

Shoot a Stranger, Save a Neighbor: Civilian & Combatant Networks Under Fire

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Abstract

During episodes of identity-targeted violence, what leads people to aid and protect the persecuted? Prior research on rescue during the Holocaust and Rwandan genocide has pointed to the importance of social networks, but the mechanisms remain unexplored, and the overall frequency of these behaviors, unknown. After surveying survivors of the 1992-5 Bosnian conflict—the first nationwide survey on wartime rescue in a post-conflict state—I find that cross-group assistance was both widespread and strongly correlated with respondents having more cross-group ties. Yet surprisingly, the strength of those ties does not appear to have had much impact. Most helpers, it seems, were willing to help not only close friends but even friends-of-friends and acquaintances. Drawing on 160 new interviews with helpers and recipients, I theorize how networks activate cross-group social capital to channel aid to those in need. Together, these sources suggest that prewar intergroup contact can increase levels of cross-group assistance during genocide, state repression, and civil war.

Keywords: Networks, Intergroup Contact, Social Capital, Bosnia, Civil War, Genocide, Rescue

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1 Introduction

Sarajevo, 1992: Two Serb militiamen burst into a tall, drab apartment block, grenades in hand, looking for Muslims to kill. In a multiethnic building where a surname can be a death sentence, Dino’s parents have not had time to hide their flat’s nameplate. As the soldiers batter down the door, screaming “Open up, Turks!” the boy’s mother bursts into tears. Just then, an old neighbor living upstairs, a WWII veteran of Tito’s partisans, appears in the stairwell, wearing pajamas. As he steps into the hall, he embraces the soldiers in a great bear hug. “We are all Yugoslavs!” he cries, and in a waterfall of warmth and forgotten pre-war patriotism, ushers the militiamen out of the building.¹ How did this life-saving act come about? First, the helper was willing, perhaps due to a sense of pan-ethnic Yugoslav identity or his history of fighting Nazis, Ushtashe, and Chetniks in WWII. Second, he had the capacity to intervene—his age, gender, and manner of speech may have evoked enough respect from the soldiers that they were willing to heed him. Finally, a social network of neighbor relationships connected him to a family who needed his help, placing him in the right place at the right time and perhaps spurring him to risk his life on their behalf. In this article, I develop a theoretical framework to explain cross-group assistance which incorporates all of these causes through the lens of cross-group social capital—the capacity to mobilize assistance from willing and capable outgroup members via a social network—and demonstrate its relevance empirically with survey, interview, and archival evidence from Bosnia.

With a few exceptions (Gross 1994; Monroe 1996), political scientists have only recently begun to devote attention to cross-group assistance, building on earlier work by psychologists (e.g., Oliner and Oliner 1988; Staub 1993), sociologists (e.g., Tec 1986), and historians (e.g., Paulsson 2002). Though this new work is sometimes more methodologically sophisticated, its scope remains limited to acts of lifesaving rescue in the Holocaust (e.g., Braun 2019; Finkel 2017), Rwanda (e.g., Brown 2014; Fox and Brehm 2018; Fujii 2011; Luft 2015*b*), and occasionally other genocides (e.g., Sémelin, Andrieu and Gensburger 2014). I break new ground by considering a broader array of helping behaviors and a wider range of violent contexts. While previous scholarship has focused exclusively on rescue, I define assistance as any dyadic, cleavage-defying behavior aimed at helping the recipient without net material gain. Examples include providing housing, food, or employment; transporting persecutees to safety; and protecting their property after they depart. By moving beyond genocide into contexts where being a minority is dangerous but not always lethal, a large array of previously overlooked helping behaviors burst into the spotlight. Furthermore, the proximity of friendly territory for minorities to flee to, in contrast to the Holocaust, over a long duration, in contrast to the Rwandan genocide, greatly expands opportunities for assistance.

¹Interview by the author in Bosnia & Herzegovina (hereafter, Bosnia) in 2008-9. Names have been changed except where requested.

This article sets out to address three questions about cross-group assistance. First, in a context where helping is less difficult than under genocide and where prewar intergroup contact was widespread, how common is cross-group assistance? To date, no study has produced even a ballpark estimate of the frequency of rescue—let alone other helping behaviors—during episodes of group-targeted violence. Based on a nationwide, door-to-door, probability survey, I show that roughly one in three Bosnians either gave or received wartime assistance from a member of another ethnic group. This estimate is supported qualitatively both by the ease with which I encountered stories of such acts during my fieldwork and in the abundant testimony collected during and after the war by local researchers. Second, are individuals with more cross-group ties prewar more likely to engage in assistance? Through multiple measures of prewar networks, I find a strong correlation between cross-group ties and both giving and receiving wartime help. To address concerns about memory and social desirability bias, I show in a robustness check that municipalities with denser cross-group kinship networks had higher rates of residents joining an outgroup’s army (a.k.a. ethnic defection), a non-dyadic form of cross-group assistance. Third, are stronger cross-group ties more predictive of assistance than weak ones? Using both survey and interview data, I show that, surprisingly, the answer appears to be no, at least for helpers, though it may matter for recipients. The relationship between prewar ties and the likelihood of providing assistance is not moderated by tie strength and acts of assistance themselves do not appear to come predominantly from strong ties. In fact, interviewees frequently report getting assistance from people they barely knew or who were friends-of-friends. On the other hand, help from total strangers was rare. Having a social network connection—even an indirect one—was clearly important, even if the strength of that connection was not.

This work not only expands the interdisciplinary genocide rescuer literature, but contributes to political science in three important ways. First, it brings to light a broader array of cleavage-defying behaviors than has previously received attention in the civil war literature. While considerable work has highlighted how people in conflict zones may develop new group identities in wartime (Kalyvas 2006; Wood 2003), join outgroup armies as combatants (Kalyvas 2008; Lyall 2010; Staniland 2012a), or switch sides during a war (McLaughlin 2020), little work has focused on people who choose to help an outgroup without leaving the ingroup. Second, I contribute to the networks and conflict literature, arguing that networks not only harden group boundaries by mobilizing combatants (McDoom 2021; Shesterinina 2021) and their supporters (Parkinson 2013) but transcend them by mobilizing assistance across ethnic lines. While prior scholarship has demonstrated the power of networks to break down ethnic boundaries in peacetime (Wimmer 2013), we have only limited evidence to show this process is extant—let alone widespread—once violence begins. Finally, this article complements the recent blossoming of experimental work on intergroup contact theory in conflict settings (e.g., Mousa 2020; Scacco and Warren 2018). Rather than examining whether newly formed ties

affect behavior, as interventions are well setup to do, this article traces the trajectory of longstanding, prewar ties into the violent period to determine whether their power to provide assistance fades or endures. Though such as design cannot make causal claims, it adds external validity to this valuable new work, suggesting that the mechanisms they find hold relevance not only in post-conflict and low-level violent contexts, but in an atmosphere of ongoing civil war, mass killing, and ethnic cleansing.

The rest of the paper proceeds as follows: In Section 2, I demonstrate how a mixed-methods investigation of cross-group help during a civil war fills an important gap in the conflict literature. I then articulate my theory for how capacity, willingness, and networks together create cross-group social capital that can be activated in wartime for cross-group assistance. Section 3 presents the data sources and variables. In section 4, I present my quantitative findings and expand on them in Section 5 with excerpts from my interviews. Section 6 concludes.

2 Networks, Intergroup Contact, and Social Capital

2.1 Networks in the Line of Fire

Much of the micro-level scholarship on civil conflict in recent years points to the importance of social networks in motivating civilian and combatant behavior (Gohdes 2015; Larson 2016; Lewis 2013; Marks 2019; Metternich et al. 2013; Perliger and Pedahzur 2011; Petersen 2001; Scacco 2008; Staniland 2012*b*; Weidmann 2015; Wood 2008; Zech and Gabbay 2016). When it comes to group-targeted violence, their findings are largely pessimistic. McDoom (2014*a*) finds that Rwandans with more friends were more likely to kill during the Rwandan genocide. Balcells (2017) and Kalyvas (2006) show that interpersonal ties carry not only friendship but also rivalry and grievance, leading to betrayal and revenge under the cover of war. At the community level, Larson and Lewis (2017) find that a handful of ties between two ethnic groups in one village can actually reduce cooperation and disrupt intergroup peace. Thus, there is ample reason to suppose that networks harm rather than help persecutees during episodes of identity-targeted violence.

However, harm and help are not mutually exclusive. In the words of one Bosnian Muslim man I interviewed, there were combatants in Bosnia "...who maybe went to the point of being war criminals but were very good to their neighborhood". Much of the literature on the Holocaust and Rwandan genocide suggests that networks may play a palliative role in group-targeted violence, even if they fail to stem the bloodshed. In a decade-long study of nearly 1000 confirmed rescuers, Oliner and Oliner (1988) found that rescuers were twice as likely as non-rescuers to have Jewish friends before the war. In a statistical analysis of the fates of 7665 Jews randomly selected from a Nazi registry in Amsterdam, Tammes (2007) concludes,

“Survival correlates most strongly with having close social ties with non-Jews.” Finkel (2017) finds that Jews living in cities that had been more integrated before WWII were more likely to turn to Christian contacts for help—an ultimately met with greater success—whiles Jews in highly segregated cities relied on fellow Jews in positions of limited authority whose power to protect proved ephemeral. Braun (2019) finds that Dutch Jews living near a minority denomination church were more likely to survive, which he attributes both to cross-group ties between Jews and their Christian-minority neighbors and dense networks of ingroup ties within these minority church communities that facilitated collective action. Thus, even in places where local outgroup civilians willing join in the persecution, networks can simultaneously contribute to assistance, as Fujii (2011) artfully demonstrates in an ethnographic study of two communities in Rwanda. Thus, we have good reason to expect that networks will prove instrumental in facilitating cross-group assistance in wartime, even if they also produce violence. In the remainder of this section, I extend this literature by exploring why and how networks produce wartime assistance.

2.2 Intergroup Contact and Expectations of Assistance Frequency

Why might networks be important? Beginning with Allport (1954)’s seminal book *The Nature of Prejudice* and earlier papers by Williams Jr. (1947) and Smith (1943), an extensive social science literature suggests that intergroup contact can reduce discrimination and prejudice (Pettigrew and Tropp 2006). In a recent meta-review, however, Paluck, Green and Green (2018), reveal that very little of this research has focused on racial and ethnic divisions among adults, the dimension of intergroup contact most directly relevant to civil wars and contentious politics. They point out that Allport’s original conditions for intergroup contact to be productive—close cooperation between the groups, common goals, equal status within the space of interaction, and support for authorities, institutions, or customs—may still be essential, despite earlier studies’ attempts to discard them. In other words, in order for intergroup contact to lead to positive outcomes, the quality of the interactions matters.

Indeed, close contact between identity groups is neither inherently prosocial nor antagonistic; encounters with outgroup members can break down or reinforce stereotypes, foster understanding and empathy or generate rivalry and competition. Kopstein and Wittenberg (2011) find there Poles were more likely to participate in the murders of Jews during WWII in areas were Jews and Poles had been more politically polarized during the interwar period, with less killing in areas where the state had more successfully integrated the two communities. Likewise, Balcells (2017) argues that left-learning and right-leaning neighbors were more likely to betray each other in the Spanish civil war in towns where prewar political rivalries has been most intense, and McDoom (2014b) finds that the onset of violence was delayed in parts of Rwanda where

Tutsis and Hutus were more socially integrated. That said, although more social integration and cooperation between groups may reduce or delay locally driven violence, it will not necessarily reduce the gross body count. External actors such as genocidal militias may deliberately target areas of high cooperation if they see them as an ideological threat to their goals of ethnic purity. Furthermore, in ethnic conflicts, external actors may not need to rely on local informants in order to determine which citizens are members of the outgroup; skin tone, facial features, dress, language, accent, or—in Bosnia’s case—name and neighborhood—may be sufficient to mark one for death. Assistance, on the other hand, is an overwhelmingly local phenomenon. Thus, we should expect an even stronger relationship between meaningful intergroup contact and assistance than we see between intergroup contact and casualties.

