



Exposure to collective gender-based violence causes intimate partner violence

Wolfgang Stojetz¹ & Tilman Brück²

HiCN Working Paper 389

March 2023

Keywords: war; gender-based violence; intimate partner violence; wartime sexual violence; ex-combatants; demobilization

JEL classification: D74; J12; J16

Abstract

Globally, one in three women experience intimate partner violence (IPV) over their lifetimes. Yet, the factors that cause men to commit IPV remain poorly understood. We propose and test a causal long-term link from past exposure to gender-based collective violence to violent behavior against an intimate partner. Combining novel survey data from Angolan war veteran families and a natural experiment, we find that exposure to sexual violence by armed groups against women makes male veterans about 30 percentage points more likely to commit physical – but not sexual – violence against a female intimate partner 18 years later (on average). Our results are not consistent with standard explanations of IPV based on group norms and intra-household bargaining. Instead, we attribute the effect to a lasting reduction in self-control skills. These findings challenge standard approaches to preventing IPV and emphasize the potential of working with men, especially after episodes of collective violence.

¹ ISDC – International Security and Development Center, Berlin, Germany; and Humboldt University Berlin, Germany. Email: stojetz@isdc.org (corresponding author).

² ISDC – International Security and Development Center, Berlin, Germany; Leibniz Institute of Vegetable and Ornamental Crops, Großbeeren, Germany; and Humboldt University Berlin, Germany.

Acknowledgements: For comments and discussions we thank Stathis Kalyvas, Jochen Kluge, Lukas Menkhoff, Anastasia Shesterinina, John Spall, Nikolaus Wolf, and conference and seminar participants at AfK Methods, ASSA, CSAE, EEA, the Harvard-MIT-Yale Conference on Political Violence, HiCN, the Lindau Meeting in Economic Sciences, Lisbon, Monash Prato, NEPS, UNU-MERIT, USIP, VfS, VfS AEL, and Yale. We are grateful to Teresa McIntyre and Ângela Maia for sharing several survey modules. For project funding we thank the United States Institute of Peace and the Portuguese Research Council, for fieldwork assistance we thank Development Workshop Angola. We received IRB approval for this research under IRB Services Protocol USIP-070-10F (6659). Stojetz also thanks Yale University and the German Institute for Economic Research for institutional support, and gratefully acknowledges financial support by Humboldt University and the Fulbright program.

1 Introduction

Why do so many men harm their intimate partners? Globally, one in three women experience domestic violence in their lifetime (WHO, 2013; Devries et al., 2013), creating enormous risks for victims and future generations (Pollak, 2004; Aizer, 2011; Sabia, Dills, and DeSimone, 2013).¹ The welfare cost of domestic violence is estimated at around 5% of global GDP – about five times that of wars (Fearon and Hoeffler, 2014). However, identifying reliable policies of preventing and reducing domestic violence is proving difficult (Iyengar, 2009; Bobonis, González-Brenes, and Castro, 2013; Ellsberg et al., 2015). A central limitation is that the individual-level factors that lead men to commit violent acts against intimate partners are not well understood.

Most existing research into the drivers of intimate partner violence (IPV) builds on the notion that the socio-cultural environment can govern human behavior via ‘mental models’ (North, 1994; Fehr and Hoff, 2011). This perspective attributes acts of IPV to prevailing norms and beliefs, primarily those of gender identity and relations (Bloch and Rao, 2002; Dagirmanjian et al., 2016). The main alternative class of explanations is based on intra-household models of bargaining between two rational partners, assuming that a woman accepts violence by the man in exchange for material compensation (Farmer and Tiefenthaler, 1996, 1997; Aizer, 2010; Anderberg et al., 2016). By contrast, more recent perspectives emphasize the relevance of intra-personal and situational factors for violent behaviors. Yet, scholars across disciplines lament that research on IPV in that vein is “uncohesive” (Berscheid and Regan, 2005; DeWall, Anderson, and Bushman, 2011) and that “there is little conceptual organization regarding how and why risk factors influence IPV” (DeWall, Anderson, and Bushman, 2011, p. 247). Causally identified impacts of variation in intra-personal factors are particularly rare across disciplines.²

This paper studies the causal long-run impacts of individual experiences on intimate partner violence. Specifically, we focus on exposure to gendered collective violence and ask how wartime exposure to sexual violence against women affects veterans’ long-run propensity to commit intimate partner violence.³

We study the impact of exposure to wartime sexual violence for two main reasons. First, perpetrating or witnessing violence is often associated with engaging in violent behavior in

¹While the term “domestic violence” generally includes all violence between individuals within households, we will focus on violence against women by a male intimate partner and refer to intimate partner violence (IPV), spousal violence and domestic violence interchangeably. As common in academic and public debates, by domestic violence we primarily mean physical abuse, unless indicated otherwise.

²Card and Dahl (2011) introduce such a perspective into the economics literature formally, and provide a model and empirical evidence for the causal impact of short-term emotional cues on domestic abuse following upset losses by sports teams.

³By the “long-run” we mean more than a decade after a conflict ended. Our empirical analysis studies IPV 12 years after the end of the (Angolan) war and, on average, about 18.4 years after exposure to sexual violence by armed groups against civilian women during the war.

the future – ‘violence begets violence’ (Pollak, 2004; Littman and Paluck, 2015). Second, sexual violence is among the most intense and recurring forms of gender-based violence and war atrocities, affecting millions of people across the world as victims and perpetrators. Starting with events in Bosnia and Rwanda, awareness for wartime sexual violence in the global policy discourse has increased rapidly, and United Nations Security Council Resolution 1820 recognizes that “rape and other forms of sexual violence can constitute war crimes, crimes against humanity or a constitutive act with respect to genocide” (UN, 2008, 3). Nonetheless, armed groups around the world continue to perpetrate sexual violence, including contexts as diverse as Liberia, Eastern Congo, Colombia, Timor–Leste, Pakistan, or Iraq and Syria (Cohen, Green, and Wood, 2013).

Theoretically, it is a priori unclear whether a man’s exposure to collective sexual violence against women increases or decreases his long-run propensity to commit IPV. We propose and test five theoretical mechanisms: a shift in traditional norms of gender identity and relations, the creation of violent models, a decrease in self-control, psychological distress, and a reduction in intra-household bargaining power.

Testing the causal net relationship and the underlying mechanisms presents considerable data and identification challenges. First, high quality micro-level data on conflict experiences beyond exposure to battle-field violence are scarce (Brück et al., 2016). Second, unobserved individual-level factors may vary simultaneously with wartime exposure to sexual violence and domestic violence today. To overcome the data challenges, we collected detailed primary survey data on exposure to wartime sexual violence from 578 Angolan war veterans, and on IPV today from their intimate partners⁴ as part of a larger mixed-method research project. To establish causality, we rely on a natural experiment in Huambo province, the epicenter of the 1975-2002 Angolan Civil War.

Our survey dataset includes detailed information on war and military service experiences, pre-service background characteristics and post-war social behaviors in 2014. The questionnaire was strongly informed by twelve months of ethnographic research into social behaviors and wartime experiences among ex-combatants in Huambo province (Spall, 2015). To reduce bias from self-reporting committed acts of IPV, we rely on information from the man’s partner, collected in separate but simultaneous, private interviews with a female enumerator, and based on standard Demographic and Health Survey (DHS) protocols and survey instruments.

Our identification strategy exploits that the Angolan war created exogenous variation in how likely it was that an individual soldier would be exposed to wartime sexual violence against women. The war was characterized by mass militarization and military competition between two high-capacity actors, the *Movimento Popular de Libertação de Angola (MPLA)* government

⁴Here, we only retained respondents with co-habitant intimate partners on domestic violence reported in a private interview. We discuss issues of selection into singledom further below.

and the *União Nacional para a Independência Total de Angola (UNITA)* rebels. Throughout the war, both sides used their institutional capacity to enforce compulsory military service for all men at (approximately) the age of 20 in territories under their control, which changed often and extensively during the war. Universal conscription exogenously selected individuals into different military entry dates (based on their date of birth) and different armies (due to shifts in territorial control).

Specifically, we exploit that for a specific subset of the army and date of birth combinations the ex-ante risk of exposure to sexual violence was much higher than for all others. While most of the time levels of sexual violence were low for both groups, there were two brief spikes in wartime sexual violence by MPLA, which were more than two decades apart (one in the late 1970s and one in the late 1990s). The natural recruitment experiment thus suggests that men born about 20 years before one of the two spikes *and* recruited into MPLA (the “high-risk group”) were at a much higher risk of exposure than all other combinations of birth dates and armed groups.

Our central result is that a man’s exposure to wartime sexual violence against women as a soldier significantly increases his propensity to commit violence against a female intimate partner more than a decade after the end of the war and, on average, about 18 years after exposure. We provide evidence that this effect is not driven by local effects, omitted individual variables, measurement error, linear model specification, mis-specified inter-correlations in the error term or the choice of our preferred instrumental variable specification.

We present empirical evidence to argue that the underlying mechanism is a lasting reduction in self-control, fostering more impulsive and automatic responses to critical situations with their partner. Surprisingly, we find no support for alternative theories based on living up to norms of gender, social models of violence, distress or a reduction in economic bargaining power, which have dominated explanations of intimate partner violence in across disciplines. Our findings are consistent with an interpretation of IPV as ‘expressive’ behavior, where men intrinsically value the expression of violence and violent acts may arise as an unintended outcome.

Our theoretical arguments and empirical findings contribute to three literatures. First, we extend the literature on domestic violence. In addition to our contribution to understanding the long-term roots of domestic violence discussed above, our findings also inform debates about the *behavioral nature* of acts of domestic violence. Domestic violence is often assumed to be ‘instrumental’ behavior and used to control the partner, send signals, or extract resources, among other objectives (Bloch and Rao, 2002; Bobonis, González-Brenes, and Castro, 2013). Recent contributions – including modern bargaining models – increasingly advance that domestic violence may be expressive behavior, as defined in the previous paragraph (e.g. Aizer, 2010; Card and Dahl, 2011). Our findings provide supporting evidence

that domestic violence may indeed be expressive behavior (among veterans), originating from individual experiences in the past.

Second, our paper contributes to understanding the long-term drivers of human behavior among adults in two ways. Recent research into the causal roots of social and economic behaviors among adults has placed particular emphasis on experiences during the in-utero and early childhood stages (e.g. [Cunha and Heckman, 2007](#); [Shah and Steinberg, 2017](#); [Adhvaryu, Fenske, and Nyshadham, 2019](#)). We add evidence for lasting behavioral impacts of experiences during *early adulthood*. Moreover, we add evidence on the existence and nature of cycles of violence (e.g. [Erten and Keskin, 2019](#)).⁵ At the individual level, cycles of violence present a puzzle. On the one hand, exposure to violence is aversive and distressing for most people, and yet does it appear to make individuals more prone to violence in the future ([Littman and Paluck, 2015](#)). On the other hand, exposure to war violence sometimes seems to make individuals to behave more – rather than less – pro-socially and risk-averse ([Bauer et al., 2016](#); [Jakiela and Ozier, 2019](#)), which may deter some people from using violence. [Littman and Paluck \(2015\)](#) emphasize the particular need to develop a better understanding of how collectives, and armed groups in particular, trigger cycles of violence at the individual level. We use an original design to test a causal, long-term impact of previous exposure to violence on violent behavior today, establishing a novel and robust link from wartime sexual violence by armed groups to post-war domestic violence.

Third, our paper also contributes to identifying and quantifying the effects of wartime military service. Following [Angrist \(1990\)](#), a prominent body of literature has used conscription lotteries for service in World War II and the Vietnam War to produce convincing estimates of the causal impact of veteran status on individual outcomes, such as future employment, earnings, disability status, mortality, crime and marriage (e.g. [Siminski, 2013](#); [Angrist and Johnson, 2000](#); [Heerwig and Conley, 2013](#)). Some other papers consider the role of deployment to war zones among US veterans who served more recently ([Negrusa and Negrusa, 2014](#); [Cesur and Sabia, 2016](#)). Yet, the exact service characteristics and experiences underlying observed post-service differences remain unidentified and most analyses study veterans and non-veterans in a developed country.⁶ Our paper estimates the causal effect of a specific, prevalent, and

⁵Studying the links between initial experiences of violence and violent behavior in the future is complicated, however, by the fact that violent experiences in the first place will typically not occur randomly, which severely limits the availability of causal evidence ([Widom and Wilson, 2015](#)). In addition, measures of previous and current violent behavior need to rely on self-reporting, which invites systematic measurement error, and evidence from randomized controlled trials is obviously limited to simulating exposure to violence or maltreatment. For a review of the literatures on intergenerational transmission of violence and the relationship between the exposure to violence at a young age and perpetrating violence and violent crime later in life, see [Widom and Wilson \(2015\)](#). For a review of the extensive macroeconomic literature on cycles of violence and violent conflict, see [Blattman and Miguel \(2010\)](#).

⁶A notable contribution from developing countries is the paper by [Blattman and Annan \(2010\)](#), which compares labor market outcomes of former child soldiers and comparable non-child soldiers in Northern Uganda.

intense wartime military service experience on a household outcome, based on variation in a developing country population.

Our findings have important implications for policy and practice. In developing countries, policies aimed at reducing rates of domestic violence have predominantly been victim-centered. Yet, the effectiveness of such programs is mixed across contexts, and there is a risk that such programs provoke perverse effects, as partners of receiving support may react violently to these interventions and their implications (Heise, 2011; Ellsberg et al., 2015). Our findings emphasize the need for paying more attention to perpetrators and the factors internal to them that precipitate violent acts, especially in developing countries. Working with affected and at-risk males may then offer a fruitful additional route to reduce and prevent domestic violence by treating an underlying causal factor.

In addition, a large class of programs and interventions seeks to strengthen social cohesion in post-conflict societies. It has been shown that conflict legacies can last for decades (Verwimp, Justino, and Brück, 2019), and that both social and psychological factors need to be taken into account to deliver support that is both responsible and effective (Cilliers, Dube, and Siddiqi, 2016). Specifically, post-conflict reintegration programs for perpetrators often assume that former combatants pose a threat to political stability, and focus on individual-level economic assistance (Gilligan, Mvukiyehe, and Samii, 2013). Our study exposes important stability threats at the *family* level and can inform future assistance, tailored to and addressing the psychological conflict legacies carried by veterans, which is crucial to their own and their families' well-being. In fact, the negative impact channel we detect might be mitigated by innovative forms of psychotherapy.⁷

The rest of the paper is organized as follows. **Section 2** provides theoretical motivations for a lasting, causal effect of exposure to wartime sexual violence against women on the individual propensity to commit domestic violence. **Section 3** describes the Angolan context and the survey data we collected and analyze in this paper. **Section 4** presents the identification strategy, econometric specifications and assumptions of the empirical analysis. **Section 5** presents the main results. **Section 6** discusses the mechanisms underlying these results. **Section 7** concludes.

⁷Existing evidence from rigorously evaluated programs suggests that psychotherapy helped to reduce criminal and violent behaviors among disadvantaged youth in Chicago (Heller et al., 2017) and among ex-combatants in Liberia (Blattman, Jamison, and Sheridan, 2017).

2 From wartime sexual violence to post-war intimate partner violence

In this section we discuss how personal exposure to wartime sexual violence may plausibly precipitate intimate partner violence in the long run. We derive five potential mechanisms.

2.1 Group norms and models

Perspectives across disciplines view domestic abuse as a response to specific social norms and models.

Mechanism 1: Beliefs related to gender identity and relations. Men may use domestic violence strategically to assert manhood status, coercively control their partner and extract resources, among other things (e.g. [Macmillan and Gartner, 1999](#); [Bloch and Rao, 2002](#); [Dagirmanjian et al., 2016](#)). The root causes of intimate partner violence in these models are prevailing social norms, predominantly those of gender identity and relations.

In sociological and economic analyses of collectives, it is well established that experiences with armed groups and forces can change beliefs related to gender identity and relations (e.g. [Costa and Kahn, 2003](#); [Akerlof and Kranton, 2000](#)). For instance, there is ample evidence that group leaders sometime deliberately re-interpret existing gender stereotypes (e.g. [GIZ, 2009](#)).

Exposure to groups that promulgate such patriarchal values and norms can increase the risk of intimate partner violence, as predicted by theories such as the ‘male peer support theory’ ([DeKeseredy and Schwartz, 1993](#)). Exposure to wartime sexual violence against women may thus change gender beliefs, in a direction that provokes more violence against women.

Prediction: *If exposure to wartime sexual violence has a lasting effect on beliefs related to gender identity and relations, this mechanism predicts a positive effect on, i.e. more, domestic violence today.*

Mechanism 2: Models of violence. Social learning theory posits that individuals can also acquire *new* behaviors through observation, modeling and reinforcement contingencies in the context of social interactions ([Bandura, 1977](#)).⁸ Specifically, individuals may learn violent behavior from violent models ([Bandura, 1973](#)), as the parental models that make children more violent as an adult according to ‘intergenerational transmission theory’ ([Straus, Gelles, and Steinmetz, 1980](#); [Mihalic and Elliott, 1997](#)).

Such models of violence often arise within military collectives or are directly promoted by

⁸In the economics literature, see e.g. [Banerjee \(1992\)](#) and [Bikhchandani, Hirshleifer, and Welch \(1992\)](#).

group leaders (Littman and Paluck, 2015). With respect to gender-based violence, wartime sexual violence by armed groups may be tolerated, legitimized, justified, encouraged and even ordered (Wood, 2012). Social learning theory hence suggests that soldiers may ‘learn’ violence against women through either direct experience or vicariously through observing others’ behavior. The underlying learning process may be normative or non-normative. Akers (1998), for instance, distinguishes “the direct association and interaction with others and their conforming or deviant behavior” (*behavioral/interactional*) and “the different patterns of norms and values to which an individual is exposed through association” (*normative*).

These arguments suggest that exposure to sexual violence against women during the war may create corresponding models of violence. Exposed soldiers may internalize norms of violence against women (*normative*) or align their behavior (*non-normative*).

Prediction: *If exposure to wartime sexual violence instills lasting models of violence against women, this mechanism predicts a positive effect on, i.e. more, domestic violence today.*

2.2 Psychological pathways

Psychological and recent economic research advances that not all behavior – including interpersonal violence – is the result of cognitive deliberation. For instance, “visceral factors” like emotions may play a role (Loewenstein, 2000). Behavior can also be “automatic” (Shiffrin, Dumais, and Schneider, 1981; Heller et al., 2017) or “mindless” (Langer, 1989), especially under stress.

This suggests that intra-personal and situational factors may drive behavior, including intimate partner violence (e.g. Finkel et al., 2009; Angelucci, 2008; Johnson, 2010; Card and Dahl, 2011). The psychological drivers may originate from and be coded by experiences a long time ago, including exposure to traumatic events during military service. For instance, various studies suggest a long-term link between combat exposure and mental disorders (e.g. Dohrenwend et al., 2006). Based on these insights we hypothesize that a long-term, individual level causal impact on domestic abuse may operate via psychological pathways.

Mechanism 3: Self-control. Driven by visceral factors, people sometimes act against their norms and ideals and may rather experience a feeling of ‘out of control’ (Loewenstein, 1996).

Psychological ‘control theory’ and recent models in behavioral economics emphasize that emotional cues are intrinsic to human nature and that they govern behavior (Gottfredson and Hirschi, 1990; Loewenstein, 2000; Laibson, 2001). Low self-control – the ability to self-regulate behavior and inhibit impulses – may cause (in our case) men to lose control and commit possibly unintended violent acts (e.g. Anderson et al., 2015), including against intimate partners (e.g.

Card and Dahl, 2011).

