THE WAR AT HOME:
MILITARY SERVICE AND DOMESTIC VIOLENCE

WORK IN PROGRESS - COMMENTS WELCOME

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31 December 2015

ABSTRACT

This paper introduces and studies a new long-term link between wartime military service and post-war behavior. Our key result is that exposure to wartime sexual violence significantly increases an Angolan Civil War veteran's propensity to commit domestic violence more than a decade after the end of the war. We find strong effects on non-sexual physical violence, similar and related effects on disputes between intimate partners, no effects on sexual violence, and a slightly negative effect on domineering behavior. Taken together, we interpret these findings as in favor of an underlying effect based on gender-based violent behavior rather than living up to gender norms. We find no evidence for alternative theories of domestic violence, based on a reduction in economic bargaining power or high levels of distress. This supports a perspective of domestic violence as expressive behavior that is not triggered by payoff-relevant considerations.

1 INTRODUCTION

Violence by men against their intimate partners is one of the most common yet puzzling forms of criminal behavior. Globally, more than 30 percent of women are victims of domestic violence during their lifetime (WHO 2013, Devries 2013)[1], bearing enormous risks for victims and future generations (e.g. Pollak 2004, Sabia et al 2013). The welfare cost of domestic violence is estimated at around 5 per cent of global World GDP, about five times that of wars (Fearon and Hoeffler 2014). Yet, identifying effective ways to reduce domestic violence against women is proving difficult. A key limiting factor is that only very little rigorous evidence exists on when and why domestic violence occurs. Econometric evidence, grounded in microeconomic, sociological and psychological theory, is crucial for developing better policies to protect families, individuals and societies.

This paper contributes to filling this gap by examining the long run effects of military service in war zones on perpetrating domestic violence. Specifically, this paper focuses on gender-based violence and asks: how does the exposure to sexual violence against women during a war affect the propensity to commit domestic violence twelve years after the end of the war[2]?

The focus on the role of military service in war zones is motivated by two empirical regularities. First, partners of veterans are at an elevated risk of being a victim of domestic violence. In the US, for instance, combat veterans are responsible for about one quarter of reported cases of domestic violence (ref). Robust evidence on the underlying factors driving this observation is particularly scarce. Second, perpetrating and witnessing violence seems to makes people more, not less, likely to engage in violence in the future, or that "violence begets violence". From a social psychology perspective this presents an open puzzle, as theory maintains that perpetrating intense violence, such wartime rape, as is often an aversive and distressing experience (Littman and Paluck 2015).

The global objective then is two-fold: 1) Test if a causal relationship exists between the exposure to wartime sexual violence and domestic violence in the long run, 2) Test the explanatory power of four specific, theoretical mechanisms that may substantiate such a link. Based on these mechanisms, we argue that, theoretically, it is a priori unclear whether the exposure to gender violence during the war increases or decreases males' propensity to commit domestic violence. If we make the isolated assumptions that (more) exposure to gender violence 1) weakens the veteran's economic bargaining power, 2)

[1] While the term "domestic violence" generally also includes violence between other individuals within households, we will refer to intimate partner violence, spousal violence and domestic violence interchangeably.

manipulates/exacerbates gender stereotypes, or 3) socializes into gender-based violent practice, or 4) produces distress, the basic predictions are as follows: models based on economic bargaining and aversion to violence arguments (1 and 4) predict a *decrease* in domestic abuse, while approaches based on gender norms and gender-based violent practice (2 and 3) predict an *increase* in domestic abuse.

Testing the relationship between exposure to wartime sexual violence and post-war domestic violence faces empirical challenges. First, both measuring the exposure to sexual violence during a war and assembling such data in practice is difficult, and high-quality micro-data are therefore scant. Second, assessing whether an observed correlation has a causal interpretation is a daunting task, especially in the long run. Unobserved individual level traits may exist that co-vary simultaneously with wartime exposure to sexual violence and violent behavior today and soldiers may misreport either.

In order to address the first challenge, we have collected a unique dataset data documenting the experiences of 759 demobilized soldiers during the Angola civil war, and their socio-economic integration and well-being 12 years after the end of the war. The 27-year war from 1975 to 2002 presents a long, intense and dynamic case of mass militarization, including compulsory military service, and military competition between two high-capacity actors that both also engaged heavily with civilians in violent and non-violent ways (Pearce 2011, Ziemke 2011). It is therefore a natural setting to study the effects of variation in the exposure to war violence. To reduce bias from self-reporting of perpetrated violent acts, we rely on reports on acts of domestic violence by the veteran's spouse, collected in separate and private interviews and based on standard Demographic and Health Survey (DHS) protocols and survey instruments.