I maintain that cross-group social networks, as opposed to mere geographic mixing of identity groups, present clear evidence of Allport’s sort of intergroup contact in action. Yugoslavia, which Bosnia was part of until the outbreak of the 1992 war, promoted common goals, equal status, and close cooperation between the state’s three major ethnoreligious groups (Muslims, Serbs, and Croats),² underscored by its slogan “Brotherhood and Unity” (Woodward 1995). The result was widespread friendship and intermarriage, as I show descriptively based on survey and census data in Appendix C, echoing the prior literature (e.g., Gagnon 2004; Mueller 2000; Smits 2010). What remains to be shown is whether intergroup contact, even under ideal conditions, will be sufficient to promote assistance once fighting begins. Bergholz (2016) and Balcells (2017) suggest that once fighting gets underway, endogenous cycles of revenge may take hold. Fujii (2011) argues that Rwandan Hutus became involved in killing their neighbors as their prewar social ties were supplanted by new ones generated through the conflict, echoing Wood (2008)’s demonstration of how social networks can be transformed in wartime. Straus (2014) goes a step further, showing that Hutus who initially tried to protect Tutsi neighbors eventually turned to persecuting them as the danger of defying the social pressure became too great. I hypothesize, therefore, that while we should expect to see widespread assistance in Bosnia due to its high levels of Allportian social contact prewar, we should expect riskier forms of assistance to be less frequent.

Hypothesis 1: Cross-group assistance—particularly less risky forms of assistance—will be widespread in wartime Bosnia.

Note both that the hypothesis is descriptive and that we do not have prior points of comparison.

²I use these names throughout this article, because they are terms most often preferred by my interviewees. In Bosnia, Croats and Serbs are sometimes referred to by their nominal confessions—Catholic and Orthodox respectively—even though some of their members were entirely irreligious. Bosnian Muslims later adopted an ethnonym—Bosniaks—during the war.

2.3 A Theory of Cross-Group Social Capital

Moving from the societal to the individual level, what sorts of individuals should be most likely to engage in assistance? The genocide rescuer literature identifies a myriad of causal factors leading to cross-group assistance, which I group into three categories: capacity, willingness, and networks. First, [Gushee \(1993\)](#) and [Bjørnskov \(2015\)](#) cite biographical factors such as assets, abilities, and social status which affect an individual’s *capacity* to provide assistance. As [McAdam \(1986\)](#) discusses in his study of white civil rights workers during Freedom Summer, individuals who have fewer personal constraints on their time and responsibilities are more likely to risk their lives to secure voting rights for a racial outgroup. Second, Holocaust scholars such as [Fogelman \(2011\)](#), [Fagin-Jones and Midlarsky \(2007\)](#), and [Monroe, Barton and Klingemann \(1990\)](#) emphasize how individuals’ *willingness* to risk their lives to save others stems from personality and attitudinal traits including a universalist worldview, kindness, or bravery. [Tec \(1986\)](#), for instance, examines how individuals’ religiosity affected how willing they were to shelter Jews. Finally, Holocaust and Rwanda scholars from several disciplines emphasize the importance of both helpers’ and recipients’ social ties (e.g., [Fox and Brehm 2018](#); [Luft 2015a](#); [Paulsson 2002](#); [Tec 1986](#)).

To integrate these three sets of causes into a comprehensive framework, I propose a theory of cross-group social capital. I define an individual’s *social capital* as their ability to get help from others.³ To make the definition concrete, let us consider a woman attempting to borrow a lawnmower and the factors that might affect her success. First, how many people does she know well enough to ask for such a favor? This aspect of social capital is determined by the woman’s position in her social network, and largely a function of her *degree* (number of social ties). Second, do any of her friends own lawnmowers? This aspect of social capital reflects the capacity of others in her community to provide the assistance she seeks. Third, are any of them willing to share their lawnmowers? The answer to this question is shaped by each individual’s inherent willingness to share. It may also be influenced by *tie strength*—the strength of the relationship between the woman and the neighbor she is asking.

Despite its ability to bring people together and enhance their access to emotional support, goods, and services, social capital can have extremely deleterious effects. For instance, [Berman \(1997\)](#), [McDoom \(2014a\)](#), and [Satyanath, Voigtländer and Voth \(2017\)](#) document how social capital can lead people to support genocidal regimes or even join in the killing. [Putnam \(2000\)](#) draws a distinction between the “bonding” and “bridging” dimensions of social capital: the former bonds together members of the same homogeneous cluster, the latter bridges the gaps between clusters. If a community is sufficiently integrated before the war—as

³Other definitions of social capital which focus on the community level incorporate norms, trust, and civil society organizations. While these variables may well affect cross-group assistance, my focus here is variation at the individual level, holding community-level social capital constant. See [Coleman \(1988\)](#); [Lin \(1999\)](#) and [Putnam \(2000\)](#) for further elaboration.

many urban parts of Bosnia were (Burg and Shoup 1999)—members of different ethnic groups are quite likely to be found in the same social circle, the term “bridging” makes little sense. Ties that are bridging in a structural sense may not be bridging in terms of social cleavages, and visa versa. Therefore, I employ the term *cross-group social capital* to specify one’s ability to muster assistance across salient social divisions and not necessarily from outside one’s tight-knit community, family, or circle of friends. The central argument of this paper is that prewar cross-group social capital predicts wartime acts of cross-group assistance.

While an ideal study would go back in time and measure not only how many outgroup friends each persecutee had before the war, but also how willing and able those friends were to assist, data limitations force us to focus on the network alone. All things being equal, persecutees with a higher outgroup degree (more cross-group ties) should be more likely to receive assistance. Members of the dominant group with higher outgroup degree should likewise be more likely to assist, not because it increases their social capital per but because it connects them to more people looking for assistance.

Hypothesis 2: The more cross-group ties an individual has prewar, the more likely they will be to give or receive assistance.

From the perspective of the dominant group, slightly more data is available. We should expect individuals with greater wealth or with key resources such as a house (for hiding people) or a car (for transporting people) to be more likely to provide assistance due to capacity. Individuals who are more sociable and agreeable or who were raised with tolerant values should be more likely to provide assistance due to willingness. While these variables are not the focus of the study and are generally treated as controls, I address them in Appendix F and find they generally match these expectations.

As noted above, tie strength may also influence an individual’s willingness to help a particular persecutee. In the presence of great danger, cost, inconvenience, or ingroup pressure, a would-be helper may limit their help to those they feel closest to. An individual in need of help, meanwhile, may not feel comfortable putting their life in someone’s hands unless they already have a high level of trust. Furthermore, tie strength may encompass not only the emotional content of a relationship but the frequency of interaction. Those who interact more often may be more likely to find one another in the right place at the right time. The persistence of these ties after the start of the war may also be easier to observe. A would-be helper may not know if his neighbors are still on speaking terms with their outgroup friends, but he is likely to know if they are still living with outgroup spouses, parents, and children.

A positive relationship between tie strength and assistance is by no means guaranteed, however. While strong ties might influence an outgroup member’s willingness to provide assistance, they may be negatively correlated with capacity. Varshney (2003) finds that formal, institutional ties between Hindu and Muslim

local leaders prove more robust in times of religious tension and crises than the informal friendships and quotidian ties we might naturally expect to be more emotionally strong or meaningful. [Granovetter \(1973\)](#) suggest that alters with weak ties may be more effective than those with strong ties at help people find jobs, because weak ties are more likely to be bridging different social clusters and thus provide job-seekers access to fresh information about jobs they are not already aware of. It also possible that tie strength may matter more for people seeking assistance than for those providing it. A recipient on the other hand may only trust their closest alters enough to put their lives in their hands or make a costly request. While a helper may face danger as well, they are unlikely risk of betrayal from the person they are helping. In contrast, a persecutee who approaches a member of the dominant group for help may be uncertain if this person will persecute them unless they have a sufficiently strong tie. Thus, a reasonable starting point would be to hypothesize one's ability assistance through one's social network will be moderated somehow by tie strength.

Hypothesis 3: Stronger cross-group ties will be more predictive of assistance than weaker ones.

2.4 Scope Conditions and Case Selection

One of the primary aims of this study is to expand the scope of the genocide rescuer literature, not only to non-lifesaving forms of assistance, but to new contexts. My theory of cross-group social capital implies the existence of two or more groups, but it does not specify what types of groups they must be. For there to be a clear ingroup and outgroup, residents of a local community must have a shared conception of who is a member and who is not, whether that distinction is based on religion, sect, ethnicity, race, class, language, region, or something else entirely. These identities may, at times, be fluid, but once the violence begins, they must be sufficiently “sticky” that persecuted individuals cannot easily shed them. Ideological identities can become as hard to escape as ethnic ones, as [Balcells \(2017\)](#) makes clear in her examination of rivalry and revenge during the Spanish Civil War. The theory of cross-group social capital does not require the individuals to be socialized into separate clusters. Therefore, it is sufficiently flexible so as to not require a deep societal cleavage before the conflict begins. All that matters is for individuals to be targeted based on a hard-to-remove identity and for there to be ties between members of the two groups. The violence itself can take the form of a riot or pogrom, civil war, identity-based state repression, ethnic cleansing, or genocide. Purely interstate wars with domestic armed opposition generally fall outside the scope of this theory due to the lack of ties between groups living in different areas, but there are exceptions. Within domestic conflicts, the theory would be inapplicable in regions where the entire population is being targeted and there is are not local members of the dominant group to provide assistance.

The 1992-5 conflict in Bosnia & Herzegovina provides fertile ground in which to test this theory. The

conflict was organized along ethnoreligious lines, yet the three groups—Muslims, Croats, and Serbs—had a high rate of intermarriage and mixed neighborhoods before the war. The four years of violence featured widespread territorial cleansing, massacres, and an act of genocide in which civilians were targeted en masse based on their group identity. Thus, this is the sort of place where my theory would predict cross-group assistance. At the same time, however, this conflict provides widespread subnational variation allowing me to test both the validity and scope of the model. Rates of intermarriage, a good proxy for cross-group network integration, varied widely by township, as did rates of mixed neighborhoods and non-private apartments. Conflict type also varied widely. Some areas like Vitez and Mostar saw back-and-forth contestation of territory between two armies engaged in trench warfare. Others like Prijedor and Bijeljina were dominated by one-sided violence including massacres and ethnic cleansing [Burg and Shoup \(1999\)](#). Sarajevo experienced a siege, Srebrenica an act of genocide, and Banja Luka a crackdown on Muslims that resembles non-cleansing repression of a minority by an authoritarian state [Maass \(1996\)](#). Thus, my findings should be applicable in a wide variety of contexts where group-targeted violence is widespread but falls short of mass killing in all but a handful of locales.

3 Data and Methods

3.1 Outcome Variables

The primary data source for this analysis comes from a pilot survey conducted in Bosnia in 2019. While the small sample size ($N=100$) limits precision, the sample is nationally of adults who were at least 7-years-old and living in the country at the outbreak of hostilities.⁴ Though past scholars such as [Oliner and Oliner \(1988\)](#) have surveyed known rescuers to better understand their motivations and attributes, the present survey represents the first systematic effort to measure the frequency of assistance in the general population. To measure the frequency of cross-group assistance, enumerators read aloud six categories (*provisions, documents, shelter, transport, warning, intervention*), pausing after each one to ask whether the respondent had given that type of aid, received it, or neither. Aggregating the responses at the individual level, I generated three binary outcome variables: *help*, *gave help*, and *received help*. The first is an aggregation of the latter two. All individuals who responded affirmatively were asked a series of follow-up questions about the incident they could best remember, including the ethnicity of the other person and their relationship (see [Appendix A](#) for further details).

⁴The 95% margin of error for a sample this size is at most ± 9.8 percentage points, and considerably lower for proportions expected to be close to zero. For detailed methodology and an analysis of impact of outmigration, death, and memory biases, see [Appendix C](#)

3.2 Main Predictors

To measure respondents' prewar social networks, I created a new survey instrument that presents a list of common Bosnian names and asks respondents to identify which names correspond to people they knew before the war.⁵ Many popular first names in Bosnia are used primarily by a single ethnic group, and those I chose were unambiguous. Therefore, if a Muslim respondent reported knowing people with five of the ten Muslim names, but only one of the ten Croat names and zero of the ten Serb names, we can infer that this individual had relatively few cross-group ties before the war. In order to minimize social desirability bias, I placed this battery, which I dub an *identity roster*, prior to any questions that mentioned ethnicity, religion, politics, or war.⁶ Names were presented in alphabetical order rather than subdivided by ethnicity in order to avoid giving the impression that ethnicity was a topic of interest. Nevertheless, there are two primary forms of bias we should be concerned with. First, respondents who did not engage in help might harbor greater animosity toward other groups and thus might deny having known individuals with outgroup names. Second, individuals who gave/received help might have been more likely to have maintained contact with outgroup members in the years since the war, making them more likely to recall those people (they might also be more likely to remember the name of the helper/recipient). Both of these biases would result in an overestimate of the relationship between assistance and cross-group ties. To address these concerns, I examine the effects of intermarriage (measured by the 1991 Yugoslav census) on ethnic defection at the municipality level. The latter is based on military casualty data gleaned from *The Bosnian Book of the Dead* (Tokača 2012), a decade-long project conducted by the Sarajevo-based Research & Documentation Center, which outside evaluators estimate contains at least 96% of direct fatalities of the war (Ball, Tabeau and Verwimp 2007). Each line of the database provides the victim's name; birth date; ethnicity; soldier versus civilian status, military formation in which they fought (if applicable); and the places of birth, last residence, and death.