Abundant research shows how self-control develops (or not) during childhood and adolescence (e.g. Heller et al., 2017) and more recent studies demonstrate that self-control is also malleable in adulthood (Blattman, Jamison, and Sheridan, 2017; Wang, Rao, and Houser, 2017). Across developmental stages, a variety of theories suggest that exposure to violence can harm self-control, such as the ‘depletion model’ (Muraven and Baumeister, 2000). Behavioral and neurological evidence support the existence of adverse effects of exposure to violence on self-control, both in the short- and long-terms, and for different forms of violence (e.g. Blair, 2010; Agnew et al., 2011; King, Lengua, and Monahan, 2013; Monahan et al., 2015; Imas, Kuhn, and Mironova, 2015).

Prediction: *If exposure to wartime sexual violence conditions reduces a soldier’s self-control in the long run, this mechanism predicts a positive effect on, i.e. more, domestic violence today.*

Mechanism 4: Distress. An alternative family of theories of domestic violence emphasizes the role of emotions to explain acts of interpersonal violence.

While wartime violence is prevalent, psychological accounts of wartime violence also emphasize that exposure to intense war violence is (usually) an aversive and distressing experience. Two common factors that deter many individuals from violent acts are “outcome aversion” (Miller and Cushman, 2013) and an “action aversion” (Miller, Hannikainen, and Cushman, 2014).⁹

Due to existing aversions, perpetrating or witnessing a violent act is often associated with substantial psychological and physiological distress. World War II soldiers, for instance, reported not to have fired or to have misfired deliberately even when they were in the line of fire themselves (e.g. Grossman, 1996). Sexual violence in particular is considered a ‘traumatic event’, and can cause psychological struggles and negative emotional states, which may become chronic and lasting (Dekel, Mandl, and Solomon, 2011). A burgeoning literature transcending disciplines reports that these struggles may improve social relationships via ‘post-traumatic growth’ (PTG) (Tedeschi and Calhoun, 1996); for a review in economics see Bauer et al. (2016). Negative emotional states include guilt and shame and have been shown to be correlated with PTG (Dekel et al., 2016b). This logic predicts that exposure to wartime sexual violence lowers the risk of post-war domestic abuse.

However, distress may also increase the risk of domestic violence, if the exposure to violence causes or contributes to a mental disorder (e.g. Dohrenwend et al., 2006; Cesur,

⁹The two different types of aversion distinguish concerns related to (executing) a harmful action, and those related to its consequences, or outcomes, such as empathy with the victim’s suffering.

Sabia, and Tekin, 2013). Various studies suggest that post-traumatic stress disorder (PTSD) among veterans tends to be associated with more – rather than less – domestic violence (e.g. Sherman et al., 2006). Dekel, Mandl, and Solomon (2011) show for former prisoners of war that PTSD and PTG share a number of jointly correlated factors. The average probability of PTSD following a traumatic event is less than 10% but significantly higher for ‘traumatic events of human design’, and more than 30% of (all) PTSD cases become chronic (Dekel et al., 2016a). This logic predicts less domestic violence today among veterans exposed to sexual violence.

Prediction: *If exposure to wartime sexual violence increases psychological distress in the long term, the effect on domestic violence today may be positive or negative.*

2.3 Economic bargaining

An alternative approach advanced by economists and criminologists models domestic violence as part of the bargaining processes between partners. Our final mechanism links exposure to wartime sexual violence to a man’s long-term intra-household bargaining power.

Mechanism 5: Male bargaining power. In collective perspectives on household decision-making, each household member (or partner) has their own preferences, and intra-household allocations are determined through a bargaining process.¹⁰ An individual’s welfare in case the household dissolves – their ‘outside option’ – determines their ability to influence household decisions – their ‘bargaining power’.¹¹ In consequence, power proxy variables, such as earnings ability, realized income or remarriage prospects, are assumed to furnish an individual more influence over intra-household decision-making and allocation.

Recent studies explicitly incorporate intimate partner violence into these economic models, in which two partners bargain over the level of abuse and the allocation of consumption. It is essentially assumed that a partner may accept violence in exchange for intra-household transfers. A key prediction of these models is that a man with *less* relative bargaining power commits *less* violence against the woman (e.g. Aizer, 2010). These results resonate with slightly different criminologist theories of ‘female exposure reduction’, which argue that better or more employment of the woman, i.e. a decrease in the male’s relative bargaining power, is associated with less abuse by the man (e.g. Dugan, Nagin, and Rosenfeld, 1999). Exposure to wartime sexual violence may affect human capital and labor market outcomes, possibly but no necessarily through one of the mechanisms above, and

¹⁰See e.g. Browning and Chiappori (1998), Chiappori (1988) and Blundell, Chiappori, and Meghir (2005).

¹¹See Chen and Woolley (2001) and Lundberg and Pollak (1993) for models of *within*-marriage outside options, and Lundberg and Pollak (1996) for an excellent general account of bargaining in marriage.

effectively reduce a male's economic bargaining power.

Prediction: *If exposure to wartime sexual violence has lasting negative effects on relative bargaining power, this mechanism predicts a negative effect on, i.e. less, domestic violence today.*

3 The Angolan Civil War

Between 1975 and 2002, two highly organized and capable military factions fiercely battled over territorial control and legitimizing their respective nation-building missions in the Angolan Civil War (e.g. [Pearce, 2011](#)): the MPLA (the government) and UNITA (the rebels). [Appendix A](#) provides further information on the two actors' origins and brief overviews of the war and post-war periods.

3.1 Relevance

Angola's 27-year civil war offers a suitable setting to study the link between wartime sexual violence and long-run IPV. Immediately after independence, the nation experienced a very long and intense war, where territorial control was highly volatile.¹² On the other hand, the civil war was fought by the same two factions throughout, both with sufficient capacity to enforce compulsory military service for young men ([Stojetz, 2021](#)). Both sides engaged frequently and often systematically with local populations under their control in a variety of constructive and destructive ways. These interactions between civilians and combatants ranged from peaceful policies such as the provision of public services by the armed group, protection from enemy violence and taxation schemes, to extensive violence against civilians, including mass killings and sexual abuse of civilian women (e.g. [Ziemke, 2008](#); [Parsons, 2006](#)). This suggests that the population of veterans in post-war Angola is a) extremely large, b) likely to offer substantial variation in timing of service and c) likely to exhibit large variation exposure to wartime sexual violence.

¹²As [Pearce \(2012\)](#) notes, the end of the war in 2002 marked the first time since independence that the government had at least notional control of the entire Angolan territory, including large areas it had not held in a long time or ever. At certain points during the war, the government held as little as 20% of its nominal territory.

3.2 Natural experiment in exposure to wartime sexual violence

3.2.1 Exogenous variation

Variation 1: Army. The literature on wartime politics in the Angolan Central Highlands argues *against* ‘selection’ into either side based on private incentives associated with rebel recruitment or mobilization into armed groups (as e.g. in Weinstein, 2007; Berman and Laitin, 2008). Historical accounts document that control of local territory during the war shifted frequently and concludes generally that “*a person’s first contact with any political formation was as likely to have been with UNITA as with the MPLA*” (Pearce, 2012, 463). (Pearce, 2009, 4) adds that “*political identity was a matter of necessity rather than of conviction. It is for this reason that I use the word ‘adherent’ rather than ‘supporter’ when referring to the people who lived under the control of one or other movement during the war, since ‘support’ suggests a degree of voluntary affiliation which misrepresents the relationship.*” These accounts suggest that the armed groups were able to exert strong control over territories and their populations. For young men, both groups enforced compulsory enlistment into the military throughout the war (Stojetz, 2021).

Information from our survey (see Section 4) on recruitment date by army and region presented in Figure A2 are consistent with these accounts.¹³ For each region, we observe considerable variation over time in how likely, i.e. frequent, it was to join a certain army. There is no region where individuals were consistently more likely to join one army than the other throughout the war. Even though both armies sometimes claimed control over pockets of territory in the same region at the same time, a negative correlation in the distribution over time between the two armies is apparent (conditional on region).

Table A1 presents estimates from a simple regression of the armed group indicator on a large set of family background characteristics, inspired by previous work on rebel recruitment (e.g. Blattman and Annan, 2010). The results suggest that *no* family background characteristic is a robust predictor of which army a soldier joined and reject joint significance, across specifications with and without fixed effects of recruitment date and region (and classical standard errors).¹⁴ In the absence of any fixed effects, the negative adjusted R^2 -value in column 1 demonstrates that the set of background variables has essentially no predictive power. Consistent with

¹³To produce these graphs, we divided the sample into five sub-samples based on the broad region where a soldier was recruited. The regions are the Center, North, West and East of Huambo province, and a fifth category, into which all soldiers are pooled who were recruited outside Huambo province (about 6.5% of the sample). We split each regional sub-sample by which army a soldier joined and plot the army-specific densities of entry date.

¹⁴It should be noted that the dependent variables denotes the army the soldier joined when he entered the military for the *first time*. More than 95% of all sampled veterans joined an armed group exactly once. Related results from unconditional comparisons and Bayesian Model Averaging over the full model space of covariate combinations confirm this finding, and will be discussed in Section 5.

Figure A2 the simultaneous inclusion and interaction of recruitment date and region dummy variables provide explanatory power of the model, while that of the background variables remains insignificant. Based on these findings, we conclude that the armed group indicator was plausibly determined exogenously.¹⁵

Variation 2: Date of birth. What determined the *date* of military entry? In state-controlled regions, military service was compulsory by law for men who were obligated to register for military service in the year of their 18th birthday. UNITA effectively built a ‘state in the state’ and enforced conscription in a very similar way (Stojetz, 2021). This means that we expect the date of military entry and date of birth to be highly correlated for soldiers of either side. Figure A3 shows the distribution of age at entry in our sample and confirms the expected concentration of mass entries in late teenage years, consistent with compulsory enlistment. The overall mean age at entry is 19.6 years. Figure A4 reveals substantial variation in date of birth, and Figure A5 confirms that there is a very strong linear relationship between date of birth and date of military entry in both armies. This means that a) date of birth is a robust mean predictor of the date of enlistment, and b) we should see a similar pattern in exposure to wartime sexual violence over date of birth as over date of entry (shifted by 20 years, the rounded overall mean of age at entry).

Identifying variation: Army x date of birth. For identification we exploit the *interaction* of a man’s date of birth and the army he joined as a source of natural variation in the (ex-ante) likelihood of individual exposure to sexual violence. Dates of birth will be grouped into eight half-decades, as discussed in the next section.¹⁶ As both date of birth and army are plausibly determined exogenously, so are interactions between the two. It is worth noting that this holds even if doubts remained whether or not the armed group assignment was determined exogenously.¹⁷ Technically, the strategy follows the same logic as a difference(s)-in-differences estimator: we hypothesize that the expected difference in exposure to sexual violence due to army assignment varies with (grouped) dates of birth.¹⁸ Econometrically, we exclude the interaction of the army and (grouped) birth-cohort indicators

¹⁵We acknowledge that concerns may remain. We discuss these below.

¹⁶We use half-decades as a natural form of groups, avoiding concerns related to ‘hand-picking’ time intervals.

¹⁷A few recent papers have logically similar arguments for using an interaction of two variables as an instrumental variable. Examples include the interaction of local rainfall and physical distance of households to health centers (Adhvaryu and Nyshadham, 2015) and the interaction of US wheat production and a country’s likelihood of being a US food aid recipient (Nunn and Qian, 2014). A main difference is that in these strategies one of the two interacted variables is very likely not determined exogenously, while in our case both plausibly are.

¹⁸The mechanics of the instrumental variation are similar in spirit to studies exploiting wartime lotteries such as the Vietnam draft in the US: the effect of a randomized outcome (a lottery number) depends on the year of birth (e.g. Angrist, 1990; Angrist and Chen, 2011).

in the ‘second-stage’ of IV-estimation while including the main terms. In the next section we motivate the origins of the variation in the risk of exposure, introduce the grouping of birth-cohorts and discuss threats to the excludability of the instrumental variation.

3.2.2 Group-level variation in sexual violence against civilians

Why and when do armed groups perpetrate (more) sexual violence? While rigorous, statistical evidence is rare, recent studies emphasize variation in incidence based on the institutional make-up of the perpetrating organization (see e.g. [Cohen, 2013](#); [Cohen and Nordås, 2014](#); [Wood, 2015](#)). We define a ‘high-capacity actor’ as either a sovereign state or a rebel organization that has the capacity to establish bounded monopolies of force and enforce ‘state-like’ public policies (locally), such as public good provision, taxation and compulsory military service. The two armed groups in Angola were both high-capacity actors.

We draw on two insights from the literature. First, several studies document that high-capacity actors fighting a civil war tend to be less likely – on average – to perpetrate sexual violence against civilians, compared to armed groups of different nature. For instance, [Wood \(2006, 331\)](#) contends that “where insurgent groups depend on the provision of support (supplies, intelligence) from civilians and aspire to govern those civilians, they do not engage in sexual violence against those civilians if they have a reasonably effective command structure.” Second, wartime sexual violence can be a strategic ‘weapon of war’, as is often emphasized in the policy sphere. For example, UN Security Council Resolution 1820 states that sexual violence can be “used or commissioned as a tactic of war in order to deliberately target civilians or as a part of a widespread or systematic attack against civilian populations” ([UN, 2008](#)). While the recent literature emphasizes that wartime sexual violence against civilians may not necessarily be *ordered* ([Cohen, Green, and Wood, 2013](#)), there is evidence from many contexts that mass rape may under certain circumstances indeed serve as a key instrument of powerful armed groups to terrorize civilians (e.g. [Maedl, 2011](#)). High capacity actors are no exception: A recent body of scholarship documents that also high-capacity actors sometimes order or tolerate sexual violence by its soldiers against civilians as a tool of torment, torture or terror ([Wood, 2006](#); [Cohen, Green, and Wood, 2013](#)). Examples include systematic, collective rape by state actors targeted at certain sub-populations ([Green, 2006](#)) and strategic sexual violence against detainees or individuals suspected of supporting an insurgency ([Wood, 2006](#)).

We therefore expect to observe temporal variation in the extent of sexual violence by a high-capacity actor. More specifically, we expect to see moderate baseline levels over time, and high extent only during specific episodes. Based on this logic, we hypothesize that a soldier is more likely to serve in the perpetrating actor’s army during one of those episodes than others is also more likely to be exposed to (more) wartime sexual violence than others.

Sexual violence against civilians. The historical literature of the Angolan war confirms that the ‘baseline’ levels of sexual abuse by the two armed groups was comparably moderate, which is consistent with both seeking to legitimize their state-building missions. We exploit that there were two distinct spikes in systematic non-lethal violence against civilians by the MPLA, which plausibly included high levels of sexual violence and were about two decades apart.

Episode 1: The first episode of increased MPLA violence against civilians spans around five years in the late 1970s and early 1980s, when the MPLA faced *internal* tensions and some factions mounted challenges to their leadership (Pawson, 2014). Starting in 1977, these developments involved street demonstrations, the breaking open of a prison, the hijacking of a state radio station, and the killing of several prominent party leaders. A repressive large-scale campaign of violence and terror against civilians ensued. While often considered a key turning point in Angolan history, the details of these events remain highly contested in Angola and discussing them is still a taboo for many Angolans (Pearce, 2015b).

Episode 2: Secondly, the MPLA sharply increased violence against civilians during the final years of the war, after another peace agreement failed in 1998. This period is often referred to as “confusão,” roughly meaning imbroglío, and involved all actors of the war. Notably, an overwhelming number of reports by human rights organization and observers noticed and lamented specifically a change in behavior by the MPLA as a crucial factor feeding the *confusão* (e.g. Fonseca do Carmo et al., 2011). Reported atrocities by the MPLA included widespread sexual violence against civilian women, as described in a report by Human Rights Watch on the period between late 1999 and early 2000 (HRW, 2001, 2):

In the central highlands, allegations of rape by government soldiers increased. Soldiers broke into houses and raped women, or raped women they encountered working in the fields. These occurrences were widespread near military camps.

Other forms of violence against civilians. A key concern with the identification strategy is that sexual violence by the armed group may have coincided with other forms of violence against civilians, such as looting or massacres. To mitigate this issue, we exploit that the baseline level of sexual violence was similarly low for both groups throughout the war, while UNITA relied more systematically and more often on looting to feed its soldiers than MPLA. This means that we expect the overall difference in levels to be larger than for sexual violence and, most importantly, to vary very differently over time (and due to compulsory service also over dates of birth).

Similar concerns may be raised regarding massacres. Overall, it appears credible that the overall extent of massacres was low compared to other contexts due to the two groups’

ambition to win people's hearts and minds and build the state. The best documented episode of an MPLA massacre occurred in 1992, when MPLA orchestrated a few weeks of extreme violence after UNITA rejected the results of the presidential election. In the course of these events, the MPLA forces reportedly massacred tens of thousands of individuals suspected to be a member or supporter of UNITA, including in the so-called "Halloween Massacre" (Pearce, 2015a; Pereira, 1994). These events were distinct in nature due to their political motivation, targeting of men, and focus on fatal violence.

Overall, we argue that how other forms of group violence and the difference in levels between the groups vary time is significantly different compared to sexual violence.

Event-level data. Unfortunately, there is no systematic dataset of sexual violence during the Angolan War. Yet we can explore temporal variation in reported events of any form of violence against civilians by armed groups in the quantitative conflict event dataset by Ziemke (2008).¹⁹ Figure 1a plots all events of violence against civilians over time. Generally, we observe the three expected types of variation: (i) across armies, (ii) over time (within armies) and (iii) how the difference between armies varies over time. The blue line confirms the historical evidence for the MPLA massacres after the election in 1992 and the spike in large-scale sexual violence towards the end of the war ('Episode 2'). Given the sensitivity of and mysteries around the events in the late 1970s ('Episode 1'), it seems almost by construction that these events do not appear in the ACLED data.²⁰

3.2.3 Individual-level variation in the *likelihood of exposure* to sexual violence

At the individual level, we expect that soldiers who were recruited into the MPLA *and* served during one of the two episodes of spikes in wartime sexual violence by the MPLA were more likely to be exposed. Turning to our individual survey data, Figure 1b presents local polynomial smooth plots of the self-reported exposure to sexual violence against a civilian woman (ever) over date of military entry. Due to the limited number of observation per year of entry, the confidence bands are obviously large. Yet, the visual intuition is striking: the dashed blue line for MPLA recruits suggests local maxima in mean exposure for those who entered a) in the late 1970s and b) at the very end of the war, i.e. those who were most likely to be in the military

¹⁹This dataset has been integrated into the cross-country dataset compiled by the Armed Conflict Location & Event Data Project (ACLED). It is a conflict event database for the Angolan War and includes data on (any) reported operations against civilians, coded by date and actor. One caveat is that the information is almost exclusively based on Portuguese and Portuguese-speaking media sources, which likely favored the MPLA. This means that MPLA atrocities may be underreported.

²⁰As outlined above, we expect – on average – moderate levels violence against civilians by either actor. Yet, even with these priors, the total absence of MPLA violence against civilians up to 1990 is not plausible. At least quantitatively, these patterns need thus be interpreted with caution.

during Episodes 1 and 2 described above. As expected, we do *not* observe a local maximum for MPLA soldiers who joined the military shortly before or during the massacres after the 1992 elections.

Turning to UNITA recruits, we observe two things. First, the overall mean exposure of UNITA soldiers is not noticeably different from that of MPLA soldiers (as expected). Second, the temporal variation within the UNITA sample is very different compared to that in the MPLA sample. These patterns confirm that the interaction of whether a soldier joined the MPLA and whether or not he served (or was more likely to serve) in the late 1970s or at the very end of the war is an *informative* source of variation in the likelihood of exposure to sexual violence.

