect by itself did increase economic outcomes, but failed to move the needle on support for violence.

B.1 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 IBMN, which makes sense because IBMN training had no economic components.

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 plus IBMN). A separate pool of enumerators surveyed the pure control villages in Maradi so it is difficult 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. Figure A1 shows that enumerators explain four percent of variation when it comes to economic outcomes, as opposed to 44 percent when it comes to support for violence. As such, we feel reasonably confident that tables B1 and B2 do not suffer from error induced by different survey enumerators. These regressions also differ from those in the main paper because we omit the wealth index from the

⁷In February 2025, the Youth Connect program was canceled as part of the US foreign assistance draw down.

set of control variables because we use it as an outcome variable in table B1.

B.2 Violence outcomes

Table B3 replicates table 1, but includes data from the pure control group. This table shows no difference between villages which received Youth Connect only (the reference level) and the pure control group. However, as we discuss above, the non-random selection and the enumerator effects mean we cannot attribute causal effects to this table. Nevertheless, it provides suggestive evidence that Youth Connect, by itself, was not sufficient to move the needle on support for violence.

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

	Wealth index		Employment	
	(1)	(2)	(3)	(4)
Youth Connect + IBMN	-0.226 (0.223)	-0.158 (0.132)	0.001 (0.031)	-0.015 (0.027)
Pure control	-0.717** (0.273)	-0.676* (0.267)	-0.198*** (0.056)	-0.218*** (0.063)
Direct YC recipient	0.247 (0.241)	0.264 (0.205)	-0.481*** (0.062)	-0.494*** (0.072)
Ethnicity: Peul		-0.029 (0.132)		-0.133** (0.044)
Ethnicity: Tuareg		-0.562** (0.179)		-0.034 (0.050)
Ethnicity: Zarma		-0.101 (0.183)		-0.119** (0.041)
Autochthone		-0.344+ (0.192)		0.010 (0.040)
Age		-0.111 (0.080)		-0.025 (0.025)
Age squared		0.002 (0.002)		0.001 (0.001)
Female		0.021 (0.091)		-0.144*** (0.028)
Education: Islamic		0.019 (0.127)		0.029 (0.050)
Education: Primary		0.331*** (0.096)		0.077+ (0.044)
Education: Secondary/higher		0.721*** (0.142)		0.160*** (0.046)
ACLED events		-0.007+ (0.004)		-0.001 (0.001)
ACLED deaths		0.002 (0.003)		-0.001 (0.001)
N. of YC beneficiaries		0.000 (0.001)		0.000+ (0.000)
Distance to a river		0.005 (0.013)		0.001 (0.003)
Distance to the border		0.000 (0.002)		-0.001+ (0.000)
Commune seat		0.618*** (0.124)		0.065+ (0.037)
Region Fixed Effects	X	X	X	X
Enumerator Fixed Effects	X	X	X	X
Mean of outcome	0.972	0.972	0.298	0.298
Num.Obs.	1734	1727	1734	1727
R ₂	0.134	0.279	0.243	0.325

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 + IBMN	-0.032 (0.032)	-0.040 (0.032)	-0.027 (0.033)	-0.055+ (0.029)	-0.052 (0.084)	-0.071 (0.080)
Pure control	-0.190*** (0.047)	-0.172** (0.061)	-0.102** (0.033)	-0.176*** (0.039)	-0.420* (0.162)	-0.699*** (0.177)
Direct YC recipient	0.285** (0.090)	0.265** (0.090)	0.558*** (0.076)	0.529*** (0.078)	0.220 (0.221)	0.257 (0.226)
Ethnicity: Peul		-0.081+ (0.042)		-0.004 (0.031)		-0.218+ (0.119)
Ethnicity: Tuareg		-0.065 (0.045)		0.123** (0.045)		-0.071 (0.155)
Ethnicity: Zarma		-0.089* (0.041)		0.093* (0.039)		-0.126 (0.123)
Autochthone		0.039 (0.037)		0.069* (0.033)		-0.219+ (0.131)
Age		0.051* (0.025)		0.027+ (0.016)		0.142* (0.070)
Age squared		-0.001 (0.001)		0.000 (0.000)		-0.003+ (0.002)
Female		-0.011 (0.022)		0.023 (0.024)		0.140+ (0.071)
Education: Islamic		0.073+ (0.038)		-0.033 (0.035)		0.026 (0.101)
Education: Primary		0.119*** (0.027)		0.035 (0.028)		0.077 (0.095)
Education: Secondary/higher		0.142*** (0.039)		0.026 (0.032)		0.286** (0.090)
ACLED events		-0.002+ (0.001)		0.001 (0.001)		-0.006* (0.002)
ACLED deaths		0.000 (0.001)		0.000 (0.001)		0.008* (0.003)
N. of YC beneficiaries		0.000 (0.000)		-0.001** (0.000)		-0.001** (0.000)
Distance to a river		-0.005* (0.002)		-0.006+ (0.003)		-0.004 (0.008)
Distance to the border		0.000 (0.001)		0.000 (0.000)		-0.001 (0.001)
Commune seat		0.004 (0.033)		0.038 (0.025)		0.209* (0.086)
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	3.756	3.756
Num.Obs.	1733	1726	1733	1726	1729	1722
R2	0.306	0.330	0.467	0.482	0.319	0.363