To measure the distribution of tie strength, the identity roster asked respondents to state whether the person with a given name whom they had known best was a relative, close friend, friend, or acquaintance. From this question, I construct the variable *strong ties*, a count of all ties that were not acquaintances. Since the identity roster represents a novel approach to network data collection, I validate it with a traditional name generator in which respondents were asked to name friends they felt close to from before the war from school, work, clubs and organizations, neighborhood, and "other." To minimize the risk of upper censoring of the

⁵Namely, "someone who knew your name whom you would have said hello to on the street in those days."

⁶The consent script did not mention these things either. The opening text began, "We are conducting research about friends and neighbors in Bosnia. We are studying what these relationships were like in Yugoslav times and how they have changed since." For ethical reasons, we requested consent again before beginning the module related to the war, warning the participants that this was to be the topic.

data, respondents were permitted to list up to 10 names, though few did. I constructed the variables *ingroup friends*, *outgroup friends*, and *total friends* based on an aggregation of their responses across categories. To determine which names belonged to the respondent’s own group, I had two Bosnian research assistants code the ethnic identities of all 564 unique names (see Appendix C.1 for inter-coder reliability). For names which could plausibly belong to more than one ethnic group or whose affiliation could not be determined, I ran robustness checks with each possible coding.

3.3 Control Variables

Opportunity The frequency of cross-ethnic social ties and cross-ethnic violence both vary by region, inducing an uneven landscape of opportunity for assistance. Areas with more people naturally provided more opportunities for assistance, as did areas with a larger share of the population belonging to the disenfranchised group. Battles were often fought and atrocities occurred in ethnically mixed areas where multiple actors sought to shore up their territorial claims. Although the violence eventually reached nearly every part of Bosnia, demand for cross-group assistance was not evenly distributed. Generally speaking, being a Muslim in areas controlled by Serb separatists proved far more dangerous than being a Serb in areas held by the Muslim-dominated Bosnian army. Also, while some areas saw significant contestation between the different sides leading to expulsions and massacres, others remained firmly under the control of one side throughout the war. I account for these factors in three ways. First, I include the variables *Muslim pop*, *Croat pop*, and *Serbs pop* to control for each group’s share of a township’s population, based on census data. Second, I include the vote share for non-ethnic parties based on 1990 municipal election returns to capture prewar rivalry and identity-based contestation. Third, I include the number of *civilians killed* in each township as a proxy for the level of victimization and hence the need for assistance. This variable may also serve as a proxy for the level of risk involved for helpers and recipients. These data derive from *The Bosnian Book for the Dead* (Tokača 2012).

Capacity In models where the outcome is *help* or *gave help*, I control for helper characteristics that may confound the respondent’s network attributes. Since children from monoethnic villages or neighborhoods become more likely to meet people outside the ingroup as they grow older and travel farther afield for school or work, their network is likely to become more diverse as they age. Their capacity to offer assistance likewise increases as they grow older. Therefore, I control for *age* in all these models. I include two economic variables directly related to assistance: whether someone in the respondent’s household owned a *car* or a weekend/summer *cottage*, both of which were fairly common in Yugoslav times and could be used for transport or shelter. I also include *gender* and *education*, though these could also be thought of as

willingness variables.

Willingness To control for willingness, I include the following attitudinal controls: how religiously the respondent was raised (*religiosity*), whether their parents would have disapproved if their child had married a member of the outgroup (*parents disapprove*), and if being Yugoslav was important to their parents (*parents Yugoslav*). All of these factors are likely to affect both one’s propensity to form outgroup ties prewar and one’s willingness to provide cross-group assistance. I include two biographical availability traits: whether the respondent had other people to support financially (*dependents*) and whether they were taking care of children daily (*childcare*). These traits could constrain one’s willingness to take risks (they could also be seen as a capacity variables since family obligations constrain availability). Finally, I control for the personality traits most likely to affect willingness to help and propensity to form outgroup friendships. I assess these using the BFI-2, the shortened version of the Big Five Personality Inventory, standard in the field of personality studies, that measures individuals along five personality dimensions (Soto and John 2017). To this I added one question from an earlier section of my survey, *thrill-seeking*, that asked participants if they tended to seek out risky, thrill-seeking or dangerous activities for fun before the war.

3.4 Interviews

Over the course of 10 months, I conducted over 160 interviews with individuals who had lived in Bosnia during the war. The war is still a sensitive topic in Bosnia, and many Bosnians are either tired of talking about it, wary of discussing it with a stranger, or worried about how their own neighbors will react if they found out what they had said. Thus, the vast majority of interviews were arranged through one of four interpreter/research assistants, who tapped their extended social networks including relatives’ acquaintances and former colleagues. That said, even when each research assistant knew the respondent and could vouch for me, an overwhelming majority of those contacted declined to be interviewed. In addition, a considerable number of interviewees had no prior connection to my research assistants but were referred to us by people we had already interviewed. Finally, a handful of interviewees were found by approaching individuals on the street or at their place of work, engaging them in friendly conversation about their past, and asking if they would consent to a formal interview. See Appendix A for details on procedures. To address concerns about positionality and memory bias, I compare my interviews to other sources including Broz (2004), an anthology of stories of assistance collected by a Yugoslav cardiologist during and shortly after the war, and RDC (2010), a collection of testimonies compiled by the Sarajevo-based Research & Documentation Center.

FREQUENCY OF CROSS-GROUP ASSISTANCE BY CATEGORY

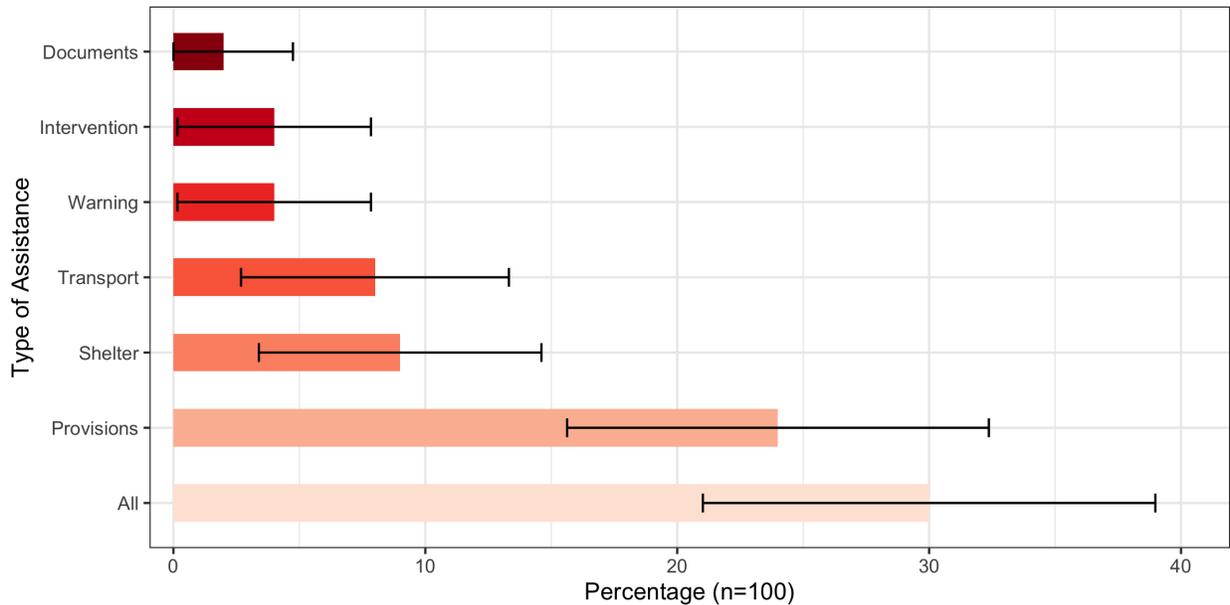


Figure 1: Helpers and recipients are pooled. Error bars represent 95% confidence intervals for population frequencies.

4 Results

4.1 The Ubiquity of Cross-Group Assistance

Just how widespread was cross-group assistance? As shown in figure 1, three out of 10 respondents reported having given or received cross-ethnic help (30%, with a 95% confidence interval of [21, 40]). The numbers remain substantial even if we limit the scope of assistance to the five categories of acts other than providing *provisions*: *documents*, *shelter*, *transport*, *warning*, and *intervention*. Thus, the remarkable frequency of assistance cannot be explained simply by people who were sheltering in the communal basement of their apartment buildings sharing their pantry and humanitarian care packages with all of their neighbors, a theme that frequently came up in my interviews in Sarajevo. The other five types of assistance are more likely to be dyadic, or at the very least, would only be relevant to members of a persecuted group. As my interviews suggest, most non-*provisions* acts of assistance required a conscious choice to defy the conflict cleavage, rather than just dispersing help indiscriminately to whichever alters from any group happened to be on hand. Thus, even with a narrower definition of what constitutes assistance, we still see these behaviors in 14% ([7, 21]) of the population. See Appendix A for a sensitivity analysis, as well as a breakdown into help given and help received.

Does such widespread assistance seem credible in light of other evidence? Anecdotally, I can say that over my 10 months of fieldwork, I frequently came upon stories of cross-ethnic help when I wasn't

even looking for them. The Alamo rental car agent, my first landlady, a staff member at the survey firm, and a professor I met at a coffeehouse all volunteered stories about themselves or a relative who had been rescued from dire straits as soon as I mentioned why I was in Bosnia and what I was studying. Although my research assistants often tried to seek out people they knew who had stories, we often met with older Bosnians who didn't have a clear picture of what my research was about and who, it turned out, had helped an outgroup member during the war or had themselves been helped. The limiting factor when it came to getting interviews was not whether people had relevant experiences to share but whether they were willing to share them with an American researcher or even willing to relive them at all.

Similarly, [Broz \(2004\)](#) does not seem to have had much trouble finding cases for her collection even while the war was still going on. Her project of collecting positive stories began, in fact, while she was running a free clinic in Serb-held Eastern Bosnia during the first year of the war and began hearing such tales from her patients. Her clinic was situated in the Drina valley, a region that had seen some of the most brutal ethnic cleansing and massacres, yet despite this tragic circumstance (or because of it) stories of cross-ethnic assistance abounded. [Mertus et al. \(1997\)](#)'s anthology of writing by women refugees, compiled during the war, also contains references cross-ethnic assistance in nearly all their stories about "the journey out" from Bosnia, despite the fact that the editors were not explicitly seeking them. Returning to her hometown of Srebrenica for her master's thesis, two decades after the massacre, [Medic \(2016\)](#) managed to interview three Serb helpers and eight Muslim recipients in the course of two weeks. This sample represents only a fraction of the helpers Medic identified since most were not willing to be interviewed. In the course of field testing questions for my pilot survey, one of my research assistants found that many of the older residents in her building had helped or been helped during the war. Thus, whether the researcher was a complete outsider (myself, Mertus), a regional insider but outsider to the community (Broz, Mertus' coauthors from other Yugoslav republics), community insider (Medić and my research assistants), we see a similar pattern: individuals who gave or received help are not hard to come by.