We now consider the exogenous variation in date of birth, which exploits that – due to conscription – individuals born about twenty years before a certain period were *more likely* to have served during that period.²¹ To avoid ‘fishing’ concerns, we categorize the date-of-birth distribution into eight bins (‘Bin 1’–‘Bin 8’) representing half decades, e.g. 1961-1965 or 1976-1980. Bin 1 (the earliest birth dates) and Bin 8 (the latest birth dates) span more years by construction. Using this metric, we expect that individuals born into YOB-Bins 3, 7 and 8 were most likely to be in an army during the two episodes of heightened sexual violence by the MPLA.

Figure 1c displays polynomial smooths of exposure to sexual violence over date of birth, which reveal the expected, similar patterns as for date of entry in Figure 1b. Within the MPLA sample those born about about twenty years before the late 1970s or the late 1990s/early 2000s turn out to be more likely to report exposure to wartime sexual violence. The actual difference it made for exposure whether an individual was recruited into the MPLA or UNITA varies strongly with date of birth. In summary, these unconditional relationships suggest that the Angolan Civil War created an informative and exogenous source of variation in exposure to sexual violence based on an interaction between the army the individual joined and his year of birth. To be precise, we expect that soldiers that joined the MPLA and were born into pooled birth cohort-bins 3, 7 and 8 were significantly more likely to be exposed to wartime sexual violence.

3.3 Identifying assumption

The identifying assumption is that the combination of being recruited into the MPLA plus being born into bins 3, 7 or 8 is uncorrelated with confounding factors, most importantly background traits that correlate with the proclivity for intimate partner violence and exposure to forms of wartime violence that increases the risk of intimate partner violence in the long term. To

²¹As note earlier, 20 years is the rounded overall mean of age at entry in the sample.

assess the theoretical arguments for the validity of the instrumental variation empirically, we collected detailed self-reported data on a) pre-service background just before recruitment and b) a host of wartime experiences other than sexual violence, especially other forms of violence against civilians such as looting and massacres. We discuss the empirical results in detail in [Section 5](#).

4 Empirical framework

4.1 Research design and data

The population of this study are all living males residing in Huambo who were ever part of an armed group during the Angolan War . The new survey data we use in this paper is based on a sample of 759 Angolan war veteran households from 34 different localities collected by the authors in the Study of Angolan Ex-Combatants (POEMA).²² The quantitative component of POEMA was supported by an anthropological companion study, which included twelve months of ethnographic fieldwork preceding the survey ([Spall, 2015](#)). Qualitative findings have been used to assess the validity our hypotheses, determine their relevance in the local context, refine the survey questionnaire design, interpret quantitative results and explore underlying mechanisms.

The survey dataset documents detailed information on war and military service experiences, pre-service background and post-war social, economic and political behaviors twelve years after the end of the war. Sampled veterans had to complete two interviews. First, a private household-level interview, together with their (main) cohabitant partner, if they had one at the time of the survey. Second, a private individual interview, which included extensive modules on pre-military service, military service and war experiences. Sampled veterans' (main) cohabitant partners were privately interviewed simultaneously, with a focus on intra-household and family outcomes, including domestic violence. Individual interviews with males were done by male enumerators, interviews with females by female enumerators.

The domestic violence measure is based on eight different acts of abuse, as suggested by the Demographic and Health Surveys Domestic Violence Module ([DHS, 2016](#)). Following the literature, our main outcome variable is a binary indicator (e.g. [La Mattina, 2017](#)), which equals one if the partner reports that she experienced at least one of these acts in the last 12 months. Our key explanatory variable is based on the veterans' wartime exposure to sexual violence against women. In our main specifications we use a binary measure, which equals one

²²The main analysis of this article focuses on the sub-sample of 578 veterans who have a partner. As a robustness check, we test for possible selection into "not having a partner." We do not find a significant relationship between exposure to wartime sexual violence and having a partner in 2014 (see [Table A2](#)).

if the veteran reports having been exposed to at least one situation where a civilian woman was abused during the war.²³

Appendix B provides more detailed information on the sampling strategy, the different interviews, motives and nature of our key measures, alternatively constructed indices of exposure, potential issues of recall bias, and key summary statistics (**Table A3**).

4.2 Econometric specifications

For the baseline estimates we specify linear models where the effect of interest β is estimated by OLS regression:²⁴

$$Domestic_i = \beta Exposure_i + \gamma' X_i + \varepsilon_i \quad (1)$$

Here, *Domestic* is an indicator of contemporaneous intimate partner violence; *Exposure* denotes exposure to wartime sexual violence; X is a flexible vector of controls variables; and ε is the error term.

The identifying assumption for a causal interpretation is that the exposure to wartime sexual violence and the error term are not correlated, conditional on control variables. The set of control variables we include is flexible across specifications. First, all specifications include location fixed effects to purge systematic variation across locales. Second, we add pre-military service – and therefore pre-treatment – family background characteristics and pre-treatment region fixed effects to control for pre-existing differences. Third, we add (potentially confounding) socioeconomic variables that have been linked to perpetrating partner violence and might simultaneously co-vary with wartime sexual violence. Fourth, we sequentially add potential wartime confounders, such as experiences of violence, again bearing in mind that these may not be determined exogenously themselves. That is, we are careful with adding variables that might be "bad controls" (e.g. [Angrist and Pischke, 2008](#)). Yet, even after carefully controlling for these factors, we may measure exposure to wartime sexual violence with systematic error or may not be able to control for all individual factors that are associated with participation in both wartime sexual violence and post-war domestic violence. Either would result in spurious estimates, and we rely on IV estimates to tackle and mitigate these concerns.

²³We prefer the binary indicator as it is less prone to measurement error, but also present results on frequency of exposure.

²⁴We prefer the baseline linear probability model because the statistical mechanics of clustering, the use of fixed effects and instrumental variable estimation are more straightforward for linear models (e.g. [Angrist and Pischke, 2008](#)). Because of the dichotomous nature of our dependent variable, we also estimate non-linear models as a robustness check.

IV. The instrumental-variables (IV) strategy is based on linear models, as in Equation (1), where the effect of interest β is estimated by IV/2SLS regression.²⁵ To test the relevance of the instrument(s), we use OLS to estimate δ , as specified in the following ‘first-stage’ equation:

$$Exposure_i = \delta(MPLA_i \times YOB - Bin_i) + \lambda X_i + \nu_i \quad (2)$$

Here, *Exposure* is exposure to wartime sexual violence against women; X is the full vector of control variables, including the main effects of *MPLA* and *YOB-Bin*; ν is the error term.

YOB-Bin is a categorical variable with eight categories, as explained in the previous section. Unless explicitly stated, the fifth bin is the omitted category.²⁶ As a single instrument is favorable in terms of bias as it is approximately median-unbiased (Hahn and Hausman, 2003; Angrist and Pischke, 2008), we choose our single best instrument for our main specifications, where we interact the *MPLA* dummy with a binary indicator *YOB-Bin378*. *YOB-Bin378* equals one if an individual year of birth falls into pooled birth cohort 3,7 or 8. We then produce IV/2SLS estimates of Equation 1 using this ‘first-stage’ equation.

5 Results

5.1 OLS

Table 1 reports results from linear regression. Column 1 presents the most parsimonious form of our effect of interest, relying only on local variation. We observe a positive and statistically significant impact of exposure to wartime sexual violence on post-war domestic violence.

Columns 2 to 7 show that the positive relationship is robust in terms of magnitude and statistical significance, when we include *comuna* fixed effects, pre-service background variables, a series of other control variables, with two-way clustered standard errors.²⁷ Notably, the general indicator of exposure to operations against civilians included in column 5 does not affect the coefficient of interest and is not statistically significant. The same is true for related

²⁵In the presence of heteroskedasticity or intra-cluster correlation, the IV coefficient estimates are consistent (yet inefficient), but their standard errors and the usual forms of the diagnostic tests are not. To achieve asymptotically correct inference, in our baseline specifications are robust standard errors, corrected for clustering at the pre-service and post-service location levels, as in OLS regression.

²⁶This bin was chosen as veterans born in these years were most likely to serve in the late 1980s which was characterized by large-scale conventional battlefield war and likely less interaction with civilians.

²⁷For the main specifications we follow Cameron, Gelbach, and Miller (2011) to estimate the standard error. Observations may not be independent within two sets of locations: where individuals reside, and where they were recruited from. We thus estimate robust standard errors clustered by the primary sampling unit (*bairro*) and municipality of recruitment. ‘Few cluster’ issues and alternative techniques to estimate the standard error are discussed later in this section and in more detail in Appendix C. As also suggested by Cameron, Gelbach, and Miller (2011), we rely on conservative inference and ignore stratification and survey weights (these less conservative results are very similar and readily available upon request).

indicators of exposure to other forms of intense war violence, which have been linked to other post-war outcomes (e.g. Blattman, 2009; Bellows and Miguel, 2009). These include witnessing fatal violence against enemy soldiers, fellow soldiers and civilians.

Further robustness. In Appendix C, we show that the main result is robust to classical, Huber-White, Moulton and wild cluster bootstrapped standard errors (Table A4) and non-linear model specifications (Table A5). Yet, the result might still not have a causal interpretation and could be driven by correlations with unobserved, confounding pre-service, service and post-service variables, or systematic measurement error. To mitigate these concerns we rely on IV-estimation, which must be balanced, however, against the inevitable loss of efficiency vis-à-vis OLS. Notably, if exposure to wartime sexual violence is actually not correlated at all with the error term, the asymptotic variance of the IV estimator is always larger than that of the OLS estimator.

Endogeneity. The two primary endogeneity concerns are that the extent of exposure to wartime sexual violence may be a) a function of individual background characteristics and b) correlated with other wartime experiences, especially exposure to other forms of violence. If these background characteristics have an impact on the outcome variables, the simple OLS estimate that do not deal with these effects will be biased.

Table A6 explores individual traits associated with exposure to sexual violence during the war. Based on our survey data, it compares raw means between the two subs-samples defined by the binary exposure to sexual violence-indicator. The top panel suggests that the two subs-samples do not differ much in terms of characteristics related to the military, including army joined, role, rank or age at entry. Exposed soldiers report slightly higher levels of self-assessed unit mortality rates and were more often part of a unit operating mostly in combat areas. The bottom panel presents summary statistics on the five-year-bins of pooled birth cohorts and shows that veterans belonging to the exposed sub-sample are not distributed differently than non-exposed counterparts across bins.

By contrast, the middle panel suggests that the exposed sub-sample self-reports systematically different individual pre-service traits than then non-exposed. A veteran exposed to sexual violence is more likely to report having been a student and having had any schooling at the time of recruitment. Based on subjective health assessments, self-reports suggest that exposed veterans were significantly stronger, physically and mentally.²⁸ We observe no differences in self-assessed overall health and slightly less reports of having had formal training among the exposed.

While this descriptive evidence needs be interpreted with caution, it suggests two central

²⁸Respondents were asked to compare the composition to that of other individuals of about their age at the time of recruitment.

implications. Military-related traits and the year-of-birth distribution do not differ substantially across the two sub-samples, while individual self-reported and -assessed traits do. The latter deepens concerns that simple OLS results may be spurious.

5.2 Informativeness of the instrument

Table 2 presents first-stage results. As expected, being with the MPLA and born in bins 3, 7 or 8 predicts a substantial increase in exposure to sexual violence, conditional on MPLA and bin main effects (columns 1 to 4). Columns 5,6 and 7 shows the results for the binary instrument. Columns 2 to 7 demonstrate that both the additive as well as the single instrument are strongly relevant, with and without the inclusion of (basic) pre-/post-treatment controls. The F -statistic exceeds the critical value of 10 in all specifications.

5.3 Validity of the instrument

As a first test to assess the validity of the instrument, **Table A7** compares raw means in individual characteristics between the sub-samples now defined by the binary instrument ('balance table').²⁹ Similar to the unconditional comparisons based on actual exposure to sexual violence in **Table A6**, military-related traits do not differ substantially across the two sub-samples, and now, by contrast, individual self-reported and -assessed traits do not differ systematically, either.

Sorting based on confounding background characteristics. **Table A8**, columns 1 and 2, confirm that no family background characteristic predicts the value of the instrument variable, with and without pre-service location fixed effects respectively. Columns 3 to 6 present results from a regression of the army dummy on family background characteristics for YOB-Bin378-soldiers and others separately. With and without pre-service location fixed effects, we find no substantial differences in 'selection' for these specific bins.

To eliminate concerns of the 'correct' specification of background covariates included in the model, we also report results from Bayesian Model Averaging (BMA), which provides a weighted average over the entire model space, i.e. all possible combinations of covariates. The BMA results confirm that *no* background characteristic predicts i) the value of the IV (**Figure A6**), ii) UNITA recruitment among those born into bins 3, 7 and 8 (**Figure A7a**) and among others (**Figure A7b**) or iii) birth into bins 3, 7 and 8 among UNITA recruits (**Figure A8a**) and among MPLA recruits (**Figure A8b**).

²⁹We omit the army and year-of-birth indicators on which the instrument is built.

The instrument is based on a higher *likelihood* of having served during army-specific spikes (via date of birth bins 3, 7 and 8) rather than having actually served. Nonetheless, we repeat the BMA analyses of [Figure A7](#) and [Figure A8](#) with date of entry rather than date of birth, to test whether UNITA recruits that actually joined during periods II and IV were systematically different. The results presented in [Figure A9](#) and [Figure A10](#) show that there were no differences in background across army and dates of entry, further corroborating the argument that there was no sorting based on background.

Simultaneous selection into confounding wartime experiences. Next, we draw on detailed data on exposure to specific forms of violence to test simultaneous selection into confounding experiences of violence by the IV. Specifically, we collected data on basic exposure to two broad classes of violence against civilians: fatal violence versus non-fatal violence.³⁰

[Figure 2](#) presents plots of local polynomial smooths of binary exposure to other forms of war violence over year of birth. For comparison, we include the sexual violence plot in [Figure 2a](#). Starting with variation in MPLA soldiers (dashed blue lines), these unconditional relationships reveal two main insights. As expected, mean experiences of fatal violence against civilians and of violence not directed at civilians ([Figure 2b](#), [Figure 2c](#)) vary very differently by year of birth compared to experiences of sexual violence against civilians. Second, the variation in exposure to civilian looting ([Figure 2a](#), right) appears somewhat similar to the variation in exposure to sexual violence among MPLA soldiers. Yet, [Figure 2b](#) qualitatively confirms that – in contrast to sexual violence – the overall level and trend among UNITA soldiers is *not at all* similar to those of MPLA soldiers. Rather, the graphs reflect that UNITA for most of the war relied much more and more often on looting than UNITA. The overall temporal variation of the mean difference between MPLA and UNITA soldier for looting is very different than that for sexual violence. This suggests that the instrumental variable is not correlated with a higher risk of exposure to looting and that we can isolate the effects of exposure to sexual violence from that of looting (and other war experiences).

The regression results displayed in [Table A9](#) provide corresponding quantitative evidence. The IV is not a relevant source of variation in exposure to looting and massacres (or related forms of wartime violence).

³⁰Ethnographic studies of the Angolan conflict and the political and historical analyses described above emphasized civilian massacres and looting. [Ziemke \(2008\)](#) reports strategic mass killings of civilians and argues that these were more likely after crucial battlefield losses. On the other hand, food is the most fundamental resource to sustain an armed group, and was, generally, often extremely scarce in the Angolan conflict ([UNICEF, 1998](#)). In our survey, 53% of respondents report that “often” or “very often” they were “that hungry or thirsty that you ate or drank things you would never have imagined to ever eat or drink.”³¹

5.4 IV

Table 3 displays IV results and compares them to the corresponding OLS coefficient from the same specifications. For efficiency reasons, we use the three interaction terms as additive instruments instead of collapsing them into a single instrument.³² The IV estimates (columns 2 and 5) confirm the positive effect of exposure to wartime sexual violence on domestic violence 12 years after the end of the war found by OLS (columns 1 and 4). IV estimation returns sizable and stable point estimates around .3, suggesting that results from OLS regression underestimate the magnitude of the effect.

As a robustness check, columns 3 and 6 present IV estimates from an alternative strategy, which uses mean exposure in cells defined by the specific armed group and YOB-bin combinations as an instrument for individual exposure. For example, exposure of a UNITA veteran born in YOB-Bin 4 is instrumented with the simple average exposure over all UNITA veterans born in YOB-Bin 4. The estimates remain qualitatively and quantitatively similar across instrument choices. The stability of the IV coefficient across alternative specifications further increases confidence in our results.

The results provide strong evidence that there is a causal link between exposure to wartime sexual violence and long run intimate partner violence. As the overall mean of domestic violence among veterans not exposed to wartime sexual violence is about .44, the various IV estimates suggest that the effect is substantial (with a stable coefficient of around .3).

6 Mechanisms of impact

Based on the theoretical arguments laid out in **Section 2**, a positive link between exposure to wartime sexual violence and post-war domestic violence is consistent with four mechanisms of impact: beliefs related to gender and masculinity (mechanism 1); learning from violent models (mechanism 2); a reduction in self-control (mechanism 3); and distress (mechanism 4). Even though the sign of the main effect is not consistent with the prediction by mechanism 5 (economic bargaining power), the effect may actually be present, too, but dominated by other mechanisms at work. The results reported and discussed in **Appendix C.2** provide evidence against the presence of mechanism 5 (**Table A10**). We now present and discuss evidence to assess the explanatory power and robustness of the first four impact mechanisms.

³²Using a single instrument yields nearly identical point estimates (not shown, but readily available upon request).

6.1 Social norms and models

Gender and masculinity. Based on a set of results, we do not find evidence that changes in beliefs related to gender identity and relations undergird the link between wartime sexual violence and post-war intimate partner violence.

Table 4 reports results on behaviors that are closely related to norms of gender and masculinity. The OLS and IV results document a very weak and inconclusive relationship with domineering: exposed veterans are not significantly more likely to try to control their partners' relationships with girl friends or family members. We next investigate whether the exposure to sexual violence affects how much veterans try to live up to (related) norms of masculinity. In an ethnographic companion study, [Spall \(2015\)](#) emphasizes the role of fathering and fatherhood in Angolan War veterans' pursuit of living up to masculine ideals. We therefore test whether exposed veterans are more supportive of or have better overall relationships with their children.

We do not find that exposure to wartime sexual violence has a notable effect. Based on the reports from veterans' partners, exposed veterans are not systematically in terms of supporting their children by the veteran or in the relationships between the veteran and his children (columns 5 to 8). The impacts are very small in magnitude and, if anything, slightly negative for support of children. These results reject a dominant role of a mechanism rooted in shifts in traditional beliefs about gender and masculinity.

[Appendix C.2](#) presents and discusses outcomes of marriage formation and outcomes. Differences in these dimensions could reflect mediation mechanisms based on gender norms or economic bargaining power.³³ Yet, veterans exposed wartime sexual violence do not differ meaningfully from non-exposed veterans in terms of marriage formation and outcomes, providing (further) evidence against such a mechanism ([Table A11](#), [Table A12](#)).

Table A13 analyses different forms of violence. Looking at physical and sexual components of domestic violence separately, columns 1 and 2 reveal that the positive main link is exclusively driven by *non-sexual* acts of IPV. Even considering that only about 12% of partners report having experienced sexual violence, the impact on such acts is very small in magnitude and not statistically significant. The strong effect on physical violence and absence of variation in sexual violence provide evidence against imitating or normative violence.