Our basic assumption to establish causality is that a soldier's individual exposure to sexual violence will, on average, vary with the extent of sexual violence against civilian women perpetrated by his army during his tenure. Specifically, we exploit three types of variation in sexual violence: 1- by army, 2- over time (within an army) and 3- the difference between armies is time-variant. Informed by the Angola literature, we argue that there were two periods of increased sexual violence against civilians by MPLA. The main hypothesis is then that someone who joined MPLA and, in addition, was born about 19 years before one of the two periods, was significantly more likely to be exposed to sexual violence during his tenure, compared to all others. The identifying statistical assumption is that this source of variation in sexual violence is valid and informative.

Our key result is that exposure to wartime sexual violence significantly increases a soldier's propensity to commit domestic violence more than a decade after the end of the war. We

show that these effects are not driven by local effects, omitted individual variables, measurement error, misspecified intercorrelations of the error terms, and the choice of our preferred measure of gender violence. We find strong effects on non-sexual physical violence, similar and related effects on disputes between intimate partners, no effects on sexual violence, and a slightly negative effect on domineering behavior. Taken together, we interpret these findings as in favor of an underlying effect based on gender-based violent behavior rather than living up to gender norms. One explanation consistent with the results would be a lasting reduction or even breakdown of natural obstacles to gender-based violence. We find no evidence for alternative theories of domestic violence, based on a reduction in economic bargaining power or high levels of distress. This supports a perspective of domestic violence as expressive behavior that is not triggered by payoff-relevant considerations.

Does violence beget violence?

Our paper contributes to the debate in the scholarly and popular literature on the existence and nature of "cycles of violence". An extensive body of research has documented associations between the exposure to violence at a young age and perpetrating violence and violent crime later. Forms of exposure include parental violence as a child (e.g. Caspi 2002) and war violence during military service (e.g. Bouffard 2003), and specifically, the link of either to committing domestic violence later (e.g. King and King 2000). Yet, a recent review article by Widom and Wilson (2015) highlights two important limitations of this literature. First, while studies in the past two decades overcame many of the methodological obstacles, empirical research on cycles of violence remains severely limited in its ability to draw causal conclusions. On the one hand, measures of previous and current violent behavior need to rely on self-reporting, which invites measurement error. On the other hand, evidence from randomized controlled trials is obviously limited to simulating exposure to violence or maltreatment, and thus endogeneity bias from mechanisms such as the genetic inheritance of violent traits is difficult to rule out. Theoretically, Widom and Wilson (2015) emphasize that "future research needs to adopt models that consider the individual in the context of the broader social environment in which he or she functions". The most nuclear environment is certainly the household and family. Our paper introduces an original identification strategy to provide causal evidence for a link between wartime exposure to sexual violence and post-war domestic abuse.

Understanding the household

Based on an ideological shift in favor of the 'collective' approach over the traditional 'unitary' approach to modeling the family (Browning and Chiappori 1998), a burgeoning literature in

economics has developed models of the internal mechanics of the household and marriage. In the collective perspective on household decision-making, each household member or spouse has their own preferences, and resource allocation is determined through a bargaining process. Leading models are based on the collective labor supply interpretations in Chiappori (1988) and Blundell, Chiappori, and Meghir (2005). In these models, an individual's welfare in the event the household dissolves – his/her 'outside option'- determines his ability to influence household decisions, his/her 'bargaining power'[3]. In consequence 'power proxy variables' such as earnings ability, non-labor income or remarriage prospects, give the individual more influence over household decision-making and resource allocation. The strong focus of this literature is concerned with the effects of variation in women's bargaining position on household expenditures and outcomes of children (Bobonis 2009, Duflo 2003, Thomas 1994).