4.2 More Cross-Group Ties Correlate with More Assistance

I find substantial support my second hypothesis through logistic regression models measuring the impact of cross-group ties (measured three ways) on giving and receiving assistance. In all the relevant models, having a higher outgroup degree is consistently associated with providing cross-group assistance. As shown in the first row of Table 1, the number of outgroup names a respondent identified on the identity roster is positively associated with giving help (note the p-values, rather than standard errors, are shown). The name generator yields a very similar result (Model 2). All coefficient estimates for continuous variables

Table 1: Logistic Regressions for Giving and Receiving Assistance

	Gave Help (1)	Gave Help (2)	Gave Help (3)	Received Help (4)	Received Help (5)	Received Help (6)
Outgroup Names	1.79 (0.02)			8.38 (0.00)		
Outgroup Friends		1.65 (0.04)			1.72 (0.32)	
Mixed Family			4.38 (0.05)			3.30 (0.21)
Age	3.49 (0.00)	3.41 (0.00)	3.90 (0.00)			2.13 (0.07)
Parents Yugoslav	2.13 (0.02)	2.05 (0.03)	1.93 (0.04)	10.14 (0.01)	5.25 (0.01)	2.72 (0.01)
Car	5.06 (0.04)	5.63 (0.03)	5.45 (0.03)	21.08 (0.03)	14.19 (0.02)	6.54 (0.03)
Parents Disapprove	0.26 (0.04)	0.26 (0.04)	0.36 (0.11)	0.00 (0.01)	0.02 (0.00)	0.09 (0.03)
Ingroup Names				0.38 (0.12)		
Sociability				0.17 (0.01)	0.24 (0.01)	0.42 (0.04)
Agreeableness					0.39 (0.08)	
Religiosity				3.76 (0.03)		
Outgroup Killed				0.28 (0.01)	0.36 (0.05)	
Ingroup Pop					0.56 (0.25)	
N	100	100	100	100	100	100
AIC	92.18	93.10	90.85	65.68	70.60	71.53
BIC	107.81	108.73	106.48	91.73	96.65	89.77
Pseudo R2	0.44	0.43	0.46	0.56	0.50	0.41

p-values are shown in parentheses. Coefficients are exponentiated and can be interpreted as odds-ratios (effects above 1 are “positive”). All continuous predictors are mean-centered and scaled by 1 standard deviation. Standard errors are heteroskedasticity robust.

have been standardized, except for dummy variables, so a change of one unit corresponds to one standard deviation. They have also been exponentiated in order to be interpretable as odds ratios. Thus, a one standard deviation increase in outgroup names corresponds to the odds of outgroup help nearly doubling (that is, being multiplied by 1.79). Note that the coefficient estimates for *outgroup names* in Model 1 and *outgroup friends* in Model 2 are strikingly similar, despite the marked difference in how each of them measures outgroup degree. This similarity suggests that the effect of outgroup friends on giving help is relatively insensitive to how it is measured, and that 1 SD difference in how many people a respondent knew by sight had the same effect as 1 SD difference in how many close friends she had. Having relatives in mixed marriages is also positively and significant. All things being equal, a Bosnian from a mixed family had nearly four-and-a-half times the odds of providing cross-group assistance as someone a family with no mixed marriages.

The effects of network-related variables on receiving help vary more substantially, probably because there are fewer positive cases. Thus, these results should be interpreted with caution. Nevertheless, it appears that *outgroup ties* had a strong and statically significant effect on *received help* at a $p < 0.01$ level.

The coefficient estimates for *outgroup friends* and *mixed family* are not significant, though they are about the same size as in the previous models. Figure 2 shows predicted probabilities for outgroup assistance (giving and receiving pooled) based on the number of outgroup names identified. The effects are almost linear with slope 1 for the entire interquartile range, such that a 25 percentage point increase in outgroup degree corresponds to a 25 percentage point increase in the probability of assistance. For analysis of the control variables in this model capturing capacity and willingness, see Appendix F. In Appendix G I show that ethnic defection was likewise correlated with cross-group ties (intermarriages) at the municipality level, lending further support to this hypothesis.

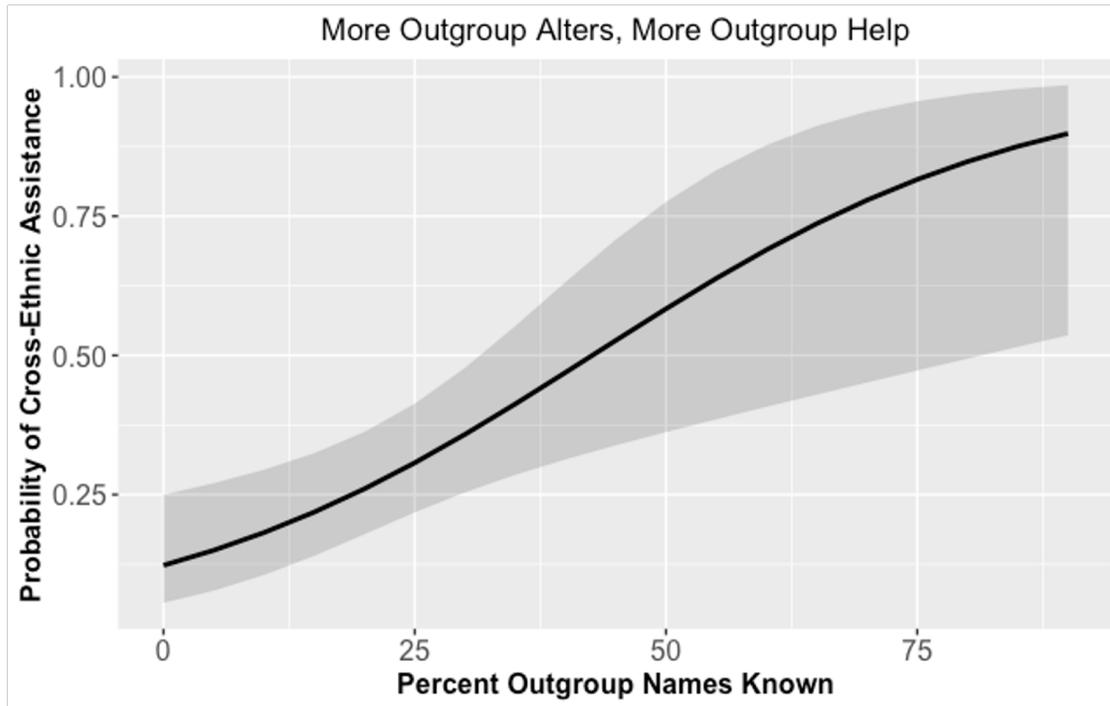


Figure 2: Predicted probabilities for *outgroup names*. The more people with outgroup names in the identity roster that respondents claimed to have known before the war, the greater the likelihood of giving or receiving wartime assistance. 95% confidence interval shaded.

4.3 Tie Strength Fails to Predict Assistance

Perhaps the most surprising finding of this analysis is that one's odds of providing assistance appear to be unaffected by tie strength. Figure 3 shows the results of four logistic regressions involving tie strength, using the same control variables as Model 1 in Section 4.2. The coefficient estimates are again exponentiated to be interpretable as odds ratios, meaning that 2 indicates a doubling of the odds and 1 leaves the odds unchanged, a null effect. The results shown are based on the identity roster follow-up question, which asked respondents about their relationship to the possessors of each of the names they said they knew. Names

COEFFICIENT PLOT FOR THE EFFECTS OF STRONG & WEAK TIES ON **GAVE HELP**

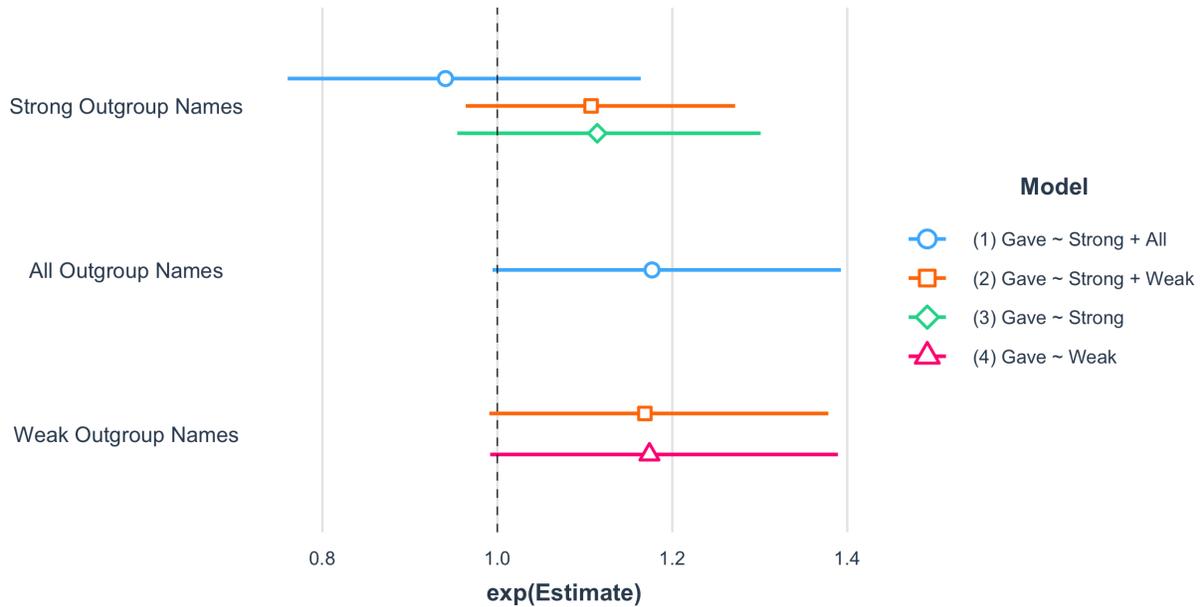


Figure 3: Logistic regression with same control variables as in Table 1, Model 1. Estimates are exponentiated to represent odds ratios but not standardized since they are already on the same scale. Thus, a one-unit increase is equivalent to identifying one additional name on the identity roster as weak, strong, or either. 95% confidence intervals based on HC2 robust standard errors.

which the respondent identified as acquaintances are coded as weak, while those which they identified as friends, close friends, and relatives are coded as strong. Further disaggregation was not possible due to lack of data (the median number of friends was 1 for both variables), but the results seem fairly clear. The effect of *strong outgroup names* on providing assistance is indistinguishable from null in all three cases, whether we control for the total number of all *outgroup names* (Model 1), the number of *weak outgroup names* (Model 2), or neither (Model 3). The effects of all *outgroup names* and *weak outgroup names*, on the other hand, are much closer to being significant ($p \approx 0.06$). We can interpret the first model as follows: conditional on the total number of outgroup ties, increasing the fraction of those ties that are strong has no effect on the probability of helping. In fact, the exponentiated coefficient estimate is less than one, meaning if that point estimate is correct, having more strong outgroup ties would actually lower one's odds of becoming a helper (the estimate is not precise enough for us to rely on, however). In models 2-4, we see that strong and weak ties are virtually interchangeable. Their point estimates and confidence intervals are virtually identical whether we include either or both of them in the model.

The non-effects of tie strength of helping are confirmed by a vignette experiment embedded in the survey. Respondents were asked how much help they would be willing to provide first, to a neighbor fleeing intimate partner violence, and second, to a local man on the run from corrupt municipal authorities. The

COEFFICIENT PLOT FOR THE EFFECTS OF STRONG & WEAK TIES ON RECEIVED HELP

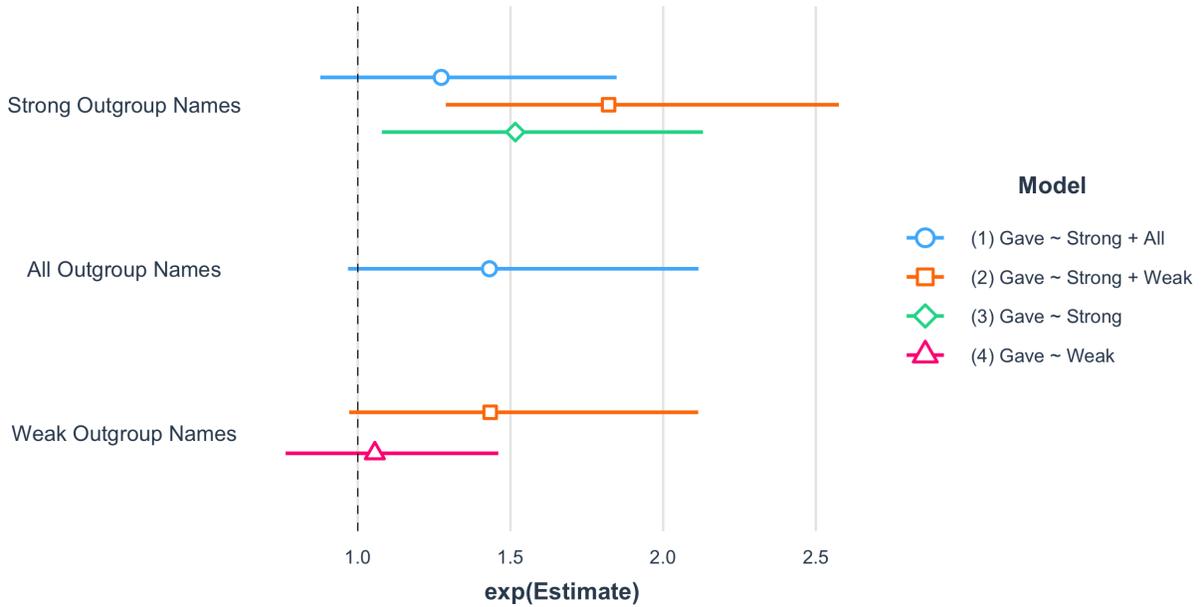


Figure 4: Logistic regression with same control variables as in Table 1, Model 4. Estimates are exponentiated to represent odds ratios but not standardized since they are already on the same scale. Thus, a one-unit increase is equivalent to identifying one additional name on the identity roster as weak, strong, or either. 95% confidence intervals based on HC2 robust standard errors.

persecutee in the story was variously described as a acquaintance, friend, close friend, or in-law in different treatment conditions, and the ethnicity of the name and whether the persecute asked for help were varied as well, resulting in factorial design. None of the treatments including tie strength demonstrated a statistically meaningful effect.