To test this conjecture further, we investigate impacts on non-gender based violence. If exposed soldiers were more prone to learning behavior from violent models, we may see similar effects for other violent behaviors and attitudes. The coefficient reported in column 3 suggests that exposed veterans are not more likely to agree with the use of violence by protesters in

³³In addition, differences in marriage outcomes might also be linked with differences in economic bargaining power.

political demonstrations. Yet, we find that exposed veterans are significantly more likely to agree that police are justified to respond violently to protests (column 4). By contrast, we find a negative effect on staying at a (hypothetical) protest that turns violent (column 7).³⁴ Finally, exposed veterans are not more likely to have engaged in illicit behavior or work as a security guard in the last four weeks – jobs that imply more violence than others.

In sum, we do not find evidence that the strong impact on gender-based violence is accompanied by similar effects on behavior or attitudes related to certain forms of political or street violence. While the results are only suggestive, they are consistent with the psychological typology of male “family only batterers” (Holtzworth-Munroe and Stuart, 1994), yet not necessarily limited to it.

Learning from models and unlearning. Table 5 reports insights into the role of eight specific factors for the impacts of exposure, which could be associated with learning from models: tenure with the armed group, armed group membership (MPLA or UNITA), time elapsed since leaving the group, formal demobilization, formal DDR program, ties with members from armed group units, the village-level share of exposed veterans (of sampled veterans in village) and the village-level share of veterans committing domestic violence (of sampled veterans in village). The only factor that significantly influences our effect of interest is the village-level share of exposed veterans. A higher share may contribute to sustain and reinforce imitating or normative behavior. Yet, residing in village with more exposed veterans diminishes – rather than increases – the effect of interest. Specifically, these results also provide further evidence that the effect of interest is persistent and suggest that it was not mitigated by the formal Angolan demobilization and reintegration processes.

Jointly, these findings provide substantive evidence against an effect based on exposure to social models of gender and/or violence.

6.2 Psychological and economic mechanisms

Self-control. Mechanism 3 is based on a lack of self-control in critical situations of discussions and conflict between partners. To better understand the extent of deliberation and cooperation among veteran couples, we first look at two potentially important and contentious topics: how to spend/save income generated by the woman and how many children to have. The binary outcome variables reported in columns 1–3 of equal 1 if the couple reports that both the partner and the veteran are involved in these processes. Two findings are apparent. First, deliberative mechanisms exist for the majority of households,

³⁴The specific question was: “Imagine that you are at a protest and it turns violent. Would you stay or would you leave?”

exceeding 80% for each outcome. Second, households of exposed veterans do seem to differ from other households in the degree of deliberation. We find no significant difference with respect to partner's income and reproduction.

Certain discussions may escalate into heated disputes, which in turn can trigger violent acts. **Figure 3** plots the relationship between the frequency of heated disputes and the incidence of intimate partner violence as reported by veterans' partners.³⁵ The figure confirms that more disputes are strongly associated more risk of with partner violence.

Based on reports by the veteran's partner, exposure to wartime sexual violence is associated with significantly more disputes in the last twelve months (column 4). Notably, the IV estimate (column 6) suggests that OLS significantly underestimates the strength of the link between exposure to wartime sexual violence and the frequency of heated disputes. This result suggests that the mechanism of impact involves a lack of self-regulation. To get a deeper understanding we study two proxy variables of self-regulation asked in the survey. We find that in the case of exposed veterans their partners are significantly more likely to report that the veteran can get angry or jealous when she interacts with other male adults. By contrast, we do not find any significant differences in general difficulties with concentration as reported by the veteran.

If the underlying effect is a lack of self-control, we expect to see an effect on multiple forms of physical violence against the partner, rather than on one specific type. **Table A14** displays results from separate regressions for each form of physical violence, using standard specifications. We observe that exposure to wartime sexual violence is a positive predictor of five out of seven items and the magnitude of the disaggregated effects is relatively similar across these items. Our main effect thus increases the risk for multiple forms of violence and is not driven primarily by the response in a single form of physical violence, strengthening the argument for a lack of self-control.

Distress. **Table 7** reports results on distress-related outcomes. Using the Rotterdam Symptom Checklist (RSCL), adapted to and validated for the Angolan context ([McIntyre and Gameiro, 2013](#); [Maia et al., 2011](#)), we find no evidence for higher levels of contemporary distress among exposed veterans. If anything, we find that exposed veterans are slightly *less* likely to suffer from psychological distress (column 1) or to show related physical symptoms (column 3). The results in columns 5 and 7 suggest that exposed veterans report similar levels of alcohol consumption and are less likely to smoke cigarettes. Instrumental variables estimation (columns 2, 4, 6 and 8) returns coefficient that have the same signs (with the exception of alcohol), but are imprecisely estimated. Yet, due to the clearly negative signs across measures we rule out a dominant role for psychological distress. In addition, these results are consistent

³⁵Question: "In the last 12 months, how often did you and your spouse have major disputes?". Answer options: "Very often", "Often", "From time to time", "Rarely", "Never".

with the weak impacts on bargaining power, which can reflect psychological well-being and the associated physical status.

6.3 Robustness

Our empirical results provide comprehensive evidence for a causal link between wartime sexual violence to post-war intimate partner violence based on a psychological pathway (self-control). Despite the results presented above, there may still be concerns about simultaneous selection into other forms of adverse experiences during the war, which might leave a psychological or socio-economic imprint driving violence against an intimate partner. Below, we aim to provide additional empirical evidence that directly addresses these concerns. To do so, we exploit the very detailed additional data that were collected on general personal experiences during the war and on general post-war economic and psychological well-being, as part of the larger research project.

Does the identifying variation predict personal experiences of extreme risk and various forms of violence during wartime military service? In [Table A15](#), we test if the identifying variation is linked with personal experiences of extreme danger (such as being ambushed or hit by a bullet) and other violence during military service (ranging from verbal abuse to being tortured to firing heavy weapons). Based on the detailed arguments above, there is no reason to believe that such correlations exist. The results shown in [Table A15](#) confirm this expectation. The identifying variation does not predict experiences of extreme risk or violence, complementing the results from general situations of violence reported in [Table A9](#).

Turning to general economic and psychological well-being, we start by comparing outcomes between those exposed and those not exposed to wartime sexual violence. Based on parsimonious specifications, [Table A16](#) suggests that there are some statistically significant differences in outcomes between the exposed and the non-exposed. These differences are modest and not systematic, and, if anything, they suggest that exposed veterans have slightly *higher* levels of well-being. For example, they are slightly more satisfied with their living standards and life in general. While only suggestive, these results are not consistent with negative imprints due to simultaneous selection into other forms of violence or with distress-based channels, to which economic stress could contribute.³⁶

Does the identifying variation predict general economic and psychological well-being? In [Table A17](#) and [Table A18](#) we present reduced-form results from parsimonious specifications. The results confirm that the identifying variation does not predict variation in any tested dimension of post-war well-being. This includes the distress measures analysed in the analyses on mechanisms as well as several other 'objective' measures of well-being, such as income, asset,

³⁶We thank an anonymous reviewer for drawing our attention to the potential role of economic stress.

education and work status, as well as self-reported subjective ratings of satisfaction with their current work situation, living standards, health and life overall. The estimated coefficients are consistently small in magnitude, differ in the sign, and are statistically insignificant.

In combination, these additional results corroborate the validity of the instrument for establishing that the documented link from exposure to sexual violence to intimate partner violence has a causal interpretation.

6.4 Intensive margin

While we believe that the binary indicator of exposure to wartime sexual violence is more reliable, Appendix C.3 presents and discusses results based on the number of situations reported. While speculative, the results displayed in Table A19 suggest an inverted-U relationship, where the main effect is driven by low and moderate exposure to wartime sexual violence. Extreme exposure, however, appears to induce high level of distress and reduce the risk of intimate partner violence.

In combination, these findings confirm that our main result is not driven by psychological distress (or well-being more generally) - neither as the main causal impact of wartime sexual violence nor by potentially confounding rival impacts of other forms of war and violence exposure.

7 Concluding remarks

In this paper, we study the long-term origins of domestic violence at the individual level. Using novel survey data from Angolan war veterans families and a natural experiment, we show that combatants' exposure to collective gender-based violence can causally increase their individual propensity to commit violence against an intimate partner decades later. We argue that this effect is caused by a lasting reduction in self-control.

For the average effect, we find no support for alternative theories based on living up to norms of gender and masculinity, the creation of violent models, distress or a reduction in economic bargaining power. In addition, we find that the strong impact on gender-based violence is not accompanied by meaningful effects on non-gender based violence. However, we find suggestive evidence that above a certain threshold of exposure intensity the main mechanism vanishes and is dominated by high psychological distress reducing violent behavior. These results challenge dominant interpretations of domestic violence as instrumental behavior.

The findings have important implications for policy and practice. After episodes of collective violence, efforts to stabilize societies have been shaped by fears that former members of armed

groups threaten social stability and cohesion. Yet, our study demonstrates that exposure to wartime violence may (also) create profound impacts at the family level, putting partners and families at risk. Moreover, policies to reduce or prevent domestic violence often operate via increases in female opportunity or gender inequality more generally. But these attempts have sometimes provoked unintended and perverse effects resulting in more, rather than less, violence. Our study emphasizes the need for paying more attention to active and potential perpetrators and the intra-personal origins of domestic violence.

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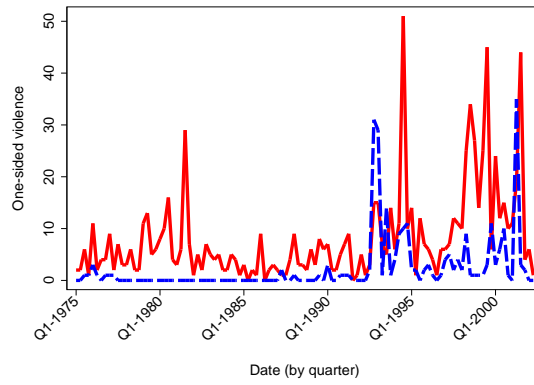
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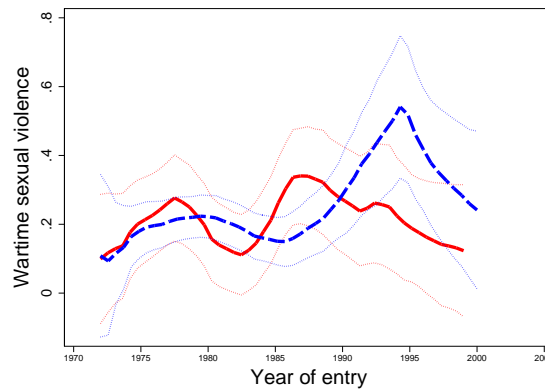
Figures

Figure 1: First-stage mechanism

(a) Event data: (Any) violence against civilians



(b) Wartime sexual violence against women over date of entry



(c) Wartime sexual violence against women over date of birth

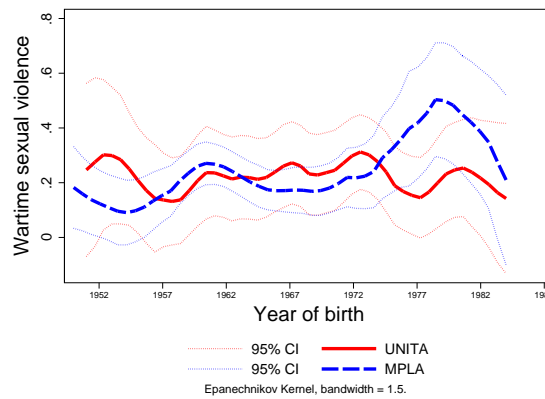
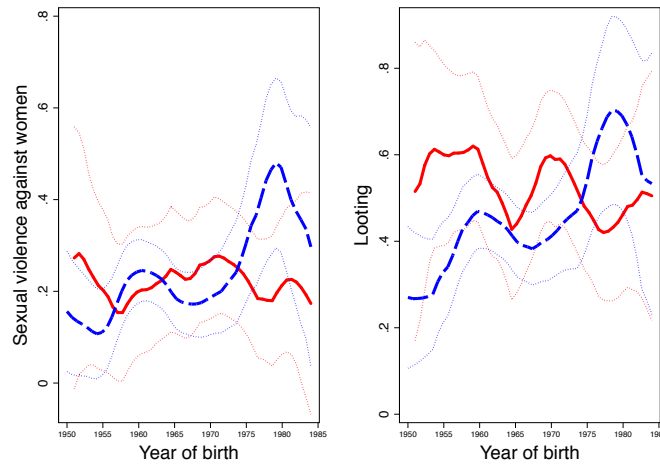
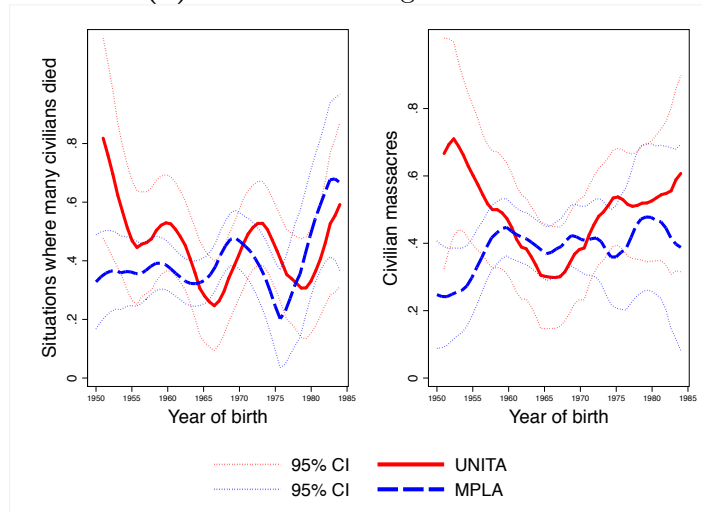


Figure 2: Exclusion restriction: different forms of war violence

(a) Non-fatal violence against civilians



(b) Fatal violence against civilians



(c) Fatal violence against combatants

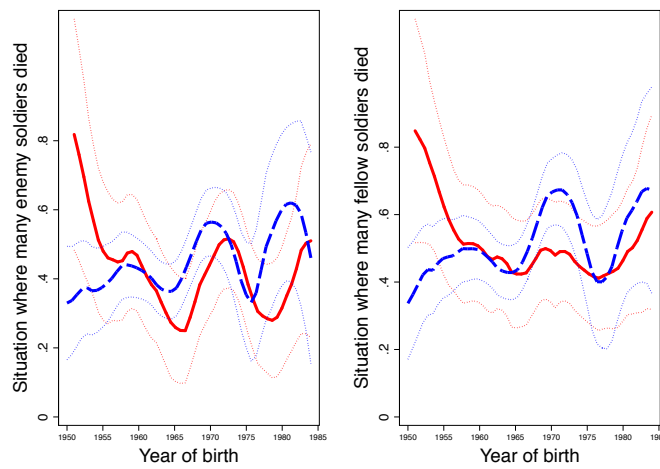
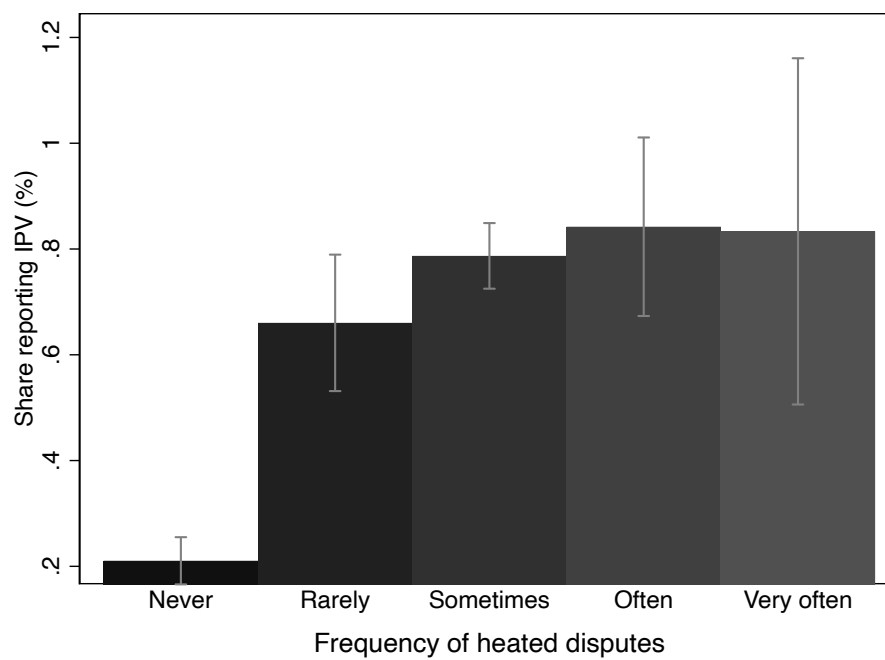


Figure 3: Disputes and intimate partner violence



Tables

Table 1: Domestic violence and exposure to wartime sexual violence

	Domestic violence						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Wartime sexual violence	0.119** (0.054)	0.130*** (0.047)	0.130*** (0.047)	0.113** (0.055)	0.132*** (0.050)	0.126*** (0.044)	0.121** (0.048)
MPLA (main)		-0.009 (0.047)	-0.008 (0.071)	-0.006 (0.049)	-0.009 (0.047)	-0.010 (0.048)	-0.007 (0.048)
Length of service			0.000 (0.005)				
Civilians targetted				0.040 (0.037)			
<i>Fatal violence...</i>							
..fellow soldiers					-0.012 (0.042)		
..enemy soldiers						0.016 (0.039)	
..civilians							0.041 (0.039)
Post-Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	No	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578
R^2	0.09	0.18	0.18	0.19	0.18	0.18	0.19

Notes: ‘Domestic violence’ equals one if woman reports having experienced at least one out of eight acts of intimate partner violence. ‘Wartime sexual violence’ equals one if respondent reports having experienced at least once a situation where a civilian woman was sexually abused, during wartime. ‘Civilians targetted’ equals one if respondent reports having experienced at least once an operation that targetted civilians or their property and was not a massacre, such as looting. ‘Fatal violence...fellow soldiers’ equals one if respondent reports having experienced at least once a situation where many or most fellow soldiers lost their life, during wartime. ‘Fatal violence...enemy soldiers’ equals one if respondent reports having experienced at least once a situation where many or most enemy soldiers lost their life, during wartime. ‘Fatal violence...civilians’ equals one if respondent reports having experienced at least once a situation where many or most civilians lost their life, during wartime. ‘MPLA’ equals one if main army was MPLA (zero means UNITA). ‘Length of service’ is wartime military service in years. ‘Post-controls’ includes these variables: respondent’s age, age squared, years of schooling, and a binary measure indicating whether the woman earns more cash income. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 2: First-stage results

	Exposure to wartime sexual violence						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
MPLA x YOB-Bin 3/7/8					0.218*** (0.057)	0.216*** (0.051)	0.217*** (0.053)
MPLA x YOB-Bin 1	0.089 (0.097)						
MPLA x YOB-Bin 2	-0.164 (0.151)						
MPLA x YOB-Bin 3	0.127*** (0.040)	0.129*** (0.039)	0.093*** (0.034)	0.099** (0.043)			
MPLA x YOB-Bin 4	0.055 (0.078)						
MPLA x YOB-Bin 6	-0.040 (0.083)						
MPLA x YOB-Bin 7	0.427*** (0.143)	0.424*** (0.139)	0.462*** (0.128)	0.473*** (0.124)			
MPLA x YOB-Bin 8	0.213*** (0.073)	0.210*** (0.055)	0.221*** (0.056)	0.189*** (0.058)			
Pre-controls	No	No	No	Yes	No	No	Yes
Post-controls	No	No	Yes	Yes	No	Yes	Yes
Pre-Location	No	No	Yes	Yes	No	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	580	580	580	580	580	580	580
R^2	0.10	0.10	0.14	0.16	0.09	0.13	0.15
F(MPLA x COB = 0)	10.40	11.87	48.04	32.69	14.52	17.57	16.45