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 B3. Youth Connect does not reduce support for violence absent IBMN training

	Defend ethnic group		Change policies		Defend religion		Retaliate	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Youth Connect + IBMN	−0.040 (0.055)	−0.035 (0.054)	−0.042 (0.051)	−0.052 (0.050)	−0.153** (0.050)	−0.121* (0.051)	−0.130* (0.053)	−0.154** (0.053)
Pure control	0.030 (0.073)	0.054 (0.106)	0.073 (0.082)	0.141 (0.109)	−0.034 (0.096)	0.128 (0.121)	0.019 (0.079)	−0.080 (0.110)
Direct YC recipient	−0.177+ (0.095)	−0.192+ (0.104)	−0.258* (0.125)	−0.220+ (0.129)	−0.297** (0.094)	−0.325** (0.119)	−0.147 (0.123)	−0.196 (0.132)
Ethnicity: Peul		−0.128+ (0.072)		−0.083+ (0.044)		−0.093 (0.066)		−0.115+ (0.067)
Ethnicity: Tuareg		−0.068 (0.098)		0.032 (0.063)		−0.277** (0.085)		0.023 (0.077)
Ethnicity: Zarma		−0.032 (0.091)		−0.042 (0.071)		−0.167+ (0.087)		0.041 (0.058)
Autochthone		−0.101 (0.100)		0.024 (0.053)		−0.033 (0.054)		−0.064 (0.075)
Age		−0.053 (0.041)		−0.042 (0.029)		−0.038 (0.035)		0.029 (0.042)
Age squared		0.001 (0.001)		0.001 (0.001)		0.001 (0.001)		−0.001 (0.001)
Female		−0.089* (0.041)		−0.080* (0.038)		0.003 (0.048)		−0.002 (0.036)
Education: Islamic		−0.012 (0.072)		0.136 (0.090)		0.096 (0.078)		−0.030 (0.064)
Education: Primary		0.031 (0.061)		0.003 (0.063)		0.024 (0.061)		0.113* (0.051)
Education: Secondary/higher		0.009 (0.068)		0.048 (0.061)		0.008 (0.062)		−0.039 (0.062)
ACLED events		−0.002 (0.002)		−0.003* (0.001)		−0.005*** (0.001)		−0.003 (0.002)
ACLED deaths		−0.001 (0.001)		0.001 (0.001)		−0.001 (0.002)		0.001 (0.001)
N. of YC beneficiaries		0.000 (0.000)		0.000 (0.000)		0.001* (0.000)		−0.001 (0.000)
Distance to a river		0.007 (0.006)		−0.012*** (0.003)		−0.004 (0.007)		−0.002 (0.007)
Distance to the border		−0.002+ (0.001)		−0.001* (0.001)		−0.002+ (0.001)		−0.001 (0.001)
Commune seat		−0.060 (0.057)		0.020 (0.059)		0.040 (0.057)		0.037 (0.069)
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	1714	1717	1710	1719	1712	1726	1719
R2	0.497	0.507	0.490	0.507	0.521	0.540	0.392	0.407

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. 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 implemented a baseline survey in advance of both Youth Connect and IBMN training. Table C1 shows the overlap between villages in which the baseline and midline took place. 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 us from adding enumerator fixed effects. Because each period had distinct enumerators, enumerator fixed effects are collinear with the period fixed effects. However, all regressions include controls for each enumerator’s median duration, which we take as a proxy measure for enumerator quality. We also include each enumerator’s 10th and 90th percentile of survey 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 + IBMN	0.286 (0.188)	0.292 (0.189)	0.089 (0.175)	0.125 (0.175)	0.039 (0.243)	0.079 (0.224)
D-in-D: Pure control	0.449* (0.207)	0.452* (0.204)	0.508* (0.208)	0.474* (0.218)	0.359 (0.281)	0.347 (0.269)
Ethnicity: Peul		0.058 (0.101)		0.091 (0.148)		−0.144 (0.202)
Ethnicity: Tuareg		0.059 (0.083)		−0.187 (0.180)		−0.486** (0.164)
Ethnicity: Zarma		0.142+ (0.083)		0.066 (0.220)		−0.378* (0.144)
Age		0.046 (0.040)		0.011 (0.065)		0.013 (0.051)
Age squared		−0.001 (0.001)		0.000 (0.001)		0.000 (0.001)
Wealth index		−0.014 (0.026)		0.002 (0.037)		−0.019 (0.033)
Female		0.015 (0.064)		0.129 (0.092)		0.137* (0.060)
Enumerator: 10th percentile	0.029** (0.011)	0.030** (0.011)	0.026** (0.008)	0.026** (0.009)	0.072*** (0.014)	0.072*** (0.014)
Enumerator: 50th percentile	0.002 (0.004)	0.002 (0.004)	−0.006 (0.008)	−0.006 (0.009)	−0.040** (0.011)	−0.041** (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
R ²	0.174	0.179	0.120	0.131	0.221	0.241