The results for receiving assistance are less clear. Again, we borrow the controls from Section 4.2, this time belonging to Model 4 in Table 1. In the first model, we see that the effect of *strong outgroup names* is not significant when we control for the total number of outgroup ties, but the latter isn't either and the coefficient estimates are similar. In Model 2, we see that the number of strong outgroup ties is significant both statistically and substantively when both *strong* and *weak outgroup names* are included in the model. For a given number of weak ingroup ties, adding more strong ties seem to have a considerable effect on the odds of providing assistance, nearly doubling them. On the other hand, *weak outgroup names* may or may not have an effect independent of *strong outgroup names*, and that effect may or may not be zero—there is simply not enough evidence to tell. Finally, when we consider the effects of *strong* and *weak outgroup names* separately in Models 3 and 4, we see that the estimate of *strong outgroup names* on their own is diminished, though still significant at a $p < 0.05$ level, while the estimate for *weak outgroup names* is shifted considerably towards zero.

5 Discussion

What are we to make of these contrasting findings? One potential interpretation is that, all things being equal, Bosnians were likely to provide assistance to whomever they knew, regardless of how well they knew them. Yet, they were only likely to receive assistance from those they knew well. The latter finding sounds illogical, unless Bosnians with few strong outgroup ties were substituting ingroup help for outgroup help or passing up getting help at all if their situation was not so dire. Naively, we should expect the total number of strong ties used to provide assistance and the total number of strong ties used to receive assistance to be equal, since they are, in fact, the exact same ties. There are a few ways out of this paradox, however. First, this might simply be a data problem. With only 15 individuals reporting having received assistance, the noise in the outcomes is considerable. Second, ties need not be symmetric: one person may consider someone a friend while the other considers them an acquaintance. They may feel differently about the relationship, or they may have different thresholds for their definition of “friend.” Third, it is entirely plausible that recipients would retroactively classify a relationship they had with a helper as friendship, even if at the time both considered each other to be acquaintances. Finally, and most importantly, just because strong ties make you more likely to receive assistance does not mean that assistance had to come through a strong tie. Persecutees with many strong ties may have felt more inclined to trust any outgroup member they knew, regardless of tie strength. Conversely, helpers with more ties of both types may have felt more compelled to do something to help, but then sought out those they cared about most to offer assistance to. In order to assess which of these four explanations seems most likely, let us ask the helpers and recipients themselves whom they got help from or gave help to.

The importance of weak ties is reflected in my interviews and in those previously collected by other scholars. Consider the following quote from Hasan, a Muslim man caught in a roundup of Muslims in the Grbavica neighborhood of Sarajevo, which had just fallen to Serb militia:

My neighbor took me out of that group so they don't take me somewhere. Because they were separating [Muslims] for the concentration camp in Lukavica. Before the war we never even had a coffee. Yeah, we greeted each other just out of politeness. I didn't even know his name!

Or this quote from Jovana, a Serb woman in Banja Luka who helped out one of her Muslim acquaintances:

I was taking care of my neighbor's children. Two girls. They lived with me, yeah. They were high school students. Their parents were there but they were afraid for them. And so they slept [in my house] because we were neighbors. Their parents and I worked together. We weren't really [close] but we knew each other. I worked in [sales] and they worked in production. They were

there for a month.

In both these cases, we observe weak ties providing crucial assistance, in Hasan’s case from the recipient’s point of view, in Jovana’s case, from the helper’s. That said, these ties are weak in the sense of affect, not the frequency of interactions. In both cases, but particularly in Jovana’s case, the helper and recipient(s) saw each other on a regular basis, thus increasing the chances they would be aware of one another’s need for assistance and capacity/willingness to provide it. Hasan surely had stronger ties elsewhere in the community, but they did not have timely information about his plight and, thus, were unable to provide assistance, if they were even able. In nearly all of my interviews, I explicitly asked how close the helper and recipient were and found that they were “normal friends,” much more often than close friends, as well as acquaintances some of the time. Rarely, however, did I hear of instances of complete strangers with whom no network connection existed.

In sum, we have overwhelming evidence to suggest that weak outgroup ties are capable of channeling assistance and that those who have more of them are more likely to provide it. This does not, however, confirm [Granovetter \(1973\)](#)’s “strength of weak ties” theory, which argues that weak ties are *more* effective than strong ones at passing along new information and opportunities. There is no evidence here to suggest that weak ties have some special property that makes them more effective. In fact, as [Centola and Macy \(2007\)](#) state in their abstract, “The strength of weak ties is that they tend to be long—they connect socially distant locations.” Cross-group ties are often already “long” in this sense, particularly in Bosnia given the ethnic homophily seen in friendships, marriages, and residential patterns. Thus, for any tie, conditional on it being cross-group, weakness probably does not confer any additional benefits. This finding may extend to other contexts, well beyond Bosnia and civil war. Strong ties may make people more likely to *receive* assistance, but the data is too noisy to be confident that this finding will hold up for further scrutiny.

6 Conclusion

Despite widespread group-targeted violence including rape, looting, massacres, territorial cleansing, and concentration camps, a substantial portion of Bosnians of all ethnic groups continued to act on their cross-group relationships throughout the war. Those who were threatened sought help from those they knew, while those who were in a position to help offered what they could to their relatives, friends, colleagues and acquaintances. As a result, cross-group assistance was widespread, with somewhere on the order of 30% of Bosnians giving or receiving help from a member of the outgroup. Help could come through strong ties as well as weak ones or even higher-order alters whom the recipient did not know but was connected to through other helpers. Individuals whose ties to the outgroup tended to be stronger were no more likely to help than

those with weaker ties.

In addition to documenting the ways in which networks mobilize cross-group assistance, this study serves as an invitation to scholars to investigate cleavage-defying behaviors more broadly. Although political scientists such as [Mueller \(2000\)](#) and [King \(2001\)](#) have long questioned whether so-called “ethnic” wars are really so different from non-ethnic ones, researchers have yet to investigate the full scope of behaviors through which individuals break out of that ethnic paradigm. [Wood \(2003\)](#) examines cleavage-defying behaviors in a conflict where the boundary is defined by class, but scholars have yet to connect her work to the literature on ethnic defection. In drawing attention to the myriad ways by which people defy the master cleavage of a civil war, I hope to encourage other researchers to look at ethnic defection, cross-group assistance, and so-called peace communities ([Kaplan 2017](#)) as part of a common framework of cleavage-defying behavior. Those who study civil wars, mass movements, repressive regimes, or ethnic and class divisions have much to gain from trying to understand why people break ranks with their group and what sorts of social structures make them more likely to do so.

Though it is tempting to take the results of this study as evidence for the efficacy of policies bringing together people of different groups, one needs to be cautious in doing so. Ethnic groups’ claims to legitimacy over a given territory are often driven by settlement patterns ([Toft 2010](#)), which residential integration can undermine. As [Woodward \(1995\)](#) documents, Croat and Serb nationalists saw Bosnia’s mixed communities as an obstacle to their aims of partition and thus embarked on a savage campaign of rape, torture, and massacres in order to achieve what [Toal and Dahlman \(2011\)](#) dub the “unmixing” of Bosnia. As a result, the war in Bosnia was primarily one of territorial cleansing in which ethnic extremists concentrated violence in areas that were highly mixed. Therefore, any policy-maker seeking to foster cross-group harmony through networks needs to think carefully before promoting residential integration as a means to achieve it. The same mixed residential patterns that give rise to cross-ethnic assistance during conflict through neighborhood networks can also attract violence entrepreneurs like a magnet if such mixing stands in the ways of their strategic or ideological aims. Nevertheless, if a community is already mixed, then promoting cross-group ties may be a good way to mitigate violence. Moreover, studying the pathways through which networks promote cross-group assistance can open the door to more nuanced policies. The non-effect of tie strength, for instance, suggests that peacebuilding funds might be best spent on programs that aim to foster weak ties between a large number of people rather than strong ties between a few. However, before these findings can be applied, further research is needed on the flip side of assistance—betrayal—to make sure promoting weak cross-group ties does not incur more harm than good.

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A Survey and Interview Procedures

Site selection was determined through a multi-stage random sampling strategy, stratified by population, local ethnic majority, and urban/rural location according to data from the 2013 Bosnian Census. Households were then selected by random walk procedure and a single respondent chosen in each household using the last birthday method. Potential respondents were asked if they would be willing to take part in "research about friends and neighbors in Bosnia" and "what these relationships were like in Yugoslav times and how they have changed since." They were handed a small gift (coffee packet or mobile credit), which they were informed they could keep whether or not they participated. Of those who could be located after multiple attempts and were eligible, 75% consented to participate, which Prism Research reports is typical regardless of research topic. Only one participant is reported to have broken off the survey once it had begun. Thus, self-selection does not appear to be a major concern. When respondents reached the middle of the survey, they were informed that the next battery of questions would discuss "how friends and neighbors acted during the war" and reminded that they could skip a question if they chose. Survey enumerators were themselves Bosnian, generally from the same region and ethnic group as the respondents they were sampling. All had worked on prior surveys for Prism Research and underwent training specific to this survey and had their work checked by supervisors. The survey was conducted orally in the local language, assisted by tablets or smartphones. Survey questions were tested extensively with local respondents before the pilot stage and independently back-translated into English to ensure translation accuracy. Survey modules included prewar life and social ties, wartime assistance and victimization, present-day attitudes, two survey experiments on willingness to provide assistance, and the short form of the Big Five Inventory (BFI) personality test. Aside from questions in which respondents were asked to supply names of contacts, all questions were close-ended and enumerators were instructed to read out all of the response options on most questions.

My interview procedures, as well as the aforementioned survey, were vetted and approved by the Northeastern University Internal Review Board (IRB #18-08-17). Most interviews took place in a café or

in the interviewee's living room and lasted 60-90 minutes. In keeping with traditional hospitality customs, interviewees would often insist on paying for our drinks if we went out or providing us with drinks and sweets if we were at their home. At other times, I would buy their drinks, and when visiting a home, I almost always brought a coffee packet as a gift. I would begin each interview by introducing myself in the local language,⁷ obtaining informed consent, and explaining in broad terms the purpose of the interview, which they generally already knew. My interpreter/RA would then take over, though as the year progressed and my language skills improved, I proceeded to ask more questions directly and not ask for interpretation of their responses. Nearly all interviewees consented to having their interviews recorded, and for these, I hired an RA, often different from the original interpreter, to go through and transcribe and translate the entire interview. Lastly, I coded the interviews with MAXQDA qualitative data analysis software to identify common themes and illustrative passages worth quoting. Interviewees were promised anonymity, though many gave permission to be named, and were not offered compensation beyond the tokens of hospitality mentioned above.