Notes: ‘YOB-Bin’ denotes pooled year of birth-cohorts (reference bin: $1965 > \text{YOB} \leq 1970$) ‘MPLA x Bin3/7/8’ denotes the interaction of being MPLA and being born pooled year of birth-cohort 3,7 or 8. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 3: Domestic violence and exposure to wartime sexual violence (IV)

	OLS	IV		OLS	IV	
	(1)	(2) 3+7+8	(3) Bin means	(4)	(5) 3+7+8	(6) Bin means
Wartime sexual violence	0.110** (0.061)	0.334* (0.188)	0.298* (0.157)	0.121*** (0.047)	0.340** (0.180)	0.319* (0.170)
Add. controls	No	No	No	Yes	Yes	Yes
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578
R^2	0.12	0.05	0.05	0.20	0.09	0.10
F-stat(IV)		14.60	24.48		28.80	24.99

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 4: Domineering and masculinity and exposure to wartime sexual violence

	Domineering partner				Role as father			
	Family		Friends		Support		Relationship	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Wartime sexual violence	-0.023 (0.025)	0.056 (0.227)	0.040 (0.042)	-0.047 (0.329)	-0.032 (0.059)	-0.807 (0.602)	0.054 (0.063)	0.167 (0.546)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.19	0.19	0.26	0.26	2.80	2.80	3.14	3.14
Observations	552	552	548	548	565	565	563	563
R^2	0.17	0.16	0.18	0.17	0.16	-0.00	0.23	0.23

Notes: ‘Friends’ equals one if partner reports that veteran seeks to control her relationships to friends; ‘Family’ equals one if partner reports that veteran seeks to control her relationships with family members; ‘Support’ measures how partner rates the degree of support of their children by the veteran (0–4); ‘Relationships’ measures how partner rates the veteran’s relationship with their children (0–4). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 5: Learning from models of gender identity and violence

	Domestic violence							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wartime sexual violence								
... x Length of service	-0.000 (0.032)							
... x MPLA		0.005 (0.039)						
... x Time since left military			0.000 (0.064)					
... x Demobilized				-0.008 (0.036)				
... x DDR					0.056 (0.051)			
... x Factional ties						-0.054 (0.063)		
... x Village share of SV							-0.063** (0.025)	
... x Village share of DV								0.002 (0.024)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578	578
R^2	0.15	0.15	0.15	0.15	0.15	0.15	0.12	0.15

Notes: All regressions include the main terms of both exposure to sexual violence and the interacted variable. The interacted variables are all standardized to zero mean and unit standard deviation. ‘Length of service’ denotes length of wartime military service [in years]; ‘MPLA’ equals one if main army was MPLA (zero means UNITA); ‘Time since left military’ denotes time since respondent left the armed group [in years]; ‘Demobilization’ equals one if respondent was demobilized in a formal process; ‘DDR’ equals one if respondent was part of a formal DDR program; ‘Factional ties’ equals one if respondent still has ties to former members of his units; ‘Village share of SV’ denotes the share of veterans exposed to sexual violence (of sampled veterans in village); ‘Village share of DV’ denotes the share of veterans reported to commit domestic violence (of sampled veterans in village). Regressions with village-level variables include comuna- instead of village-fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 6: Disputes between partners

	Deliberation				Disputes				Regulation			
	Partner's income		Reproduction		Disputes		Starts disputes		Angry / jealous		Concentrate	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV	(9) OLS	(10) IV	(11) OLS	(12) IV
Wartime sex. viol.	0.001 (0.027)	0.108 (0.195)	0.008 (0.034)	0.114 (0.096)	0.175** (0.086)	0.665** (0.323)	0.035 (0.118)	-0.166 (0.444)	0.112** (0.048)	0.540** (0.271)	-0.028 (0.031)	-0.047 (0.322)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Couple controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.85	0.85	0.92	0.92	-0.01	-0.01	0.68	0.68	0.44	0.44	0.15	0.15
Observations	470	470	454	454	484	484	215	215	443	443	488	488
R^2	0.21	0.19	0.31	0.29	0.30	0.26	0.34	0.32	0.21	0.11	0.20	0.20

Notes: ‘Partner’s income’ and ‘Reproduction’ equal one if the couple reports that both partners are usually involved in making decisions in the respective domain (possibly together with other persons); ‘Disputes’ measures the frequency of intra-partner disputes in the last 12 months reported by the partner (0–4); ‘Starts disputes’ equals one if partner reports that the veteran is more likely to start disputes than herself (if any disputes in the last 12 months); ‘Angry / jealous’ equals one if partner reports that the veteran can easily get angry or jealous if she talks to other male adults. ‘Concentrate’ equals one if the veteran reports difficulties with concentration. ‘Couple controls’ includes measures of the age and education gap between partners, income and marriage characteristics. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table 7: Distress

	Psychological distress		Physical symptoms of distress		Alcohol		Smoking	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV	(7) OLS	(8) IV
Wartime sexual violence	-0.237*** (0.063)	-0.471 (0.843)	-0.324*** (0.060)	-0.321 (0.576)	0.037 (0.044)	-0.388 (0.434)	-0.128*** (0.049)	-0.400 (0.338)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Couple controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	-0.10	-0.10	-0.06	-0.06	0.48	0.48	0.29	0.29
Observations	488	488	488	488	488	488	488	488
R^2	0.40	0.39	0.37	0.37	0.25	0.15	0.22	0.17

Notes: ‘Psychological distress’ is an index of psychological distress, evaluated via the Rotterdam Symptom Checklist (standardized). ‘Physical symptoms of distress’ is an index of physical symptoms of distress, evaluated via the Rotterdam Symptom Checklist (standardized). ‘Alcohol’ equals one if respondents reports that he drinks alcohol, zero otherwise (never drinks). ‘Smoking’ equals one if respondent reports that he smokes cigarettes (or cigars), zero otherwise (never smokes). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

For Online Publication: Appendix

A Study context

Both organizations had emerged as national movements opposing the Portuguese colonial rule in a war of independence between 1961 and 1974, alongside the *Frente Nacional para a Libertação de Angola (FNLA)*. By the time independence was consolidated in 1975, inter-movement fighting had erupted over controlling the new nation. FNLA was to fold away soon, while the MPLA, led by upper-class ‘assimilados’ seized control of the capital Luanda and became ‘the government’. UNITA presented itself as the ‘party of *all* Angolans’ and initially seized control of much of the Southern and Eastern territories, and became ‘the rebels’.

Both parties managed to secure strong international allies and access to natural resources. The MPLA relied on assistance from Cuba, the Eastern bloc and oil revenues, while UNITA was backed by South Africa, the US and diamond trade (e.g. [Guidolin and La Ferrara, 2007](#); [Berman et al., 2017](#)). This Cold War ‘proxy-war’ lasted from 1975 until 1991, and was characterized by large-scale front-line fighting, including the biggest conventional battle of post-WWII Africa in Kuito Canavale in 1987. The Cold War phase ended in 1991, when a ceasefire was agreed in the Bicesse accords. After abortive elections in 1992, the MPLA and UNITA returned to war, now without (overt) support by their Cold War allies. Extremely violent episodes and see-saw battles ensued, only interrupted briefly by a failed peace agreement in 1994. In February 2002, the MPLA secured a clear and undisputed victory, when UNITA’s leader Jonas Savimbi was assassinated in an ambush. Military operations ground to a halt abruptly and a Memorandum of Understanding was signed in April 2002, followed by rapid mass demobilization on both sides. Since then, mass violence has been absent from Angola.

Huambo province. We focus the study on Angola’s Central Highlands and its center, Huambo province, for two main reasons.³⁷ First, the Central Highlands were at the center of the war. It was in Huambo City, Angola’s second largest city, that UNITA proclaimed their own government on the same day the MPLA declared independence from Portugal in Luanda, the 11th of November 1975. Most parts of the vast Central Highlands were occupied by the two movements at different times and changed hands multiple times, which makes it possible to compare directly the members, practices and dynamics of the rival organizations. Second, Huambo province is the most densely populated region in Angola, and ethnically homogeneous (Ovimbundu). While the political literature of the Angola Civil War argues that ethnicity was never at the root of the conflict (e.g. [Pearce, 2012](#)), this design also allows to rule out confounding individual factors related to ethnicity.

Post-war politics. We study outcomes in 2014, twelve years after the end of the war. While Angola by then experienced more than a decade of enormous economic growth, this was based almost exclusively on crude oil revenues and human development remained extremely low. State institutions and their reach remained extremely weak by 2014, preventing effective

³⁷Huambo province is roughly of the size of Switzerland; see [Figure A1](#) for a map. Please note that tables and figures with numbers preceded by an A appear in the Appendix.

political and economic development (Soares de Oliveira, 2013). In comparison to countries of comparably low levels of human development, the number of NGO and foreign aid projects and their influence are also extremely small in Angola (Soares de Oliveira, 2011). In practice, this means that in most regions outside the capital, factors that may affect intra-household bargaining and the use of spousal violence have virtually not been affected by policy treatments. Methodologically, this benefits our study as it mitigates concerns that estimates of our link of interest may be conflated by the effect from the impact of post-war policy treatments.

B Data and variables

We describe here the data, including the key variables and their construction used in the paper.

B.1 Data collection (POEMA)

Quantitative fieldwork started in March 2013 and ended in February 2014. It included survey data collection in partnership with the Angolan NGO Development Workshop (DW). While NGO activity is generally very low in Angola, DW has operated in Angola and Huambo Province for more than 25 years and was instrumental in making this survey possible. Importantly, DW have not implemented aid or other development programs related to domestic violence or demobilized soldiers. With DW's support, the authors recruited, screened, trained, and managed a team of enumerators to conduct interviews based on close-ended questionnaires coded on tablet devices.

To ensure as representative a sample as possible, the survey employed three levels of randomization, where the first two involved the primary sampling unit (PSU) and enumeration area (EA) levels. A PSU (*bairro*) is in many rural areas the *comuna* (the lowest tier formal administrative unit), and in urban areas often equivalent to a borough. An EA (*village*) is in rural areas either one settlement or multiple very small settlements, and in urban areas equivalent to a neighborhood. In the absence of systematic and reliable veteran population data, we used up-to date data of the total population from the ongoing Angolan census to randomly draw PSUs and EAs, with weights proportional to the population. The sample was stratified by rural and urban locations, according to aggregate census estimates. At each survey site, we engaged with village chiefs, traditional authorities and local administrations to produce listings of all former soldiers residing in the enumeration area (EA). Results were cross-checked to develop credibly complete listings of the local veteran population. Conditional on the reliability of the general population as a proxy for the ex-combatant population, as used in the first stages, the sampling strategy is self-weighting and ensures that the geographic spread across the province is representative. Assuming that we did obtain complete lists of ex-soldiers, the EA-level sample is representative of the EA-level veteran population.

We received IRB approval for this research, including all survey modules, under IRB Services Protocol USIP-070-10F (6659). The survey design followed standard protocols, was advised by a team of psychologists and – wherever possible – used existing modules that were adapted to the local context. We describe the modules underlying our key outcome and treatment variables below.

B.2 Data

Domestic violence indicator (outcome). Our main outcome variable is a domestic violence indicator built from questions on eight specific acts of domestic violence, using the questionnaire and following the protocols of the recommended Demographic and Health Surveys Domestic Violence Module (DHS, 2016). The eight specific acts include sexual and non-sexual physical violence and are listed in Table A3. As suggested by the literature, our main outcome variable is a binary measure (e.g. La Mattina, 2017). The binary indicator equals one if the veteran’s partner reports that she experienced any form of abuse by the veteran in the last 12 months, based on eight specific forms of violence. In addition, we will also analyze a binary indicator of severe injuries suffered from domestic violence over the past year.

Exposure to wartime sexual violence indicator (treatment). Our key explanatory variable is an indicator of wartime exposure to sexual violence against women, based on Maia et al. (2011) and Brück et al. (2016). In most specifications we use a binary measure, which equals one if the veteran reports having exposed to a situation where a civilian woman was abused (during the war). We also collected data on how often the veteran reports having been exposed. While we prefer the binary indicator as it is more reliable in terms of measurement error, we also present results on frequency of exposure. For ethical and legal reasons, the questions did not probe the degree of active or voluntary participation.

Army and year of birth indicators. *UNITA* is a dummy indicator for a veteran’s main armed group. It equals one if his main army was UNITA; zero means MPLA. We create pooled year of birth indicators, each bin spanning five years, and truncated at 1950 and 1980. This results in eight pooled birth dummy variables, Bin1 means born before 1951, Bin2 between 1951 and 1960, up to Bin 8, meaning born after 1980. The cut-off at 1980 is motivated by the fact that someone born after 1980 is very unlikely to have joined an army before 1994, and on the other hand extremely likely to have served in the final period of the war from 1998 to 2002.³⁸

B.3 Summary statistics

Table A3 reports summary statistics for our main variables of interest and principal control variables. 44 percent of respondents’ partners report having experienced (any form of) domestic violence in the last 12 months. Among veterans who report having been exposed at least once, 85 percent report total numbers of situations of five or less, 2 percent state numbers of twenty or more (not shown). 21 percent report having been exposed to at least one situation where a civilian woman was abused during the war. The average respondent is in his late forties and served in the military for about 11 years during the war.³⁹

An interesting question from a development point of view is whether exposed veterans cluster in specific villages. In extreme cases, we may have villages with shares of zero or one of exposed veterans. A priori, we do not expect ‘extreme villages’, as about three-quarters report that they returned ‘home’ (see Table A3) and – due to the exposure mechanism – the

³⁸All results are not sensitive to these cut-offs.

³⁹The median age is 49 years; the median tenure is 9.58 years.

distribution of exposure among veterans from the same region should be similar to that the full sample. **Figure A11a** confirms that we do not observe extreme villages, and the village-level shares of high-exposure types are relatively narrowly distributed around the overall individual mean of being an exposed veterans (of about one quarter).

Similarly, **Figure A11b** plots the village-level share of perpetrators of domestic violence (of all sampled veterans in the village). As above, we find that the distribution is centered around the individual-level mean of around one half. The tails of the distribution again reveal that there is no village where either all sampled partners in a village report domestic violence (share=1) or none of them (share=0).

C Additional results

C.1 OLS

To account for potential intra-cluster correlation in the error term we report in our main specifications [Cameron, Gelbach, and Miller \(2011\)](#) standard errors two-way clustered at the locations of recruitment (municipality; N=38) and current residence (PSU; N=22). **Table A4** presents p-values of our coefficient of interest based on alternative standard errors. In column 3, standard errors are one-way clustered by the location of current residence, column 4 estimates standard errors are one-way clustered by the location of pre-service residence. Column 5 presents Huber-White robust standard errors. Because the numbers of clusters may be ‘small’ (see [Cameron, Gelbach, and Miller, 2011](#), e.g.) a potential concern is that we incorrectly inflate (or deflate) standard errors as a consequence of clustering. Column 6 presents p-values based on one-way clustered standard errors parametrically corrected by the Moulton-method (see [Angrist and Pischke, 2008](#), e.g.), column 7 reports standard errors estimated by the wild cluster bootstrap method.⁴⁰ The estimated standard errors do not vary noticeably across these methods, and all result in comparable confidence intervals with p-values (well) below .01.

As our main outcome variables are dichotomous, we test whether our main results are robust to non-linear model specifications. **Table A5** reports average marginal effects estimated in a logit model. Across specifications, these are very similar – in significance and magnitude – to the effects found in the linear probability model.

C.2 Mechanisms

Relative economic bargaining power. **Table A10** reports direct effects of exposure to sexual violence on specific outcomes of or related to economic bargaining power. The results provide further evidence against a decisive role of economic bargaining power. We find no significant effects on the veteran’s cash income (in logs), his labor force participation, and various measures of relative cash income (relative to his partner’s).

⁴⁰We use Rademacher weights (+1 with probability 0.5 and -1 with probability 0.5) for re-sampling residuals and impose that the null hypothesis of zero treatment effect as recommended by [Cameron, Gelbach, and Miller \(2008\)](#). While frequently used, it should also be noted that this method is only reliable for large sample sizes.

Marriage formation and characteristics. Next, we investigate the impact of exposure to sexual violence on marriage market outcomes. Differences in marriage market outcomes could reflect mediation mechanisms based on gender norms and/or economic bargaining power. In either case, we expect that exposed would ‘choose’ or be married to relatively ‘weak’ partners, i.e. partners they will be able to dominate more easily, in terms of controlling their behavior or relative bargaining power.⁴¹ Yet, the results displayed in [Table A11](#) and [Table A12](#) find no such systematic differences between exposed and non-exposed veterans in marriage formation and characteristics. In terms of marriage outcomes, exposed veterans are not significantly more likely to be married, have more wives or wives that disagree with the statement “A woman has the right to refuse sexual intercourse with her husband if she does not want to have it” (columns 1 to 3). Rather, wives of exposed veterans are slightly *more* likely to agree with such a statement (column 3), yet are significantly less likely to have known their spouse before marriage (column 4). We see only very small and insignificant differences in terms of other dimensions of marriage formation, including land the woman brought into marriage, her occupation and her age at the time of marriage (column 5 to 7). Exposed veterans are about 4 percentage points more likely to have paid a bride price, which 62 percent of all veterans did according to their spouse (column 8). Ultimately, column 9 tests whether exposed veterans are more likely to marry a previous victim of sexual abuse (by any other person), which would imply that exposed veterans may pick “victims.” Yet the results suggest that this is not the case. The coefficient is small and slightly negative. In sum, we observe that the effects on marriage market outcomes are weak and unlikely to be systematically affected by the exposure to wartime sexual violence, via gender and bargaining power channels.

C.3 Intensive margin

Among veterans who report having been exposed at least once, 85 percent report total numbers of situations of five or less, 2 percent state numbers of twenty or more. The questionnaire design was based on psychological survey instruments, but self-reported recall data on the number of different situations may be an issue, of course, and qualifications are necessary. Veterans may be likely to not remember the exact number or feel uncomfortable revealing ‘a large number’, which would probably lead to underreporting. Overall, the reported absolute numbers may seem ‘small’ or ‘too small’, but they are qualitatively consistent with the sexual violence and Angola literatures cited above, which predict and describe sexual violence by armed groups in the Angolan conflict as rare – rather than frequent – events.

[Table A19](#) reveals that the binary indicator masks important non-linearities in the effects of exposure. For the regression analysis we split the group of exposed soldiers into two sub-groups, those exposed to “1 to 4 situations” and those exposed to “5 or more situations”. The reference group are non-exposed soldiers. The results in column 1 suggest that the positive impact of exposure is driven by the less exposed, while the effect completely disappears for the most exposed. This pattern suggests that above a certain threshold the violent practice effect is dominated by a mechanism that works in the opposite direction, i.e. one that makes domestic violence *less* likely. In column 2 we effectively make mean exposure in the high exposure group

⁴¹As many other armed conflicts, the Angolan war took the lives of many more men than women. In addition, this logic builds on couples that formed after the end of the soldier’s military service. All results are presented for the whole sample, but remain quantitatively unchanged for the sub-sample of unions established after the man’s military service (not shown).