However, whether and when *more female relative bargaining power* leads to more or less domestic violence is an open question, and the theoretical and empirical evidence is hitherto markedly mixed. Theoretically, standard bargaining models, where violence is explicitly modeled, predict a decrease in domestic violence (Aizer 2010, Anderberg et al 2015). Yet, alternative models as those inspired by the sociological theory of the "male backlash", predict an increase in domestic violence. For instance, men may fear their traditional gender role is threatened (Macmillan and Gartner 1999) or empowered women may shift allocations away from their husbands' preference (Castro, Riquer, and Medina 2006; Castro and Casique 2008). Empirically, evidence from developed countries are mostly in favor of a decrease in domestic violence (Stevenson and Wolfers 2006, Aizer 2010, Anderberg et al 2015). In less developed countries, significantly less robust evidence exists and findings are highly ambiguous. In India for instance, Panda and Agarwal (2005) find women who own property are less likely to be victims of spousal violence, while Anderson and Genicot (2014) find he opposite. Chin (2012) shows that female labor force participation of rural women decreases spousal violence, while in the South Luke and Munshi (2011) find the opposite. These inconclusive patterns hold across other developing regions. Bobonis et al. (2013) finds that although women in recipient households in Mexico's PROGRESA program were significantly less likely to be victims of physical abuse than women in control households, they were more likely to be victims of emotional violence and more likely to separate. González-Brenes (2005) does not find a relationship between female income shares and spousal violence in Rwanda, Tanzania and Zambia; Lenze and Klasen (2013) find no effect of female labor force participation on domestic violence in Jordan. We provide rare evidence on the role of

[3] See 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.

variation in male background and characteristics for household and marriage outcomes and find evidence that other (mediating) factors than economic bargaining power may be crucial.

The effects of military service

Our paper also contributes to identifying and quantifying the effects of wartime military service. Following Angrist (1989), a prominent body of econometric research has used the conscription-lotteries of World War II and the War in Vietnam to produce convincing estimates of the causal impact of veteran status on individual outcomes, such as future employment, earnings, disability status, mortality and crime (Angrist 1990, Angrist 1993, Angrist & Krueger 1994, Angrist and Chen 2011, Angrist, Chen and Song 2011, Siminski 2013, Lindo and Stoecker 2012, Rohlfs 2010). An inherent limitation of this literature is that it identifies the effect of being a veteran vs. a non-veteran in a developed country. The exact service characteristics and experiences underlying observed differences and whether these hold for veterans in developing countries and civil wars remain obscure. In addition, the causal effects of military service on the household and family sphere remain very little understood. Notable exceptions are Angrist and Johnson (2000), Conley and Heerwig (2011), and Negruas et al (2014) who find mixed and generally weak effects of deployment abroad on the likelihood of divorce. Our paper introduces an original identification strategy to causally identify the effect of a specific wartime military service experience on a household outcome.

Conceptual understanding of armed conflict

Finally, the paper also contributes to the broader conceptual understanding of political and military conflict. A first generation of the relatively recent economic literature of the effects of armed conflict focuses on coarse yes vs no comparisons of “exposure to war” at the country-, regional the individual levels. A second generation applies similar comparisons based on “exposure to violence”, measured via “battle deaths or events” at the national and regional levels, geographic proximity to these events at the household or individual levels, or via direct “victimization” indicators at the individual level (see Blattman and Miguel 2010, Justino 2012). Yet, as Blattman and Miguel (2010) point out, causal identification of the effects of exposure, theory of the underlying mechanisms and high quality data at the micro-level are rare. We contribute to this literature in several ways. First, we study wartime experiences beyond battle violence. Although most microeconomic studies of civil wars equate war exposure to combat violence exposure, conflict-affected zones also experience high levels of violence against civilians, and particularly women, including rape as a weapon of war (Cohen, Green and Wood 2013). Second, we analyze important societal and institutional effects of civil wars, which are the least understood as noted in the review article by Blattman and Miguel (2010). Third, we contribute to developing a better theoretical understanding of the

micro-level effects of civil war. Existing analyses are largely empirical, as also lamented in Blattman and Miguel (2010). Finally, we introduce a novel and credible empirical strategy to identify a micro-level effect of war. Existing studies have mostly not been able to solve convincingly endogeneity issues stemming from a potential correlation between unobserved individual level traits and post-war behavior unrelated to wartime experiences. We believe this study presents convincing evidence of a causal relationship that links war violence to behavior more than twelve years after fighting stopped.