The distribution of ethnicities in the sample reasonably matches the distribution of ethnicities in the 1991 census: 35% Serbs (31% in 1991), 13% Croats (17% in 1991), and 51% Muslim (43% in 1991). The discrepancy in the number of Muslims can be accounted for by respondents who chose the Yugoslav category on the 1991 census, but today identify as Muslim. In 1991, 6% of Bosnians identified as Yugoslav, whereas in the 2013 census less than 1% did, a drop of over 5 percentage points. The proportion of self-identified Muslims, meanwhile, has climbed 8 percentage points while the proportion of Serbs and Croats has dropped slightly by 0.5 and 1.9 percentage points, respectively. Obviously, outmigration has impacted all three groups, and the gains in the Muslim share of the population are, in part, due to higher birth rates. Nevertheless, given the intense nationalism sweeping the Croat and Serb communities in 1991, in contrast to emphasis in the Muslim community on preserving a multiethnic Yugoslavia, it seems likely the vast majority of those self-identified Yugoslavs later chose to identify as Muslim in the 2013 census and this survey.⁸ The gender ratio is reasonably accurate (47/53 in the sample, 49.9/50.1 in the 1991 census). The age structure reflects, as one might expect, the die-off of the older generation. The oldest person in the sample was 52 in 1991, whereas 16.4% of the population in the 1991 census were age 55 or older. Nevertheless, two important features of the census do show up in the sample: a severe dip in the number of individuals born during WWII, and a baby boom for those born 1955–1965. Thus, we have reasonable evidence to suggest that Prism Research was successful in capturing a representative sample of those Bosnians still alive and living

⁷The name of this language is politically fraught. Bosnian, Serbian, and Croatian are all mutually intelligible and in some regions have more internal variation in dialects than there is variation between them. Linguists do not have a strict definition as to what constitutes two dialects versus two languages.

⁸Other Yugoslavs may have reclassified themselves as “other” (*ostali*), a response which grew from 1.5% to 3%.

in the country who were old enough to have social ties at the start of the war (respondents had to have been living in Bosnia at the start of the war and at least 7-years-old at the time to be eligible). In Section 4.1, I show that several important indicators of cross-group network density closely match their distributions in the census.

B Sensitivity Analysis

Despite the rigor with which this survey was conducted, results should still be interpreted with caution and in conjunction with other sources. When conducting research 24 years after a war, certain sampling and reporting biases are unavoidable. First, the respondent pools will have been disproportionately young at the time of the war as discussed earlier. Children usually assist others, but young adults might do so disproportionately, so it is unclear which way the bias goes. Second, individuals who were killed may have faced lower rates of assistance. However, given that only 2.2% of the population was killed in the war (Toal and Dahlman 2011, 136), the potential for bias is small. Third, the survey excludes Bosnians living abroad. Over a million refugees are estimated to have fled Bosnia during the war out of a pre-war population of 4.4 million. Of those, about 400,000 had returned by 2004 (United Nations High Commissioner for Refugees 2004; Valenta and Strabac 2013), but others continued to depart, seeking better economic opportunities in the European Union. Finally, respondents are likely to better remember events which are more recent or salient to them, and their reflections on prewar life are bound to be filtered through what happened to them during the war and the evolution of their worldview since. While such biases are to an extent unavoidable, the Prism Research team and I endeavored to minimize them through careful crafting of question wording and ordering. For instance, in order to facilitate recall of prewar social ties, specific loci of friendship formation are provided such as neighborhood, school, and workplace. No mention of the war or ethnicity is made until after these names have been elicited to avoid respondents attempting to satisfy the desires of the survey-taker by deliberately naming a member of each ethnic group (a problem which frequently arose during my qualitative interviews). Types of assistance are carefully delineated to avoid differences in definition of such vague terms as “help” or “rescue” and in order to prompt memories of specific forms of aid. For questions where phrasing seemed particularly likely to influence answers, two or three different versions were randomly assigned to different participants during the pilot wave in order to assess the sensitivity to wording in the responses.

While it is impossible to go back in time and fully reconstruct prewar network and wartime behavior of a war that took place 24 years ago, this survey represents a near-optimal attempt to capture what data remains in the minds of its survivors in a systematic and statistically-meaningful way. Though we would

FREQUENCY OF CROSS-ETHNIC ASSISTANCE

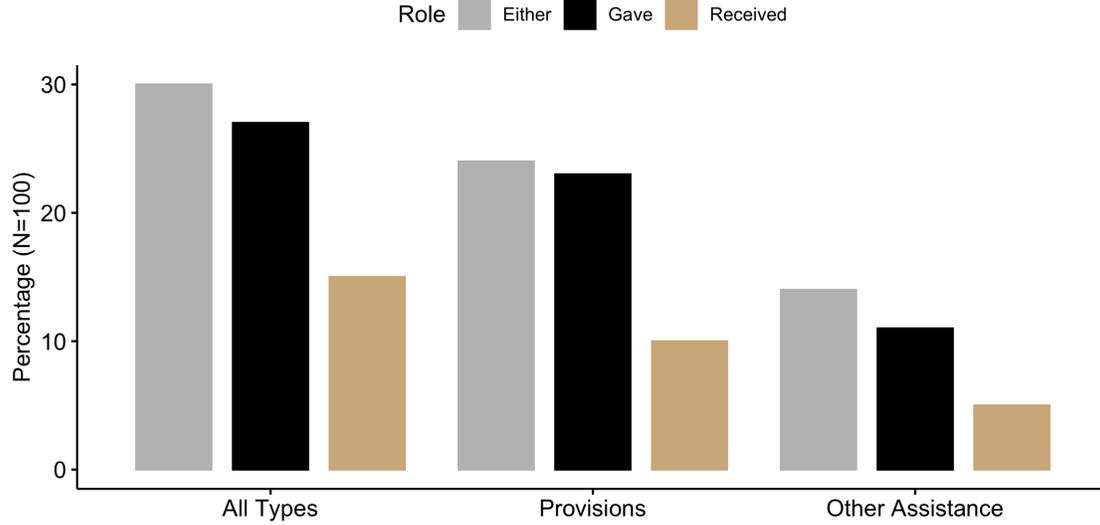


Figure 5: Note that many respondents reported both giving and receiving. Some of those who gave or received provisions also gave or received other types of assistance.

be hard-pressed to draw strongly convincing causal inferences from this data, quantifying the frequency of different types of assistance and their correlation to various network factors is an important first step. Through sensitivity analysis and an honest assessment of the direction and magnitude of the bias in each question, I aim to produce ballpark estimates that can serve as an informative prior for future studies of cross-group assistance in other political conflicts.

As show in Figure 5 reports of giving assistance (27% [19, 37]) far outstrip reports of having received assistance (15% [9, 24]). This gap persists and remains statistically significant even after we break down assistance by type as shown. In all three cases, respondents are about twice as likely to report giving than receiving assistance. There are at least four sets of explanations: a) respondents overstate being helpers, b) respondents understate being recipients, c) helpers are overrepresented and recipients underrepresented in the sample, and d) there were simply more helpers than recipients to begin with. Although I cannot entirely rule out the first possibility, it does not fit with my experiences seeking out helpers for interviews. Like other scholars discussed below, I generally encountered a reluctance to admit to having helped “the enemy” for fear of reprisals from ingroup neighbors or political actors. Thus, socially desirable effects, though possible, seem at least as likely to depress reports of help-giving as to inflate them. The second explanation—under-reporting by recipients—could occur if recipients are bitter about the war and reluctant to give credit to the other side for something positive. If true, this would imply the true frequency of cross-group assistance is closer to the higher number reported by the helpers. It seems unlikely recipients would be more likely

than helpers to forget an act of help. While it's possible that recipients received many acts of help and only remember the most salient ones, that bias cannot explain this gap since all the questions were posed in binary format. The third explanation, underrepresentation of recipients, seems quite plausible. Recipients generally hail from the group under threat in their locale and, therefore, should be more likely to die in the war or emigrate and not return compared to helpers. In fact, many of the stories told by both helpers and recipients in the interviews I examine culminate in the recipient gaining refuge in another country. This set of explanations, like the second set, would mean that the true count is closer to that reported by the helpers. Finally, according to the logic of the fourth explanation, it is entirely possible that both helpers' and recipients' estimates are reasonably accurate. A persecuted person may require many acts of help from different individuals in order to get through the war, and indeed, we see a number of such reports in the interview testimony. Alternatively, a single act of assistance may require more than one helper to be logistically feasible. Networks provide a useful way to think about this. If the recipient and helper are put in touch through one or more intermediaries, each intermediary can in turn consider themselves a helper. Likewise, if the recruitment mechanism is in play, we should expect helpers to recruit multiple assistants. While, in some cases, entire families might benefit from a single act of help, it is also possible for assistance provision to be a family activity, particularly in the case of shelter, since everyone in a house where a recipient is staying can claim to be providing help. Therefore, of the four sets of explanations, the three that seem most plausible each suggest that the self-reported frequency of helpers, rather than recipients, is more reflective of overall levels of help in the population.

How sensitive are these results to death and outmigration? According to the *Book of the Dead*, about 2.2% of the Bosnian population died during the war. Soldiers were far more likely to die than civilians; about 60% of the deaths caused by the war, including indirect casualties, were among members of an armed group, irrespective of combatant status at the time of death, yet members of armed groups made up far less than half the population. In our sample, however, respondents who reported being in an armed group during the war had a 53% ([38, 84]) chance of reporting assistance, while civilians had a 23% chance ([14, 35]).⁹ Thus, if anything, the disproportionate loss of soldiers during the war should lead to an undercount of acts of assistance, not an overcount.¹⁰ Likewise, men were about nine times more likely to die in the war than women, yet their reported rates of assistance (32% for men, 28% for women) do not differ substantially, nor is the small gap statistically meaningful. Thus, unless men were much less likely to give or receive help in the first place, it does not seem likely that wartime fatalities would result in overestimates of wartime

⁹Due to the small sample size, I pool combatants, non-combatant armed group members, and non-members who helped an armed group into the "armed group" category. The gap between civilians and combatants is slightly smaller if the last of these three types is pooled with civilians, but not substantially.

¹⁰Assuming that receiving help was not substantially more likely to be life-saving for soldiers than civilians.

assistance.¹¹

Sources disagree as to how many Bosnians left the country during and after the war and how many have since returned. Drawing on figures from the statistical agencies and embassies of the receiving countries, the Bosnian Ministry of Human Rights and Refugees estimated there were at least 2 million Bosnians living abroad in 2017, compared to 3.35 million in country (Kovacevic 2017). One cannot say for certain that nearly 60% of the wartime population left the country and never returned, however, both these figures include people born after the war. Most likely the percentage is lower, since the predominant migration trend in recent years has been for young people, many born after the war, to leave the country looking for work. The World Bank also in 2017 estimated 43% of the population lived abroad, though since Bosnians intermarry with people in the countries they migrated to, it becomes difficult to define what exactly the reference population is with regards to the next generation (Kovacevic 2017). Between 2000 and 2018, the Bosnian mortality rate hovered between 0.8% and 1.0% per year (CIA World Factbook 2018). Thus, in the 17 years between the start of the war and the survey, we would expect approximately 15% of the population to have died (ignoring compounding effects and differential risk across age groups). As a back-of-the-envelope calculation, if we assume around 60% of Bosnians have either permanently emigrated or passed away since the start of the war, then the rate of assistance could be as little as half of what was reported. This assumes, however, that none of the individuals who died or left were involved in acts of assistance. In my survey, I asked respondents if they had ever lived abroad during or after the war. Rates were virtually identical for the 15 respondents who had returned from abroad compared to the 85 who had never left (33% versus 29%). While returnees are not necessarily representative of those who stayed abroad with regards to wartime assistance, these figures do suggest that there may not be much of a gap. With regards to postwar die-off of the older generation, I lack a large enough sample to re-weight estimates by age. However, if we compare the 11 people in the sample over the age 65 at the time the survey was administered to the 89 younger individuals, we see that older respondents were substantially more likely to report assistance (a χ^2 test gives a p-value of ≈ 0.026). This finding remains statistically significant for cutoffs of age 60, 55, and 50 as well. Thus, the lack of older people in the sample seems more likely to be causing underestimates of help than overestimates. Granted, the oldest respondent sampled was 52 at the time of the war and 79 at the time of the survey. Those who were elderly at the time of the war may have been less physically capable or found themselves with fewer opportunities to provide assistance. However, only 16% of the population in 1991

¹¹While it may seem plausible that women were more likely to provide help, bear in mind that men were more likely to be in mortal peril. Even among civilians, men were three times more likely to die, in part because the enemy soldiers often assumed they were—or were capable of becoming—combatants. Therefore, it seems likely there were far more opportunities for men to become recipients of help, at least judging from fatality rates. That said, without reliable figures on the rates of wartime sexual violence, it is difficult to fully compare the frequency with which women and men were in danger (Cohen 2013) and, thus, may have needed assistance.