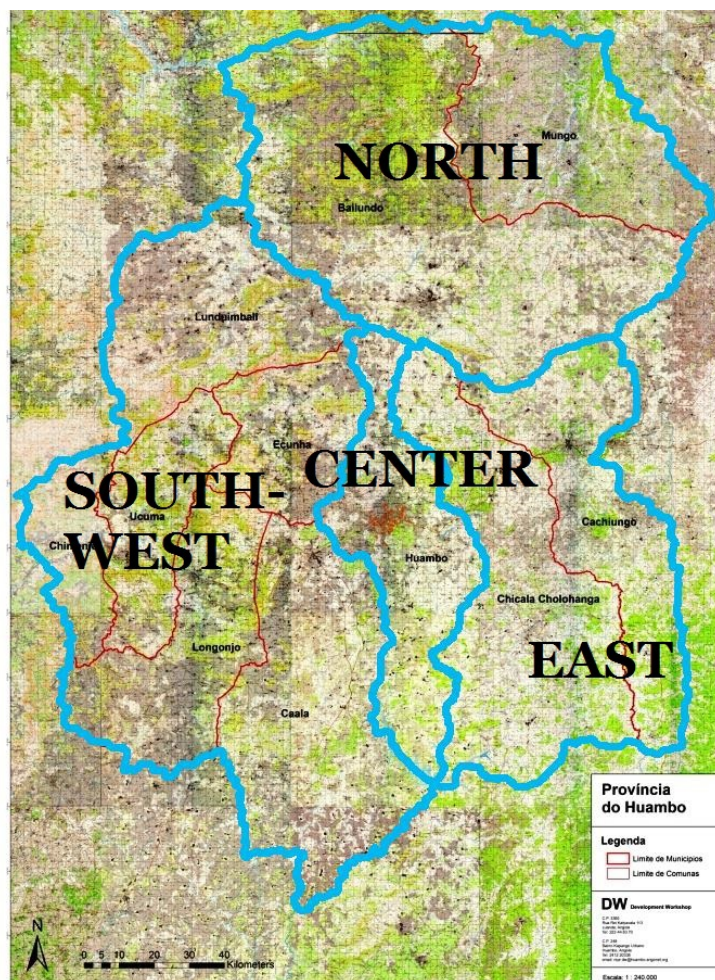
even slightly higher by setting the threshold at six situations.⁴² While only suggestive, based on very small sub-sample analysis and not statistically significant, we observe that in this specification the high-exposure now is actually *less* likely to perpetrate domestic violence than the non-exposed, while the positive coefficient on the less exposed stay more or less constant. Columns 3 and 4 demonstrate that these results hold when we drop extreme values of more than 50 situations.

To test whether the distribution of psychological distress reflects these competing forces, columns 5 to 8 report results on psychological distress. Across specifications, we find that belonging to the high-exposure group is associated with an *increase* of psychological distress and related physical symptoms. These results suggest two things. First, they provide further support for the claim that the exposure to wartime sexual violence and perpetrating post-war domestic violence are linked by psychological processes. Second, the main effect is non-linear. Above a certain threshold, the dominant mechanism based on violent practice against women vanishes and ‘high exposure’ is associated with less domestic abuse (compared to no exposure) and high levels of psychological distress.

⁴²We do not define higher thresholds because of the already very small sample size of the high exposure group.

Figures

Figure A1: Map of Huambo province



Notes: The blue lines define the borders of the four main regions of Huambo province: Center, North, South-West and East Source: Development Workshop Angola.

Figure A2: Distribution of date of entry by region of recruitment

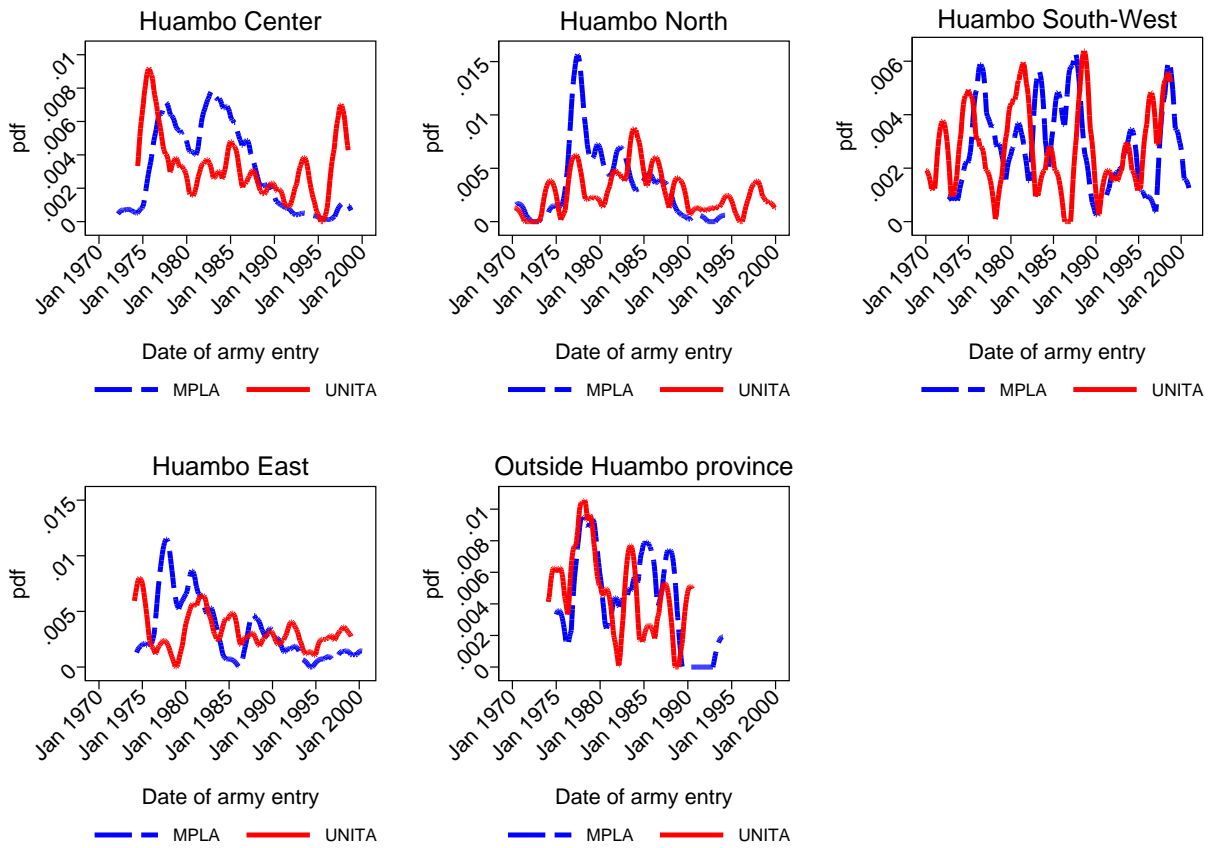


Figure A3: Histogram of age at first entry into military

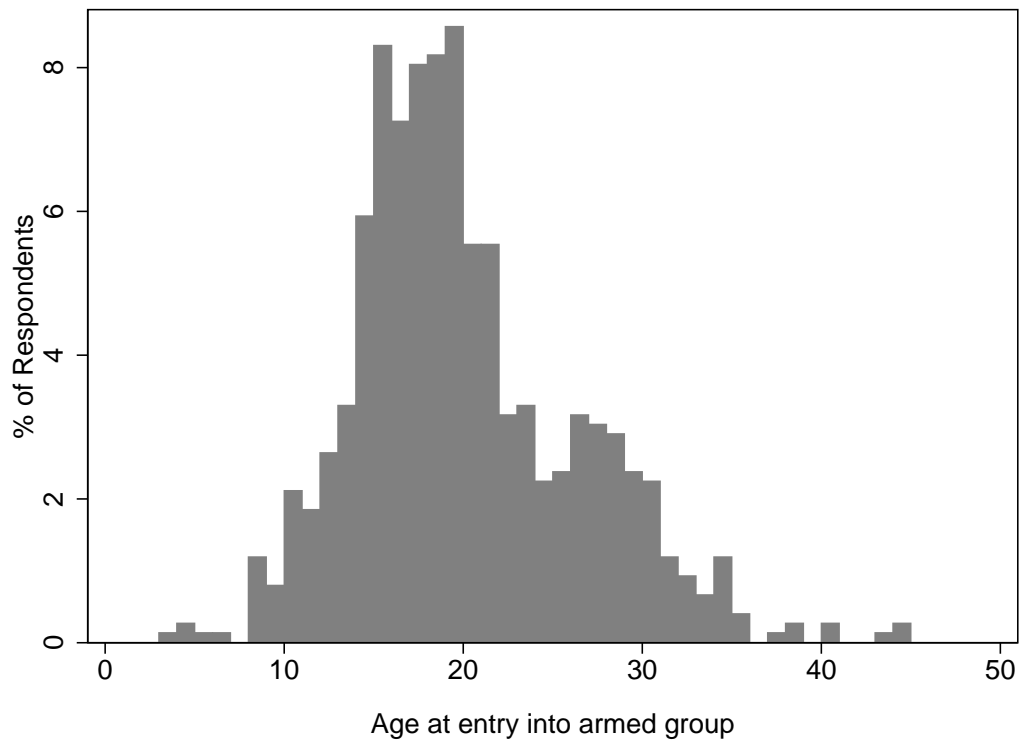


Figure A4: Distribution of year of birth

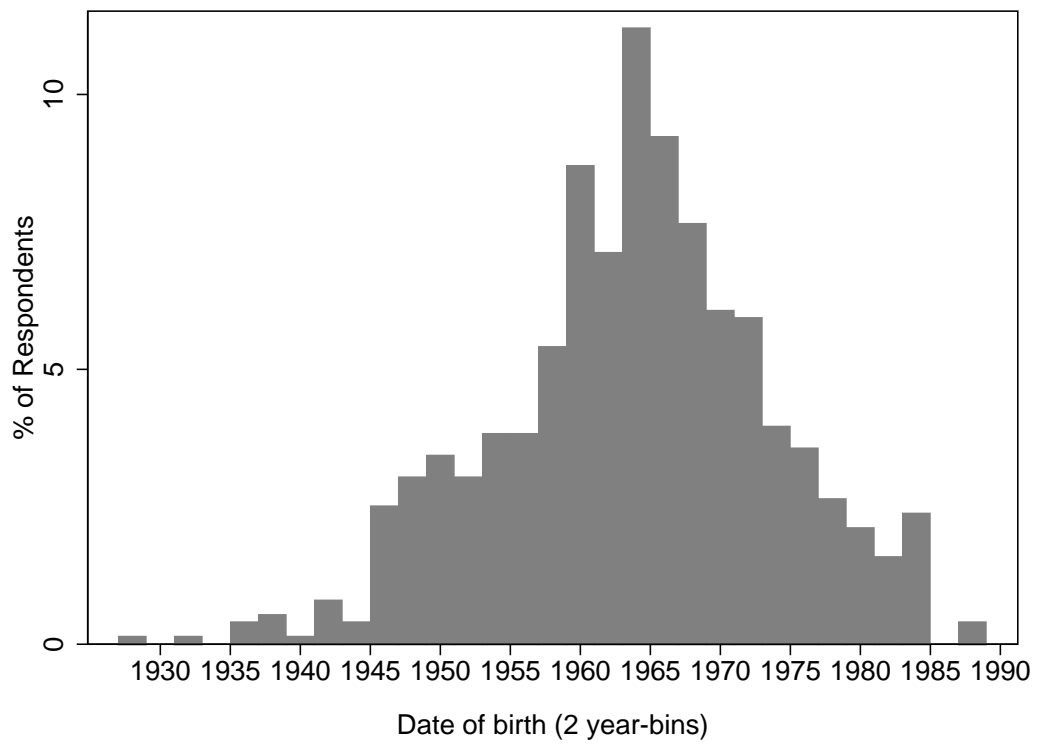


Figure A5: Local polynomial smooth of date of military entry vs date of birth, split by army

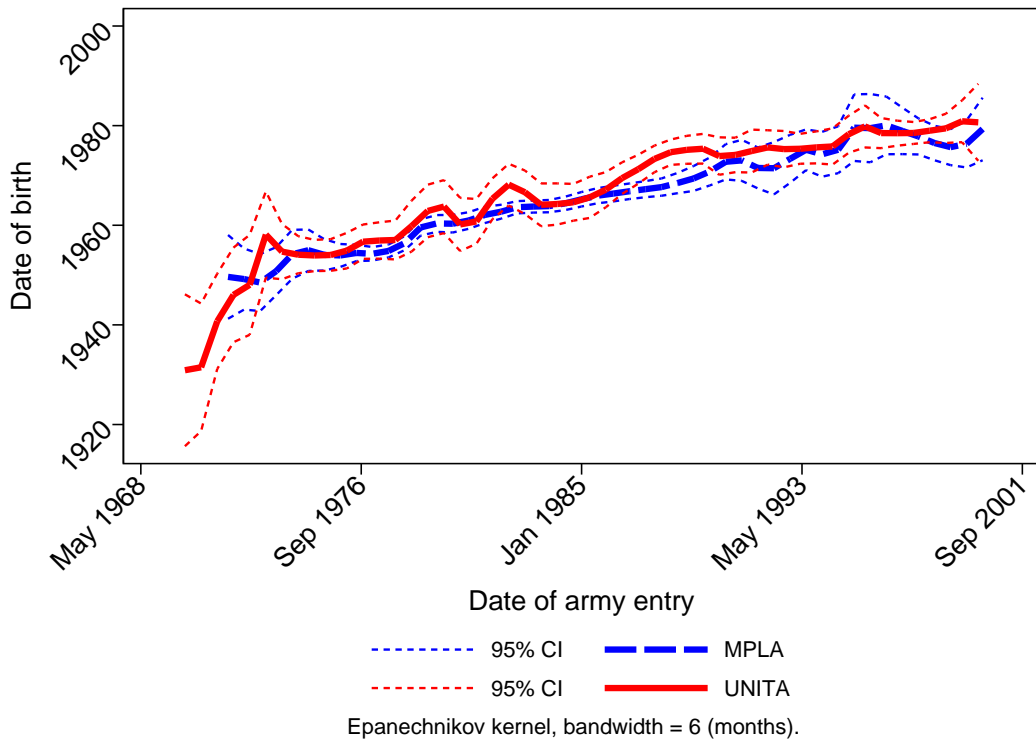


Figure A6: Predicting MPLA x YOB-Bin 3/7/8 (Bayesian Model Averaging)

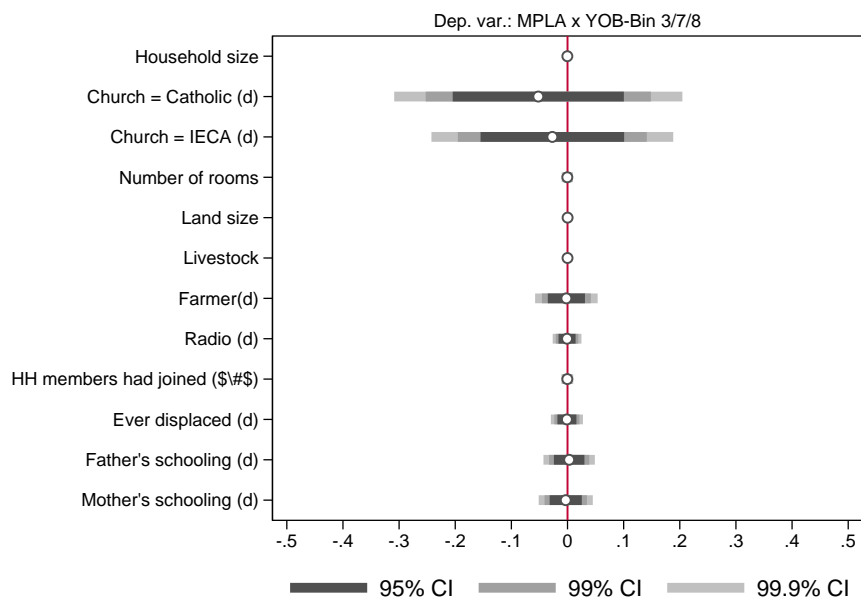
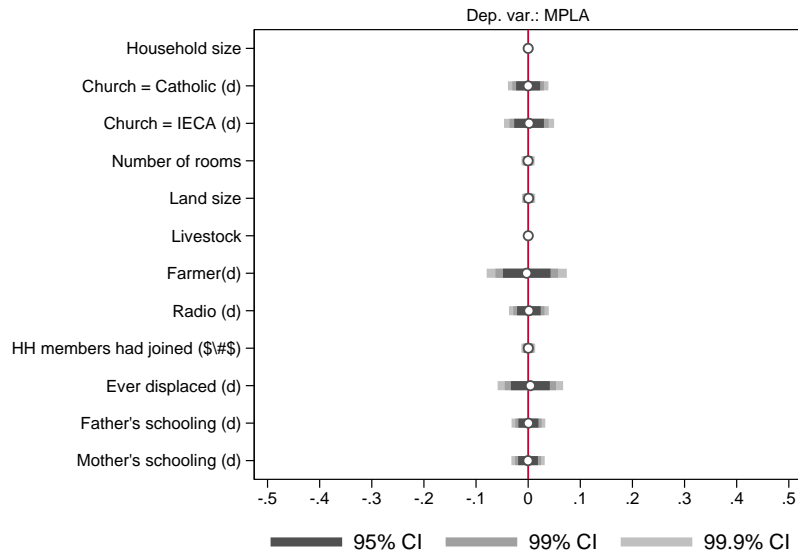


Figure A7: Predicting MPLA by date of birth (Bayesian Model Averaging)

(a) Sub-sample: YOB-Bin 3/7/8 = 0



(b) Sub-sample: YOB-Bin 3/7/8 = 1

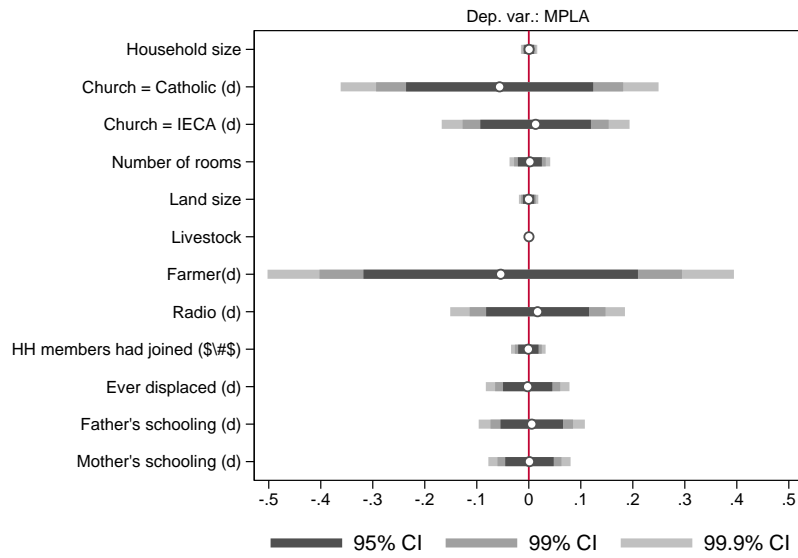
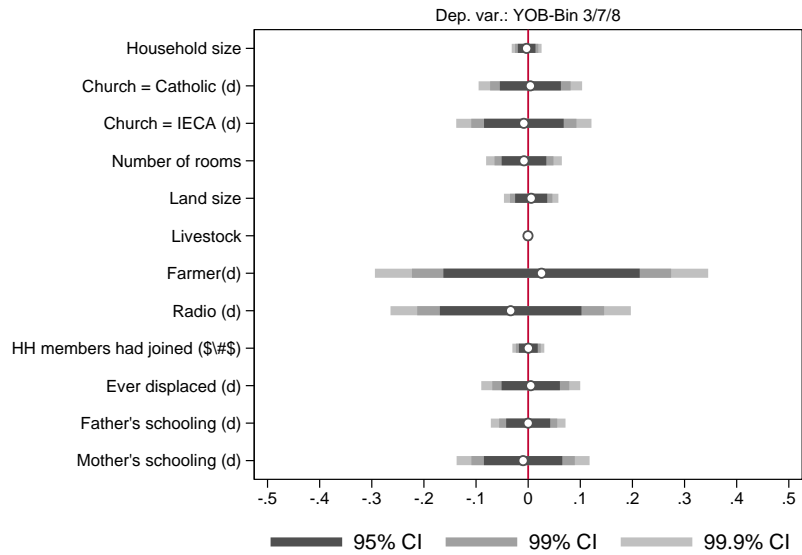