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 + IBMN	0.284+ (0.160)	0.327* (0.141)	0.856+ (0.497)	0.977* (0.482)
D-in-D: Pure control	0.374* (0.167)	0.331* (0.153)	1.679** (0.609)	1.608* (0.621)
Ethnicity: Peul		−0.063 (0.166)		−0.065 (0.345)
Ethnicity: Tuareg		−0.088 (0.150)		−0.689* (0.271)
Ethnicity: Zarma		−0.071 (0.145)		−0.290 (0.299)
Age		−0.027 (0.066)		0.052 (0.119)
Age squared		0.000 (0.001)		−0.002 (0.003)
Wealth index		−0.056+ (0.029)		−0.083 (0.078)
Female		0.132 (0.079)		0.460* (0.197)
Enumerator: 10th percentile	0.014+ (0.008)	0.016* (0.007)	0.146*** (0.023)	0.148*** (0.025)
Enumerator: 50th percentile	−0.025*** (0.006)	−0.025*** (0.005)	−0.070** (0.023)	−0.072** (0.023)
Enumerator: 90th percentile	0.000 (0.000)	0.000 (0.000)	0.001 (0.000)	0.001+ (0.000)
Village + wave FEs	X	X	X	X
Num.Obs.	1595	1594	1602	1601
R ₂	0.146	0.172	0.151	0.169

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/IBMN trainings. However, these regressions are underpowered and we do not include them in the main paper.

Table D1. After the training, IBMN 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 IBMN 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.

D Pre-post questionnaire of IBMN participants

In addition to the midline survey, we administered a short questionnaire to the 183 direct participants in IBMN 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. We posed 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.” This lack of change is probably due to floor effects: respondents were already unlikely to agree with this statement before the training, so there was minimal movement possible after treatment.⁸ These results may also be the result of social desirability bias, so we do not rely on them.

⁸These 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.

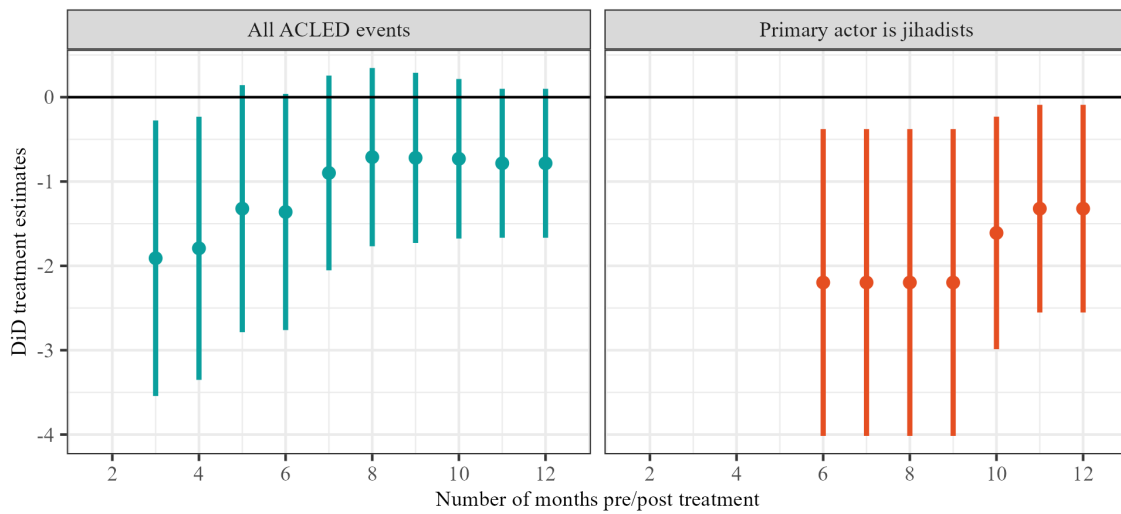
The broader IBMN randomized control trial followed a training of trainers model. A select few respondents, titled "young leaders," participated in these direct IBMN trainings which took place in the regional capitals. They then disseminated these results to the remainder of their communities through village assemblies. In that sense, these results serve as a "first-stage" results: they show that participants did indeed take-up the lessons of IBMN. This result may not have broad implications by itself, but it is an important confirmatory test for the remainder of the project. Had youth leaders not absorbed the lessons of IBMN, 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 villages where the outcome differs pre and post IBMN treatment.