IDENTITY ROSTER RESULTS FOR INGROUP AND OUTGROUP NAMES IDENTIFIED

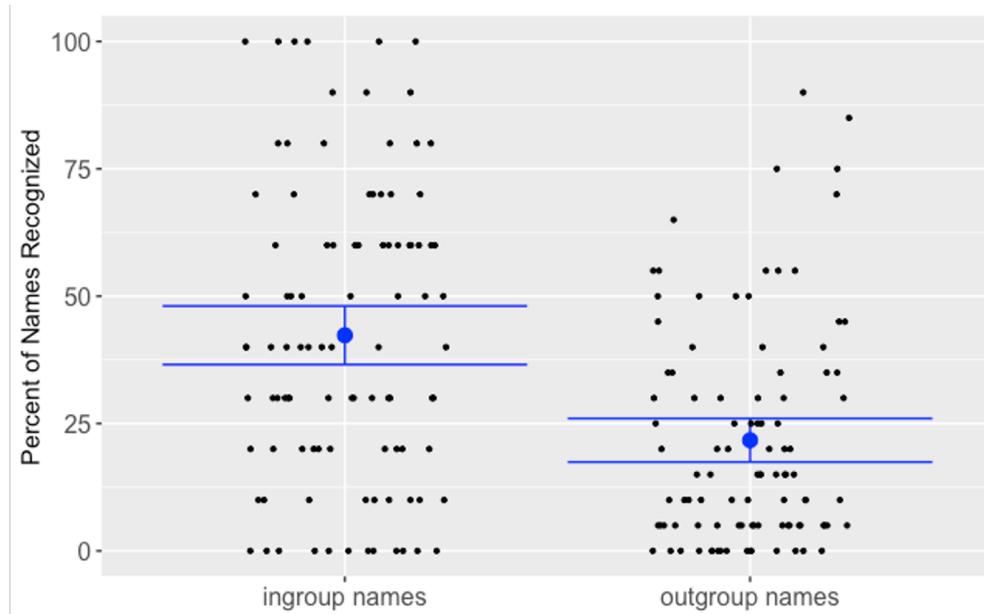


Figure 6: Distribution of responses for the identity roster survey question: “For each name I list, please tell me if you knew someone by this name before the war.” All 100 respondents saw the same list of 30 names, including 10 associated with their own religion/ethnicity and 20 associated with the other two groups. Percentages of names identified are shown for every respondent in both columns as a scatter plot with estimates and 95% confidence intervals for the population means superimposed.

was 55 and older, so the impact on the overall rate of assistance cannot be too substantial. Furthermore, in Broz (2004) and Mertus et al. (1997)’s interviews conducted during or shortly after the war, we see no shortage of older people involved in assistance, either as interviewees or people mentioned in their accounts. The same holds true for the interviews in RDC (2010) conducted in 2007-9 (compiled from earlier testimony and accounts) and my own fieldwork.

C Measurement of Prewar Ties

C.1 Survey Questions

While the names in the identity roster were chosen to avoid ambiguities in which ethnic group each name was associated with, the name generator enjoyed no such advantage since names were supplied by the respondents. To assess the accuracy of our name classification, I had a second team of Bosnian research assistants belonging to a different ethnic/religious group code the reported names as well. Intercoder reliability was 80–82% depending on how I resolved ties between names belonging to multiple groups, and of the 564 unique names, the two teams were unable to classify only 14 and 4 names, respectively. Counts based on my second coder’s

classifications led to nearly identical results, with an ingroup mean of 6.15 (versus 6.16) and an outgroup mean of 4.73 (versus 4.67). The results of the identity roster and the name generator questions point to the same finding: prewar Bosnians displayed strong ethnic/religious homophily (the tendency to forge ties within one’s own group), but still have a substantial number of cross-group ties. This finding lines up with almost all secondary sources and, furthermore, indicates that identity rosters can be a valid instrument for measuring homophily in this context.

The total number of alters identified through the identity roster and name generator are correlated, though not so strongly as to be redundant ($r \approx 0.43$). Interestingly, the correlation is actually stronger for outgroup names ($r \approx 0.55$) despite there being less data with only 20 roster names instead of 30 to choose from. This begs the question of what exactly we are measuring in each case. The identity roster asked respondents if they knew someone by that name well enough that they would have greeted each other, a fairly low bar for tie strength. The type of relationship was not specified. The name generator asked explicitly for “friends” and further stipulated that they should be friends whom the respondent had “felt close to or spent a lot of time socializing with” before the war. Thus, we might expect a stronger correlation when we subset the identity roster names by tie type (respondents were asked in a follow-up question which of the names they had said yes to were relatives, friends, close friends, or acquaintances). Yet the correlation becomes weaker when we omit acquaintances, dropping from $r \approx 0.43$ to 0.31. While this may be due solely to the increased noisiness of the data, it is important not to discount the fact that these two questions measure closeness in different ways and, thus, may correspond to somewhat different phenomena.

C.2 Intermarriage Statistics

To measure intermarriage, I report *percent mixed families* based on what proportion of two-parent nuclear families included more than one ethnic group.

Based on marriage registration records, [Botev \(1994\)](#) reports that new marriages between persons of different ethnicities fluctuated between 9.5% and 12.2% over the 30 years prior to the war with no consistent upward or downward trend. In this regard, Bosnia mirrored the other Yugoslav republics which collectively exhibited an exogamy (intermarriage) rate 1-2 percentage points higher. As mentioned in the text, the intermarriage rate I found using the 1991 census data was lower: 7.8%. There are a number of ways to explain this discrepancy, including unregistered marriages being more common in rural, highly traditional settings with a strong endogamous preference. This theme came up in discussions with local registrars during my trips to collect data from marriage records in rural municipal offices across Bosnia. However, [Botev \(1994\)](#) contends, and I am inclined to agree, that such marriages were rare enough in the socialist

CUMULATIVE HISTOGRAM OF INTERMARRIAGE RATE BY TOWNSHIP

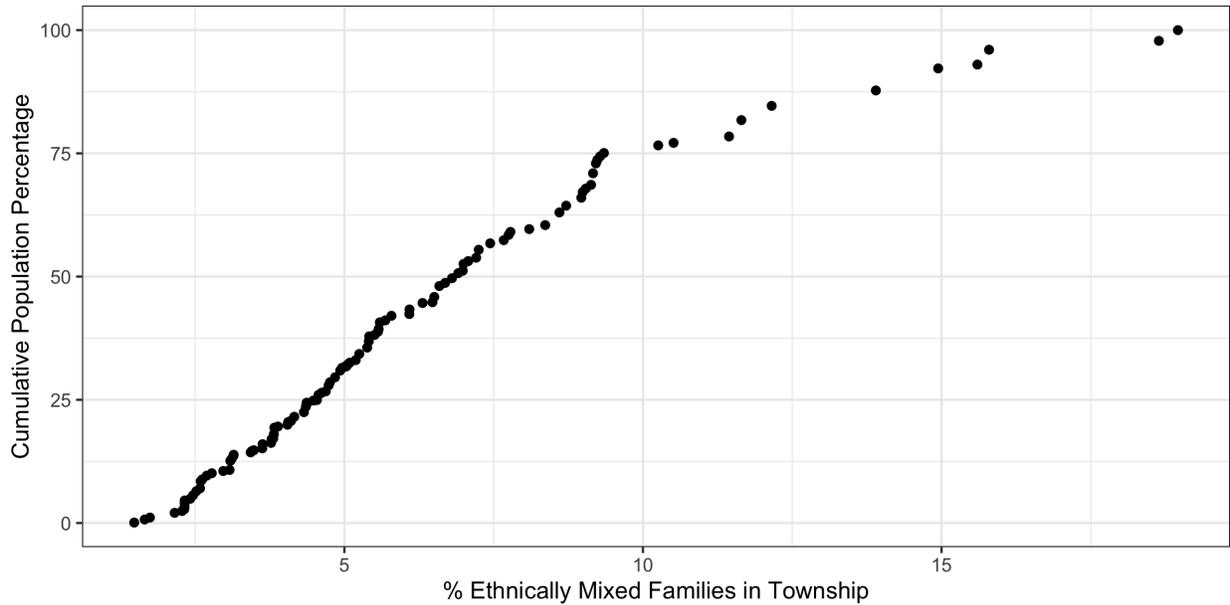


Figure 7: Each point represents a township, and its coordinates should be read as follows: $y\%$ of the population lived in townships where no more than $x\%$ of families were ethnically mixed.

period not to exert a substantial bias. Couples who married before 1962, when Botev’s data begins, may have been less likely to intermarry, owing to greater religiosity prior to Tito’s socialist regime and bitter ethnic resentments in the decade following WWII. Another possibility is that exogamous marriages result in a higher divorce rate and divorcées are reluctant to marry outside their group the second time around. However, the most likely explanation among couples who declared different ethnicities at the time of their wedding was that one spouse (most likely the woman) later adopted the other’s ethnic identity, at least for the purposes of the 1991 census. Nevertheless, the gap is not large.

D Outcome Variable Details

The categories of assistance read aloud were as follows (I have added the variable names in italics):

1. *Provisions*: Food, Money, Medicine, or Supplies
2. *Documents*: Diploma, ID, or other important documents
3. *Shelter*: A place to live or a place to hide
4. *Transport*: Transportation to a safer place or to get medical treatment
5. *Warning*: A warning that they were at risk of being arrested or attacked

6. *Intervention*: Prevented armed men from arresting, hurting, or killing someone

Some individuals helped in ways that do not fall into any of these categories. However, due to the variation in what respondents believe constituted “assistance,” it did not seem useful to have an open-ended category.¹²

E Model Selection and Robustness

Given the number of variables introduced in the Section 3, the modeling choices available are considerable. For many types of studies, the best practice is for the researcher to preregister what models she plans to fit ahead of time in order to prevent herself from only reporting those models which are significant (p-hacking) or choosing which models to test based on which earlier models show promise in such a way that leads her to chasing after spurious results (Gelman and Loken (2013)’s “garden of forking paths.”) However, for an exploratory project such as this where baseline estimates do not exist and hypotheses have yet to be developed, preregistration can prove too constraining to allow the development of new theories and reasonable hypotheses to test. Therefore, I make do with the second-best option: propose models that correspond to well-developed theories and then choose among them in a principled and systematic way. Prior literature about Holocaust rescuers and high-risk activism do suggest what variables to test; however, with only 100 individuals in my sample, my data is too underpowered to test all those variables simultaneously. Therefore, I turn instead to an automatic model selection algorithm to select an ensemble of models optimized to meet a certain criterion.

Because my outcomes for assistance are all binary, I settled on running a logistic regression. The `glmulti` package in the R programming language allows for the automated selection among generalized linear models. The `glmulti` function starts with a user supplied list of variables and randomly tests different combinations of them, ranking each resulting model according to an information criterion such as the AIC, BIC, etc. The goal is to simultaneously maximizing explanatory power while avoiding overfitting (Calcagno 2010). I set up the `glmulti` to optimize according to the AICc (Akaike Information Criterion corrected for small samples), which is recommended for logistic regressions on samples of this size. One major advantage of `glmulti` is that the function returns not just the best model but the top n models (in my case 100). The function assigns each variable an importance score, which is the overall number of top models the variable appears in, weighted by each model’s AICc (see Figure 8). Thus, I am able to see which variables are robust to slight perturbations in model specification, avoiding those for which the evidence is not compelling.

¹²For instance, one category that came up in the qualitative interview that I did not include in the survey was safeguarding the property of those who had fled. It seems plausible that some respondents who had engaged in safeguarding wouldn’t think to mention it while others would; thus, the proportion of positive responses for a category without a clear definition of the phenomenon being asked would not be interpretable.