Figure A8: Predicting YOB-Bin 3/7/8 (Bayesian Model Averaging)

(a) Sub-sample: MPLA = 0



(b) Sub-sample: MPLA = 1

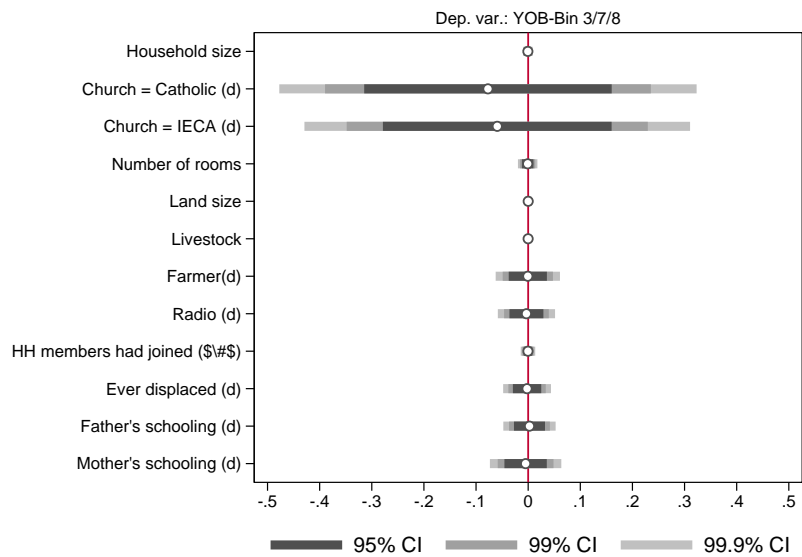
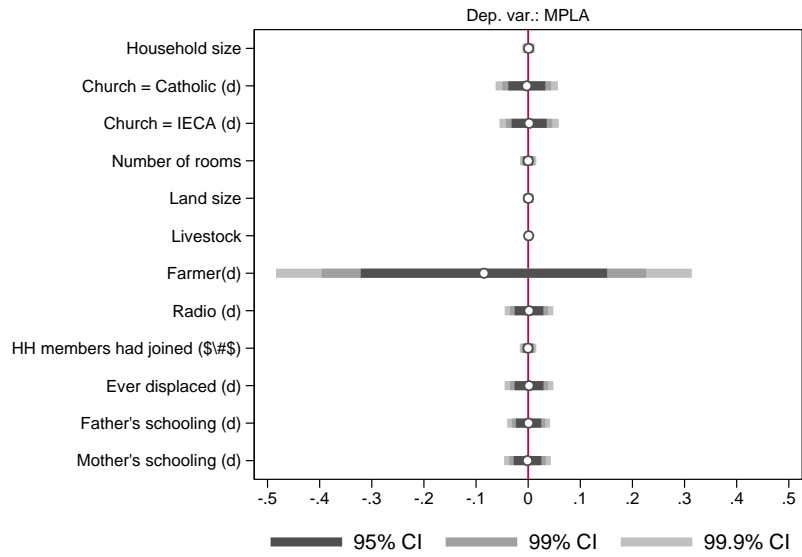


Figure A9: Predicting MPLA by date of entry (Bayesian Model Averaging)

(a) Sub-sample: YOE-Bin III/VII/VIII = 0



(b) Sub-sample: YOE-Bin III/VII/VIII = 1

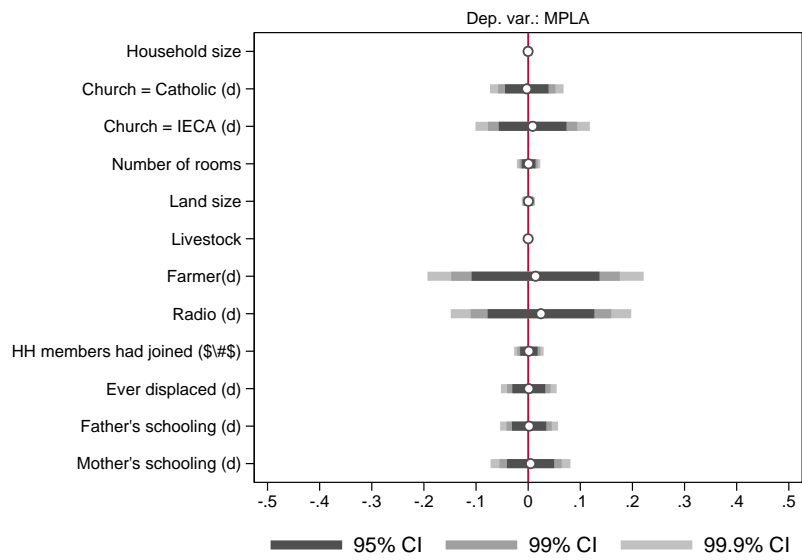
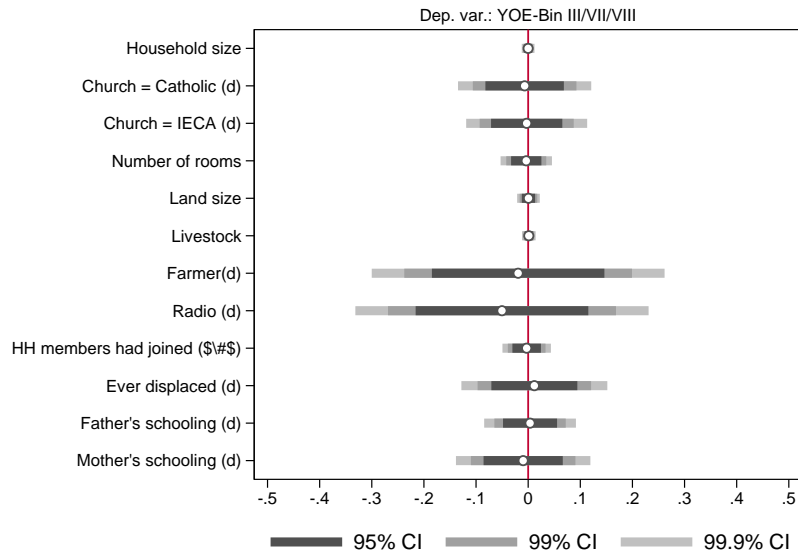


Figure A10: Predicting YOE-Bin III/VII/VIII (Bayesian Model Averaging)

(a) Sub-sample: MPLA = 0



(b) Sub-sample: MPLA = 1

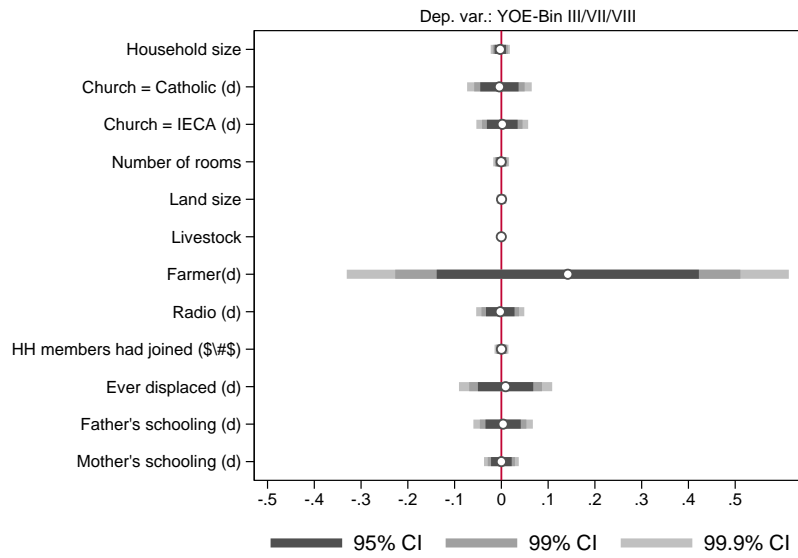
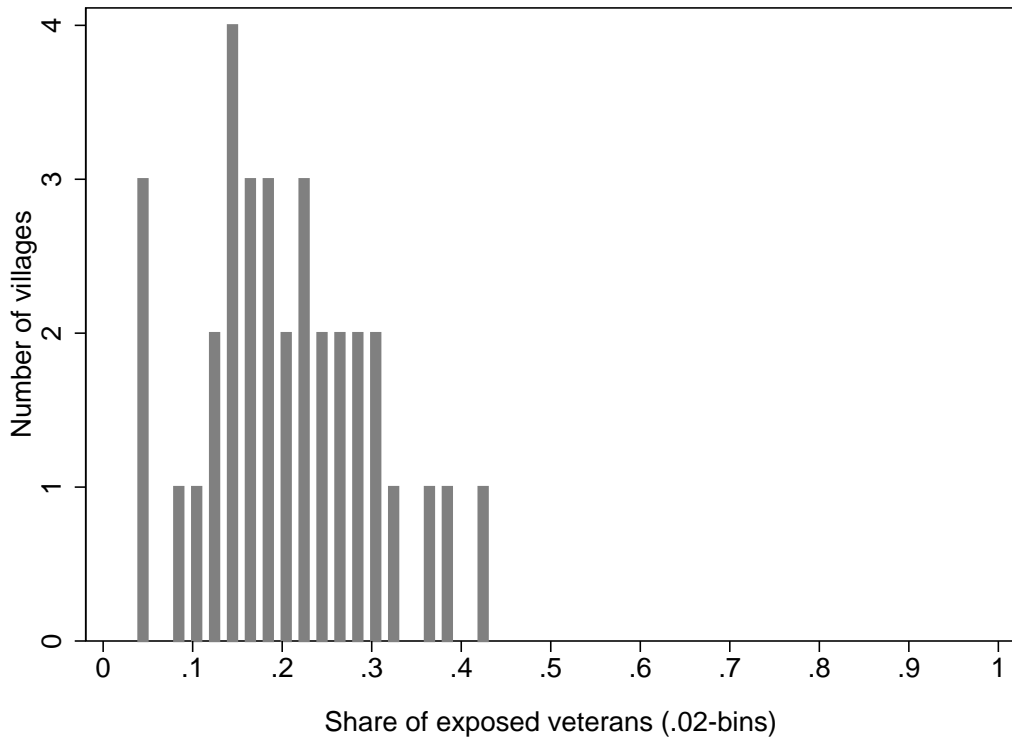
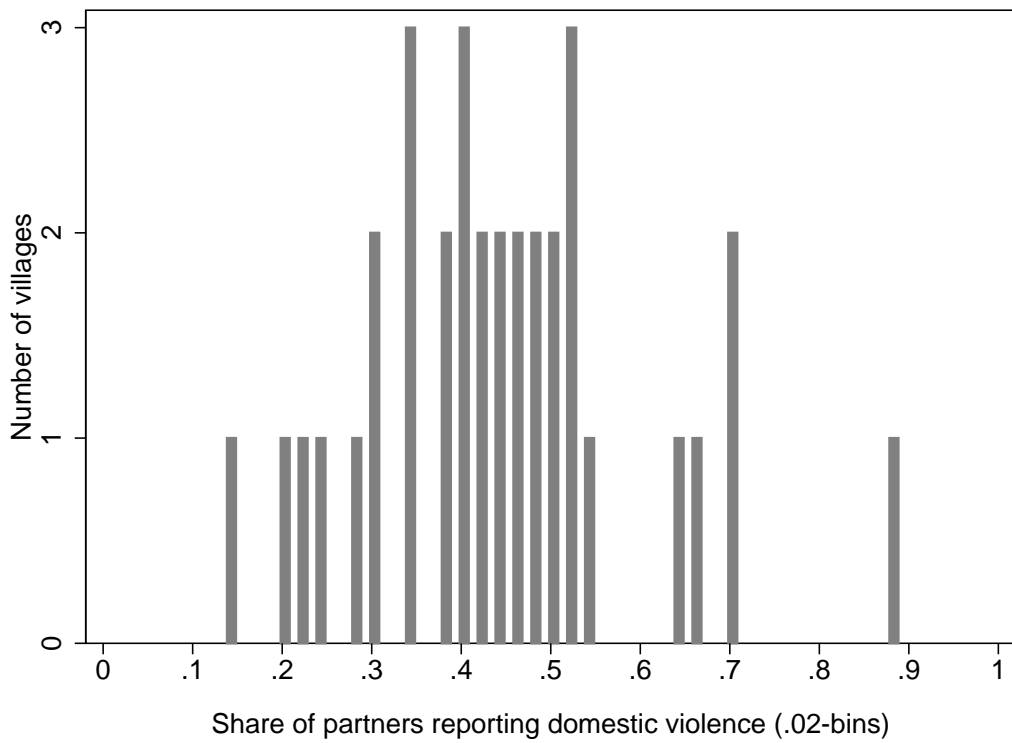


Figure A11: Histograms at the village level (N=34)

(a) Exposure to wartime sexual violence (share within village)



(b) Domestic violence (share within village)



Tables

Table A1: Selection into MPLA (vs UNITA)

	MPLA				
	(1)	(2)	(3)	(4)	(5)
Household size (#)	−0.001 (0.849)	−0.001 (0.801)	−0.002 (0.686)	−0.002 (0.775)	0.001 (0.841)
Church = Catholic (d)	−0.023 (0.700)	−0.031 (0.583)	−0.034 (0.544)	−0.066 (0.239)	−0.052 (0.349)
Church = IECA (d)	−0.032 (0.627)	−0.049 (0.427)	−0.053 (0.388)	−0.069 (0.256)	−0.050 (0.401)
Number of rooms (#)	−0.001 (0.945)	0.015 (0.292)	0.013 (0.346)	0.006 (0.634)	0.019 (0.150)
Land size (#)	−0.001 (0.880)	−0.004 (0.592)	−0.004 (0.594)	−0.005 (0.483)	−0.005 (0.512)
Livestock (#)	−0.002 (0.331)	−0.001 (0.480)	−0.002 (0.235)	−0.002 (0.278)	−0.003 (0.149)
Farmer (d)	0.061 (0.381)	0.082 (0.215)	0.064 (0.325)	0.058 (0.372)	0.082 (0.199)
Radio (d)	−0.028 (0.411)	−0.027 (0.404)	−0.020 (0.528)	−0.014 (0.653)	−0.043 (0.174)
HH members had joined (#)	−0.014 (0.329)	−0.015 (0.269)	−0.013 (0.312)	−0.015 (0.243)	−0.014 (0.235)
Ever displaced (d)	−0.044 (0.219)	−0.031 (0.357)	−0.017 (0.607)	−0.014 (0.666)	−0.024 (0.450)
Father's schooling (d)	−0.023 (0.557)	−0.011 (0.761)	−0.021 (0.571)	−0.002 (0.966)	0.002 (0.961)
Mother's schooling (d)	−0.004 (0.922)	−0.023 (0.536)	−0.043 (0.240)	−0.054 (0.136)	−0.023 (0.210)
Region	No	No	Yes	Yes	Yes
Date (decile)	No	Yes	Yes	Yes	No
Date (year)	No	No	No	No	Yes
Region x Date (decile)	No	No	No	Yes	No
Region x Date (year)	No	No	No	No	Yes
Observations	578	578	578	578	578
R^2	0.01	0.10	0.16	0.22	0.34
Adj R^2	−0.01	0.08	0.13	0.15	0.20
p(background joint)	0.800	0.796	0.647	0.623	0.602

Notes: *Pre-Location FE*: full set of recruitment region fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Classical standard errors.

Table A2: Missing data on domestic violence

	Intimate partner data missing					
	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample	Full sample	Sub-sample: Has partner	Sub-sample: Has partner	Sub-sample: Has co-habitant partner	Sub-sample: Has co-habitant partner
War sex. viol.	-0.033 (0.028)		-0.031 (0.032)		-0.030 (0.033)	
MPLA x YOB-B. 3/7/8		-0.009 (0.069)		-0.015 (0.068)		0.008 (0.070)
Age+age ²	Yes	Yes	Yes	Yes	Yes	Yes
Pre-controls	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	725	725	716	716
<i>R</i> ²	0.09	0.09	0.10	0.10	0.10	0.10

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A3: Summary statistics

	Mean	SD	MIN	MAX
Domestic violence in last 12 months	0.44	0.497	0	1
<i>Eight individual acts</i>				
Pushed you, shook you, or threw something at you	0.35	0.479	0	1
Slapped you	0.43	0.495	0	1
Twisted your arm or pulled your hair	0.15	0.360	0	1
Punched you with his fist or an object	0.24	0.428	0	1
Kicked or dragged you	0.28	0.447	0	1
Tried to choke, strangle or burn you	0.12	0.320	0	1
Threatened or attacked you with a knife or other weapon	0.06	0.233	0	1
Physically forced you to have sexual intercourse	0.12	0.327	0	1
Key explanatory variable				
Situations where a civilian woman was sexually abused	0.21	0.410	0	1
Other key variables				
Age	49.56	9.682	26	86
MPLA (main)	0.70	0.459	0	1
Length of military service (yrs)	11.17	6.763	1	32
Born here	0.73	0.442	0	1
Years of schooling	3.89	2.773	0	12
Radio	0.73	0.444	0	1
Asset index	0.01	1.063	-1	10
Cash income (log)	6.27	3.877	0	13
Woman's cash income (log)	7.48	2.807	0	11
Woman earns more	0.49	0.500	0	1
Observations	578			

Table A4: Alternative standard errors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	CGM Village Pre-mun 2-way	CGM Comuna Pre-mun 2-way	CGM Village 1-way	CGM Pre-mun 1-way	Huber -White Robust	Moulton Village	Wild cluster bootstrap Boot- cluster: village 2-way
War sex. viol. [p]	0.0059	0.0075	0.0043	0.0089	0.0075	0.0127	0.0038
Post-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578

Notes: Covariate specification from Table 1, column 3.

Table A5: Non-linear model specification

	OLS			Logit (AME)			Logit (ME at means)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
War sex. viol.	0.119** (0.050)	0.121** (0.048)	0.109** (0.052)	0.118** (0.049)	0.122** (0.035)	0.110** (0.038)	0.129** (0.055)	0.141** (0.055)	0.127** (0.060)
Civilian targetting	No	No	Yes	No	No	Yes	No	No	Yes
Post-Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578	578	578

Notes: Results from OLS and logit estimation (average marginal effects and marginal effects at means). Village-level fixed effects, Huber-White standard errors in parentheses (logit results to be interpreted with caution).

Table A6: Who gets exposed to wartime sexual violence?