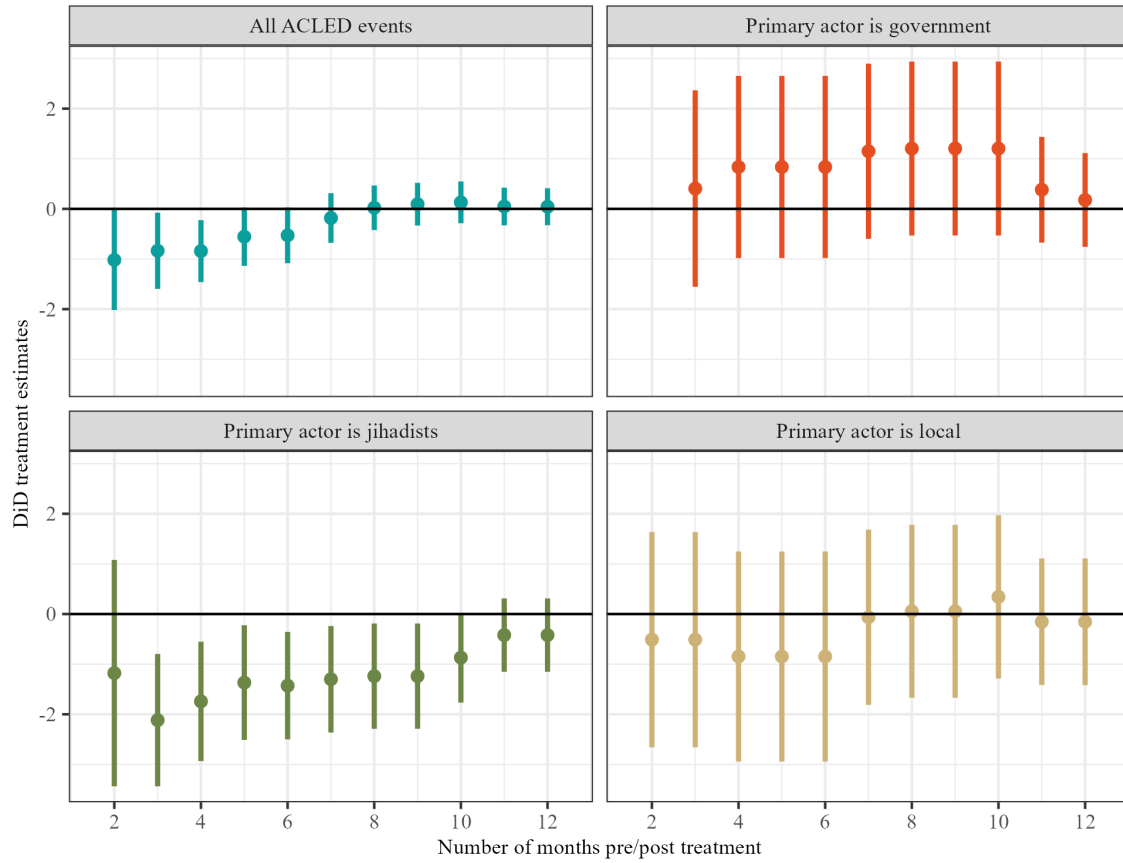
A variety of critiques exist of ACLED and similar datasets—their coverage is uneven, and they can also exclude a variety of conflict events (Demarest and Langer 2018; Eck 2012). However, for these data issues to bias our analysis, it's not enough for ACLED to be missing events: the missingness of events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

Figure E1. Difference-in-difference coefficients are robust to 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. The horizontal axis shows the number of months pre and post IBMN treatment included in the DiD window; the vertical axis shows coefficients of interest from a DiD regression, with bars showing confidence intervals. 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.

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. The horizontal axis shows the number of months pre and post IBMN treatment included in the DiD window; the vertical axis shows coefficients of interest from a DiD regression, with bars showing confidence intervals. 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, as with the main results in the paper.

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 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 IBMN training however, we follow a randomized rollout design: villages that did not receive IBMN before the (midline) follow-up survey will receive it after. The sequencing of receiving IBMN treatment was due to Mercy Corps’s capacity to implement these trainings—villages would have been delayed in receiving IBMN treatment anyways.

The second ethical consideration to this research has to do with the age of respondents: 15 to 34. 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.

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 IBMN 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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