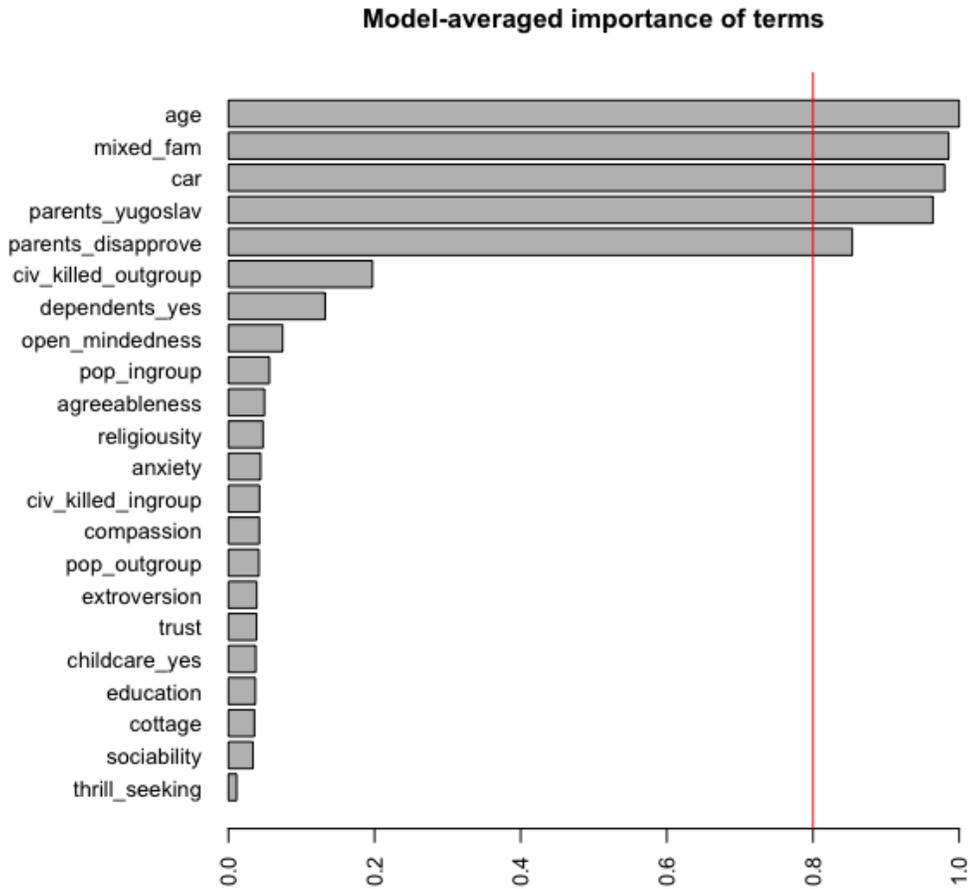


Figure 8: Results of a genetic algorithm for model selection. Terms with a weight higher than 0.8 were chosen for the final model to use in the paper.

Fortunately in my case, there was clear separation between the variables with high importance scores and low ones. Because I am interested in the effect of the network-related variables listed in Section 3, I included only one on such predictor in each model,¹³ and let the algorithm choose the control variables. As it happened, the predictor in interest was nearly always included; when it wasn't, I added it anyway to confirm there were null effects. For each of these predictors, I ran the search process separately for each outcome (*help*, *gave help*, *received help*), though the variables selected for the *help* and *gave help* outcomes were identical across all sets of models.

For robustness checks, I compared the results of the best model I had chosen with the weighted average of the top 100 models the algorithm returned. In most cases, the predictor of interest had about the same significance and effect size in both. In the case of *outgroup names*, I also tried manually adding in sets of predictors sequentially and noted that the coefficient estimates remained stable and continued to be

¹³Except in the case of the tie strength analysis where it made sense to compare strong and weak ties against one another.

significant until I reached approximately 20 variables at which point the model became too underpowered for nearly anything to have p-value of less than 0.05. Even in the full model with all 24 predictors, however, the coefficient estimate for *outgroup names* scarcely changed. I also compare the outcomes under different types of robust standard errors (HC1, HC2, and HC3) and with probit regression. Results are generally robust to these changes and are available upon request.

F Results for Capacity and Willingness Variables

Although I tested two dozen capacity and willingness variables, only a few displayed a statistically meaningful relationship with assistance. The machine learning algorithm used in model selection (see Appendix C) consistently selected the same set of four control variables from among the 24 tested in all *gave help* models. With one exception, they show consistent effects sizes and statistical significance no matter which network-related variable was included. Of the capacity variables, *age* and *car* showed up in all six models, while *gender*, *cottage*, and the biographical availability variables of *dependents* and *childcare* did not show meaningful effects. Respondents who were older at the time of the war were more likely to give assistance. This effect is likely driven in large part by general inability of children to give assistance, though the correlation between assistance and age continues into adulthood. When it comes to receiving assistance, *age* has too little explanatory power for the algorithm to select in Models 4 and 5 and has a significant but diminished effect in Model 6. Owning a car is positively associated with cross-group assistance in all six models, though its effect on receiving help is harder to pinpoint. Were the effect only significant for giving help, we might reasonably infer that it is the car itself that plays a role in providing assistance. However, since it also affects receiving help, we are left wondering whether it is perhaps also serving as a proxy for wealth, occupation, or some other underlying variable that makes receiving help more likely.

Two willingness variables were significant in almost all the models: *parents disapprove* and *parents Yugoslav*. As expected, offspring of parents who disapproved of intermarriage before the war were less likely to give assistance, while those whose parents considered it important to be Yugoslav were more likely to help. Notably, the effects remain when *mixed family* is included in the regression (Model 3). Thus, these parent-based variables that seem to be operating through a willingness mechanism are not merely proxies for having a diverse family network. One somewhat surprising finding is that both of these variables remain significant and exert even stronger effects on *received help*. Although rescue scholars have generally examined willingness variables for context of helpers, not recipients, it is entirely possible these factors make a person more willing to seek assistance from the outgroup, particularly in cases where ingroup assistance is also an option. [Finkel \(2017\)](#), for instance, shows that Eastern European Jews who were more assimilated were

more likely to pursue outgroup help as a survival strategy during the Holocaust, while those who were less assimilated tended to rely on wealthier or more powerful members of their own group. Finkel attributes these different survival strategies largely to network factors, yet, ironically, my analysis shows that something more than that is likely going on, at least in the case of Bosnia. Perhaps because most Bosnians lived under conditions that were closer to civil war than to genocide, their ingroup options were more plentiful and the consequences of getting no help less dire. In that case, ingroup and outgroup help would be direct substitutes, and those who felt less comfortable around outgroup members would simply turn to ingroup alters instead. Alternatively, if one's own parents did not identify as Yugoslav and did not approve of intermarriage, perhaps that is a sign that outgroup members in the same community didn't either. Therefore, although we only have data on the parents of the recipient in these last three models, these variables could be signifying a decreased willingness to help among their outgroup alters. Model 4 provides some evidence for this conjecture: *ingroup names* is associated with a *decreased* likelihood of receiving assistance. Even when holding the number of outgroup alters constant, having more ingroup alters (or perhaps, having a greater fraction of one's alters be from the ingroup) may lead one to appeal to an ingroup alter for aid instead of an outgroup alter.

Two items for the BFI personality test show up: *sociability* and *agreeableness* show up in the *received help* models, though there is not a clear reason why these pro-social personality traits should make receiving assistance *less* likely. Likewise, *religiosity* shows up in Model 4 with a strong and substantial effect, but it's not clear why someone whose parents were *more* religious should be more likely to get but not give assistance. A higher number of *outgroup [civilians] killed* is associated with less likelihood of assistance in Models 4 and 5, perhaps because the more outgroup members were killed, the more likely your group was to be in control of that township, thus obviating the need for receiving assistance. Yet *ingroup civilians killed* strangely does not hold notable explanatory power. Finally, ingroup population was selected in exactly one model, and its p-value does not come close to being significant.

G Robustness Check: Ethnic Defection

Ethnic defection refers to a related set of cleavage-defying behaviors including joining the outgroup's armed forces as a combatant (Kalyvas 2008), providing intelligence or logistical support to those forces (Lyal 2010; Staniland 2012a), deliberately choosing to remain in a place controlled by those forces and live among that group, and adopting that group's identity as one's own. Such behavior is distinct from cross-group assistance in that it is not dyadic; while we can identify the individuals performing the service, they are providing a public good in a sense, so we cannot identify specific beneficiaries. However, the behavior is similar to cross-group assistance in that it defies the conflict cleavage and entails the actor incurring risk or cost to

COEFFICIENT PLOT FOR THE EFFECTS OF TOWNSHIP LEVEL VARIABLES ON ETHNIC DEFECTION

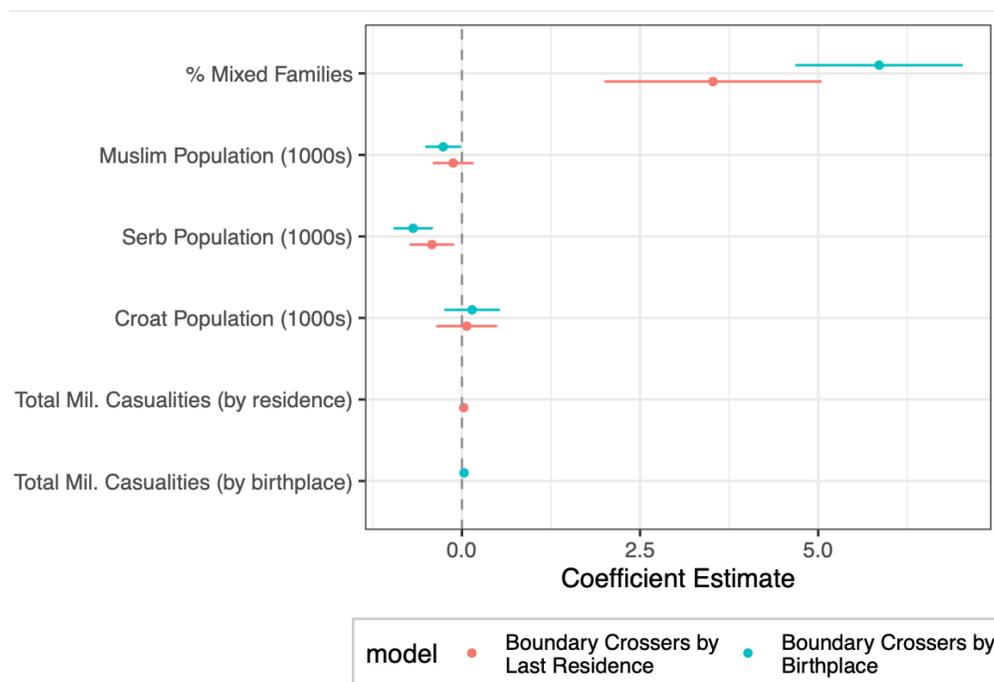


Figure 9: Coefficient plots for two linear regressions regressing *boundary crossers* on township demographics and fatalities. 95% confidence intervals shown.

benefit non-coethnics and thus be more likely to arise in the presence of substantial cross-group social capital. Furthermore, casualty records from the war offers an independently verified source of data. I extracted from *The Bosnian Book of the Dead* the total number of fatalities of non-Croats serving in the HVO and HV, non-Serbs serving in the JNA and VRS, and non-Muslims serving in the ARBiH and APZB, the six major armed forces of the war. I refer to this variable as *boundary crossers* and aggregate it two different ways: by township of birth and township of last residence. To measure cross-group social capital, I rely on Bosnian census data at the township level.

Figure 9 shows that *Percent Mixed Families*, our intermarriage variable, strongly and robustly predicts boundary crosser fatalities. Since I have no a priori reason to assume the distribution of boundary crosser military fatalities should be substantially different from the distribution of boundary crossers serving in the military, I used the former as a proxy for the latter. Thus, the greater the density of cross-group ties in a community, as measured by mixed families, the more likely people were to join an army primarily composed of the outgroup. This reinforces the degree–density hypothesis and suggests that networks can promote other cleavage-defying behaviors besides cross-group assistance. Individual cross-group social capital makes less sense as an explanation in this context, since there is no one individual who is the recipient. However, cross-group social capital at the community level still has explanatory value. Individuals embedded in a

dense web of mutual obligations and feelings of affection may feel pulled to serve the community as a whole through military service. At the same time, the fact that the percentage of mixed families in one's birthplace has an even stronger effect than the percentage of mixed families in one's place of last residence suggests there is something even stronger going on. Networks with high cross-group density, it appears, leave a lasting imprint on the minds of their members, even after those individuals have left the community and moved elsewhere. This suggests networks are operating through a willingness mechanism. The capacity of alters seems immaterial, but one's feelings towards them—perhaps moderated by tie strength—have a durable impact on wartime behavior years or decades later.¹⁴

¹⁴Serb population is negatively correlated with ethnic defection in both models. One likely explanation is that the Serb separatist forces were less willing to take in recruits of other ethnicities than the Muslim-dominated Bosnian army or Croat HVO forces. Townships with a lot of Serbs rapidly fell under Serb army control and, thus, Muslims and Croats living there had fewer opportunities to become boundary crossers.