	Sample			Difference	
	Full	Exp. to sexual violence		Diff	<i>p</i>
		Yes	No		
Military					
MPLA (main)	0.70	0.66	0.71	−0.05	0.24
Unit: combat	0.81	0.86	0.79	0.07*	0.06
Unit: survivors	59.81	49.92	62.35	−12.43***	0.00
Role: Infant	0.43	0.44	0.43	0.02	0.71
Low rank	0.47	0.45	0.48	−0.03	0.56
Age at (first) entry	19.57	19.17	19.67	−0.50	0.39
Individual					
Was student	0.23	0.35	0.20	0.15***	0.00
Had any schooling	0.62	0.71	0.60	0.11**	0.02
Had any training	0.06	0.03	0.07	−0.04*	0.07
Self: health	5.12	5.13	5.12	0.00	0.99
Self: strength	6.00	6.50	5.87	0.63***	0.01
Self: mental	6.24	7.07	6.03	1.04***	0.00
Pooled birth cohorts					
Bin1: YOB ≤ 1950	0.12	0.11	0.12	−0.01	0.83
Bin2: 1950 > YOB ≤ 1955	0.09	0.06	0.09	−0.04	0.14
Bin3: 1955 > YOB ≤ 1960	0.16	0.13	0.17	−0.05	0.17
Bin4: 1960 > YOB ≤ 1965	0.23	0.24	0.23	0.02	0.66
Bin5: 1965 > YOB ≤ 1970	0.18	0.19	0.18	0.01	0.72
Bin6: 1970 > YOB ≤ 1975	0.12	0.14	0.11	0.03	0.41
Bin7: 1970 > YOB ≤ 1980	0.06	0.08	0.05	0.02	0.29
Bin8: YOB >1980	0.04	0.06	0.04	0.01	0.45

Notes: ‘MPLA’ equals one if main armed group was MPLA (zero means UNITA). ‘Unit: combat’ equals one if unit(s) mostly operated in combat areas. ‘Role: infant’ equals one if was infant (low role). ‘Rank: private’ equals one if was private (low rank). ‘Age at (first) entry’ is age when joined armed group in years. ‘Was student’ equals one if was student at time of recruitment. ‘Had any schooling’ equals one if had any schooling at time of recruitment. ‘Had any training’ equals one if had any training at time of recruitment. ‘Self: health’ is a subjective assessment of overall health compared to other recruits of about same age at time of recruitment. ‘Self: strength’ is a subjective assessment of physical strength compared to other recruits of about same age at time of recruitment. ‘Self: mental’ is a subjective assessment of mental strength compared to other recruits of about same age at time of recruitment. ‘Year of birth bins’ are pooled birth cohort indicators and equal one if was born in indicated range. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A7: IV check 1: Are individual traits correlated with binary instrument?

	Sample			Difference	
	Full	Excl. var. equals one		Diff	<i>p</i>
		Yes	No		
Military					
Unit: combat	0.81	0.82	0.80	0.01	0.82
Unit: survivors (0-100)	59.81	58.72	59.95	-1.22	0.79
Role: infant	0.43	0.45	0.43	0.02	0.75
Rank: private	0.47	0.50	0.47	0.03	0.62
Individual					
Was student	0.23	0.22	0.23	-0.01	0.88
Had any schooling	0.62	0.67	0.62	0.05	0.36
Had any training	0.06	0.08	0.06	0.02	0.45
Self: health (1-10)	5.12	5.30	5.10	0.20	0.50
Self: strength (1-10)	6.00	5.94	6.32	-0.38	0.40
Self: mental (1-10)	6.24	6.43	6.22	0.22	0.49

Notes: ‘Unit:combat’ equals one if unit(s) mostly operated in combat areas. ‘Role:infant’ equals one if was infant (low role). ‘Rank:private’ equals one if was private (low rank). ‘Age at (first) entry’ is age when joined armed group in years. ‘Was student’ equals one if was student at time of recruitment. ‘Had any schooling’ equals one if had any schooling at time of recruitment. ‘Had any training’ equals one if had any training at time of recruitment. ‘Self: health’ is a subjective assessment of overall health compared to other recruits of about same age at time of recruitment. ‘Self: strength’ is a subjective assessment of physical strength compared to other recruits of about same age at time of recruitment. ‘Self: mental’ is a subjective assessment of mental strength compared to other recruits of about same age at time of recruitment. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A8: IV check 2: Correlation with pre-service variables (multiple regression analysis)

	MPLA x Bin3/7/8		MPLA first army			
	(1)	(2)	(3)	(4)	(5)	(6)
			Bin3/7/8	Bin3/7/8	Bin3/7/8	Bin3/7/8
Household size	-0.004 (0.442)	-0.006 (0.286)	-0.002 (0.882)	-0.002 (0.772)	-0.005 (0.752)	0.002 (0.739)
Church = IECA (d)	0.023 (0.510)	0.011 (0.762)	0.125 (0.155)	0.023 (0.626)	0.019 (0.823)	-0.014 (0.747)
Number of rooms	-0.008 (0.552)	-0.008 (0.553)	0.017 (0.644)	-0.015 (0.419)	0.014 (0.701)	0.006 (0.717)
Land size	0.006 (0.397)	0.008 (0.321)	-0.013 (0.561)	0.010 (0.316)	-0.002 (0.923)	0.007 (0.460)
Livestock	-0.001 (0.755)	-0.001 (0.688)	0.004 (0.514)	0.002 (0.514)	0.000 (0.938)	0.002 (0.353)
Farmer(d)	-0.038 (0.566)	-0.047 (0.501)	-0.255 (0.172)	-0.059 (0.510)	-0.128 (0.493)	-0.014 (0.871)
Radio (d)	-0.015 (0.647)	-0.027 (0.418)	0.090 (0.291)	0.022 (0.627)	0.030 (0.728)	0.005 (0.913)
HH members had joined (#)	-0.004 (0.749)	-0.004 (0.775)	-0.006 (0.856)	0.003 (0.854)	-0.002 (0.936)	0.006 (0.702)
Ever displaced (d)	-0.015 (0.643)	0.011 (0.739)	-0.018 (0.834)	0.048 (0.292)	0.020 (0.810)	0.049 (0.232)
Father's schooling (d)	0.061* (0.087)	0.070* (0.053)	0.011 (0.901)	0.014 (0.780)	0.085 (0.340)	0.023 (0.623)
Mother's schooling (d)	-0.063* (0.079)	-0.062* (0.087)	-0.005 (0.956)	-0.009 (0.857)	-0.001 (0.993)	-0.005 (0.913)
Pre-Location FE	No	Yes	No	No	Yes	Yes
Observations	580	580	159	421	159	421
R^2	0.01	0.07	0.04	0.01	0.19	0.22

Notes: Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Classical standard errors.

Table A9: IV check 3: Correlation with other war experiences

	Fatal violence			Operations against civilians	
	(1) Fellow soldiers	(2) Enemy soldiers	(3) Civilians	(4) Looting	(5) Massacres
MPLA x YOB-Bin 3/7/8	0.019 [0.850]	0.059 [0.554]	0.017 [0.862]	-0.035 [0.724]	0.067 [0.491]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Pre-controls	Yes	Yes	Yes	Yes	Yes
Pre-region	Yes	Yes	Yes	Yes	Yes
Observations	580	580	580	580	580
R^2	0.11	0.06	0.06	0.07	0.12

Notes: 'Wartime sexual violence' equals one if respondent reports having experienced at least once a situation where a civilian woman was sexually abused, during wartime. 'Fellow soldiers died' equals one if respondent reports having experienced at least once a situation where many or most fellow soldiers lost their life, during wartime. 'Enemy soldiers died' equals one if respondent reports having experienced at least once a situation where many or most enemy soldiers lost their life, during wartime. 'Civilians died' equals one if respondent reports having experienced at least once a situation where many or most civilians lost their life, during wartime. 'Looting' equals one if respondent reports having experienced at least once a situation where his group strategically attacked, but not civilians, during wartime (e.g. looting). 'Civilian massacres' equals one if respondent reports having experienced at least once a situation where his group strategically killed civilians, during wartime. 'YOB-Bin' denotes pooled year of birth-cohorts (reference bin: "1965 > YOB ≤ 1970") 'MPLA x Bin3/7/8' denotes the interaction of being MPLA and being born pooled year of birth-cohort 3,7 or 8. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. p-values in brackets.

Table A10: Economic bargaining power

	(1)	(2)	(3)	(4)	(5)
	Income pooled	Partner's share of income	Partner's total income	Partner earns more	Partner has say work veteran
Wartime sexual violence	-0.045 (0.044)	0.023 (0.050)	-0.057 (0.037)	-0.010 (0.075)	0.015 (0.054)
Length of Service	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.35	0.56	0.00	0.48	0.65
Observations	576	548	576	576	570
R^2	0.16	0.15	0.07	0.13	0.15

Notes: 'Income pooled' equals one if income is pooled. 'Partner's share of income' is the share of cash income earned by the partner in last 4 weeks. 'Partner's share of income' is the total share of the cash income earned by the partner in last 4 weeks. 'Partner's total income' is the total cash income earned by the partner in last 4 weeks. 'Partner has say work veteran' equals one if the partner has a say in decisions related to the veteran's economic activity. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A11: Marriage and partnership characteristics

	(1) Married	(2) Man: No. of Wives	(3) Woman: Right to refuse sex	(4) Woman: Violence (ever)	(5) Woman: Violence (No.)
Wartime sexual violence	-0.026 (0.028)	0.014 (0.014)	0.096* (0.056)	0.036 (0.034)	-0.007 (0.062)
Length of Service	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.89	0.15	0.52	0.09	0.23
Observations	579	515	579	563	559
R^2	0.23	0.14	0.20	0.15	0.44

Notes: All measures in this table were reported by the veteran's partner. 'Married' equals one if couple is married, either traditionally or legally, as opposed to other unions like "união de-facto." 'Man: No of wives' is the total number of wives the veteran has. 'Woman: Right to refuse sex' equals one if veteran's partner agrees or strongly agrees with the statement: "A woman has the right to refuse sexual intercourse with her partner if she does not want to have it." 'Woman: Violence (ever)' equals one if the veteran's partner ever used violence against the veteran in the past year. 'Woman: Violence (no.)' is the number of instances that the veteran's partner used violence against the veteran in the past year. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A12: Marriage formation

	(1) Not arranged	(2) Woman: Land	(3) Woman: Occupation	(4) Woman: Age	(5) Woman: Brideprice	(6) Woman: Victim
Wartime sexual violence	-0.058*** (0.023)	-0.001 (0.088)	-0.006 (0.019)	-0.939 (0.653)	0.042* (0.025)	-0.023 (0.032)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.23	0.29	0.90	19.04	0.62	0.06
Observations	517	517	517	472	517	556
R^2	0.23	0.16	0.23	0.18	0.19	0.17

Notes: All measures in this table were reported by the veteran's partner (married couples only). 'Not arranged' equals one if she knew the veteran before they got married. 'Woman: Land' equals one if she brought land into marriage. 'Woman: Occupation' equals one if she was a household worker before getting married, including work for the household and domestic work. 'Woman: Age' is her age when the couple got married. 'Woman: Brideprice' equals one if veteran paid a brideprice. 'Woman: Victim' equals one if she reports having experienced sexual abuse by someone else than the veteran. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A13: Different forms of violence and exposure to wartime sexual violence

	Domestic violence		Political violence			Jobs
	(1)	(2)	(3)	(4)	(5)	(6)
	Physical	Sexual	Violence protesters	Violence police	Violence stay	Violence job
Wartime sexual violence	0.118** (0.053)	0.004 (0.010)	-0.011 (0.061)	0.069** (0.028)	-0.075** (0.031)	0.004 (0.016)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.44	0.13	0.23	0.13	0.16	0.06
Observations	580	580	557	556	546	580
R^2	0.16	0.14	0.12	0.12	0.17	0.16

Notes: ‘Physical’ equals one if partner reports physical domestic violence; ‘Sexual’ equals one if partner reports sexual domestic violence; ‘Injury’ equals one if partner reports at least one serious injury resulting from domestic violence; ‘Fear’ equals one if partner reports that she is ‘often’ or ‘very often’ afraid of the veteran. ‘Violence protesters’ equals one if veteran thinks that protesters are justified to use violence in political demonstrations; ‘Violence police’ equals one if veteran thinks that the police are justified to use violence against protesters; ‘Violence stay’ equals one if veteran would stay at a protest that turns violent; ‘Violence job’ equals one if respondent was engaged in either an illicit activity or work as a security guard in the last four weeks (jobs that are more likely to involve violence than others). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A14: Different components of physical violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Push/shake/ throw	Slap	Twist arm/ pull hair	Punch	Kick	Choke/ strangle/ burn	Attack with weapon
Wartime SV	0.116* (0.064)	0.094* (0.050)	-0.002 (0.024)	0.090** (0.043)	0.095* (0.054)	0.034 (0.035)	-0.005 (0.020)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.36	0.43	0.16	0.25	0.28	0.12	0.06
Observations	570	572	576	572	574	576	575
R^2	0.16	0.17	0.15	0.15	0.16	0.13	0.17

Notes: All measures in this table were reported by the veteran’s partner. the reference periods are the last 12 months. ‘Push/shook/throw’ equals one if he “pushed you, shook you, or threw something at you”. ‘Slap’ equals one if he “slapped you”. ‘Twist arm/pull hair’ equals one if he “twisted your arm or pulled your hair”. ‘Punch with fist’ equals one if he “punched you with his fist or an object”. ‘Kick’ equals one if he “kicked your or dragged you”. ‘Choke/strangle/burn’ equals one if he ‘tried to choke, strangle or burn you’ . Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.

Table A15: Instrument and personal experiences of risk and violence during wartime

	Life at extreme risk (0/1)			Personal violence (0-4)							
	(1) Trapped/ ambushed	(2) Severe illness	(3) Hit by bullet	(4) Verbal abuse	(5) Knife (unarmed)	(6) Knife (armed)	(7) Land- mine	(8) Cap- tured	(9) Tor- tured	(10) Machine gun	(11) Line of fire
MPLA x YOB-3/7/8	0.074 [0.221]	-0.018 [0.764]	0.091 [0.342]	-0.045 [0.876]	0.228 [0.303]	-0.016 [0.934]	-0.086 [0.606]	-0.274 [0.216]	0.234 [0.423]	0.099 [0.686]	-0.276 [0.214]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.82	0.35	0.44	0.75	0.56	0.57	0.66	0.49	0.46	2.68	2.66
Observations	576	576	576	576	576	576	576	576	576	576	576
R ²	0.02	0.03	0.02	0.03	0.02	0.02	0.05	0.02	0.02	0.04	0.05

Notes: 'Trapped/ambushed' equals one if respondent reports having experienced at least once a situation of being trapped or ambushed. 'Serious illness' equals one if respondent reports having experienced at least once a situation of a serious illness following an injury. 'Hit by bullet' equals one if respondent reports having experienced at least situation of being hit by a bullet. 'Verbal abuse' indicates how often a respondent reports having experienced verbal abuse during wartime military service. 'Knife (unarmed)' indicates how often a respondent reports having been threatened with a knife, gun or other weapon (and he was unarmed) during wartime military service. 'Knife (armed)' indicates how often a respondent reports having been threatened with a knife, gun or other weapon (and he was armed) during wartime military service. 'Landmine' indicates how often a respondent reports having directly experienced a landmine or grenade explosion during wartime military service. 'Captured' indicates how often a respondent reports having been captured during wartime military service. 'Tortured' indicates how often a respondent reports having been tortured during wartime military service. 'Machine gun' indicates how often a respondent reports having fired a machine gun or similar weapon during wartime military service. 'Line of fire' indicates how often a respondent reports having been in the direct line of fire during wartime military service. Questions on the items in columns 4-11 were answered on a five point-scale, ranging from 0 (never) to 4 (very often). 'YOB-Bin' denotes pooled year of birth-cohorts (reference bin: 1965 > YOB ≤ 1970) 'MPLA x YOB-3/7/8' denotes the interaction of being MPLA and being born pooled year of birth-cohort 3,7 or 8. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. p-values in brackets.

Table A16: Wartime exposure to sexual violence and post-war well-being

	Objective				Subjective			
	(1) Income	(2) Assets	(3) Education	(4) Employ- ment	(5) Work	(6) Living standard	(7) Health	(8) Life
Wartime sexual violence	688.447 [0.547]	0.160* [0.066]	0.204 [0.366]	-0.112*** [0.001]	0.469** [0.043]	1.523*** [0.000]	0.128 [0.518]	1.313*** [0.000]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	9534.18	0.02	3.85	0.81	2.00	3.89	5.29	4.44
Observations	576	576	576	576	571	564	573	575
R ²	0.03	0.08	0.13	0.06	0.06	0.08	0.03	0.05

Notes: 'Income' is the respondent's personal income. 'Education' is the years of formal schooling the respondent received. 'Assets' is an index measure of household assets, based on 19 items and standardized to zero mean and unit standard deviation. 'Employment' equals one if the respondent has any form of employment, including self-employment. The 'subjective well-being' indicators in columns 5-8 are self-reported ratings of the respondent's satisfaction with several aspects: work situation (col. 5), living standards (col. 6), health (col. 7) and life overall (col. 8). Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. p-values in brackets.

Table A17: Instrument and post-war well-being

	Objective				Subjective			
	(1) Income	(2) Assets	(3) Education	(4) Employment	(5) Work	(6) Living standard	(7) Health	(8) Life
MPLA x YOB-Bin 3/7/8	149.720 [0.946]	-0.116 [0.441]	0.051 [0.923]	-0.075 [0.188]	-0.006 [0.992]	0.182 [0.703]	-0.400 [0.392]	0.113 [0.762]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	9534.18	0.02	3.85	0.81	2.00	3.89	5.29	4.44
Observations	576	576	576	576	571	564	573	575
R^2	0.03	0.08	0.13	0.05	0.05	0.02	0.03	0.02

Notes: 'Income' is the respondent's personal income. 'Education' is the years of formal schooling the respondent received. 'Assets' is an index measure of household assets, based on 19 items and standardized to zero mean and unit standard deviation. 'Employment' equals one if the respondent has any form of employment, including self-employment. The 'subjective well-being' indicators in columns 5-8 are self-reported ratings of the respondent's satisfaction with several aspects: work situation (col. 5), living standards (col. 6), health (col. 7) and life overall (col. 8). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in brackets.

Table A18: Instrument and post-war psychological outcomes

	(1) Psychological distress	(2) Physical symptoms of distress	(3) Drinks alcohol	(4) Smokes cigarettes
MPLA x YOB-Bin 3/7/8	0.091 [0.690]	0.215 [0.246]	-0.040 [0.646]	0.004 [0.953]
MPLA+YOB-Bin	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes
Mean of dep. var.	-0.06	-0.03	0.48	0.30
Observations	576	576	576	576
R^2	0.03	0.04	0.06	0.04

Notes: 'Psychological distress' is an index of psychological distress, evaluated via the Rotterdam Symptom Checklist (standardized). 'Physical symptoms of distress' is an index of physical symptoms of distress, evaluated via the Rotterdam Symptom Checklist (standardized). 'Drinks alcohol' equals one if respondents reports that he drinks alcohol, zero otherwise (never drinks). 'Smokes cigarettes' equals one if respondent reports that he smokes cigarettes (or cigars), zero otherwise (never smokes). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. p-values in brackets.

Table A19: Intensive margin

	Domestic violence				Psychological distress		Physical symptoms of distress	
	(1)	(2)	(3) Drop outliers	(4) Drop outliers	(5)	(6)	(7)	(8)
War sexual violence								
Moderate exposure	0.139** (0.062)		0.139** (0.062)		-0.246*** (0.091)		-0.256*** (0.088)	
High exposure	-0.000 (0.143)		-0.000 (0.143)		0.923*** (0.312)		0.676** (0.294)	
Moderate exposure		0.146** (0.061)		0.146** (0.061)		-0.200** (0.086)		-0.217*** (0.075)
Extreme exposure		-0.235 (0.167)		-0.235 (0.167)		1.055** (0.533)		0.746 (0.496)
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MPLA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	574	574	576	576	576	576
R^2	0.15	0.16	0.15	0.16	0.30	0.30	0.28	0.28

Notes: ‘Situations of wartime sexual violence’ denotes the absolute number of (different) situations where a civilian woman was sexually abused, during wartime. ‘Distress’ is an index of psychological distress, evaluated via the Rotterdam Symptom Checklist (standardized). ‘Symptoms of distress’ is an index of physical symptoms of distress, evaluated via the Rotterdam Symptom Checklist (standardized). Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by village of residence and region of recruitment.