Policy

Our findings are of not only academic interest. They can help us understand household responses to policy treatments better and avoid unintended and perverse effects, such as in the US, where state laws which require the police to arrest abusers when a domestic violence incident is reported, significantly increased domestic homicides (Iyengar 2009). Our results can also help design policies that target male motives or mediators of committing domestic violence[4]. Finally, this study provides also direct lessons for demobilization, and disarmament and reintegration projects (DDR) and post-conflict policy programming more broadly. Many implemented projects have struggled to deliver large-scale, effective and persistent assistance to former fighters and have often exclusively focused on their individual economic integration (ref). Our findings can help understand why conflict-affected individuals and contexts may respond differently to policies and produce interventions more tailored to needs based on past experiences and at the family level.

Section 2 provides theoretical motivations for a lasting effect of exposure to wartime public good provision on the propensity to participate in public good production. Section 3 describes the Angolan context, and the survey data we collected and use in this paper. Section 4 presents the identification strategy, econometric specifications and assumptions of the empirical analysis. Sections 5 presents the main results. Section 6 discusses the underlying mechanisms. Section 7 concludes and discusses policy implications.

2 THEORETICAL FRAMEWORK: WARTIME GENDER VIOLENCE AND POST-WAR SPOUSAL VIOLENCE

The paper asks whether and how the exposure to gender violence affects the propensity of ex-soldiers to commit violence against their spouse in the long run. The traditional perspective

[4] Luca, Sharma and Owens (2015), for example, demonstrate that state level bans on the commercial sale of alcohol in India reduced both male alcohol consumption and domestic abuse.

of domestic violence is psychological, yet more recently social scientists have emphasized the centrality of the social, economic and policy dimensions in domestic violence (Tauchen and Witte 1995). Most systematic approaches to domestic violence have focused on its nature, and emphasize a distinction between instrumental and non-instrumental use of domestic abuse. Our definition of instrumental includes the use of violence as a coercive means to re-appropriate/-negotiate control over household resources or decision making from an intimate partner, and as an instrument to live up and/or signaling related to societal norms of gender and masculinity. However, recent findings cast doubts on a theory that portrays partner abuse as intentional and rational acts that occur as part of Pareto efficient bargained outcomes or a response to prevailing local or societal norms . Card and Dahl (2011) introduce the view of abuse as a not a pay-off related action to the economic literature and present theoretical and empirical evidence for short-term emotional cues as potential triggers of intimate partner violence.

In the remainder of this section we discuss how past exposure to wartime sexual violence may plausibly precipitate post-war spousal violence. Specifically, we describe two mechanisms based on the two dominant interpretations of domestic violence as partner- and norm-based behaviors, and argue that a non-instrumental interpretation of domestic violence suggests a third and fourth potential mechanism.

Partner-based approaches

Economists and criminologists emphasize rational theories of cooperation and interaction between partners. Economic household bargaining models put preferences on a couple over consumption and violence, and partners bargain over the level of abuse and the allocation of consumption in the household, with man's utility increases in violence and his partner's utility decreasing in violence. From a male's perspective, a key prediction of these models is that the level of partner violence decreases as the man's bargaining power decreases domestic violence. Aizer (2010) presents robust theoretical and empirical evidence that decreases in the gender wage gap cause such an decrease, Anderberg et al (2015) provides equally robust evidence for employment, i.e. male unemployment results in less domestic violence. While contrary to much conventional wisdom, these results resonate with findings from models in criminology such as "women's exposure reduction", where better or more employment of the woman is associated with less domestic violence as it reduces her "time of exposure to the husband" (Dugan et al 1999). In addition, a prominent literature in economics argues that military service in war zones may have causal long-term effects on labor market outcomes as earnings and employment (Angrist 1990, Siminski 2013), hence manipulating economic bargaining power. *If the exposure to sexual violence has lasting negative effects on the*

veteran's economic bargaining power, this approach predicts a negative effect on, i.e. less, domestic violence today.

Gender norm-based approaches

By contrast, in socio-cultural models of control and authority "weaker" male status may *increase* the level of domestic violence, used as a means of signaling or living up to social norms. In economics, Bloch and Rao (2002), show that males may use (domestic) violence to signal their dissatisfaction with their wife to the wife's family and extract more material transfers as prescribed by cultural norms. In military sociology and, more recently, "identity economics" it is now well established that armed groups create norms, loyalties and identification (Costa and Kahn 2003, Akerlof and Kranton 2000), which specifically includes models of gender and masculinity. Gender-based approaches to violent conflict document, for instance, that warmongers interpret and manipulate existing gender stereotypes to motivate soldiers (e.g. GIZ 2009). *If the exposure to sexual violence indeed reflects the lasting creation or manipulation of gender stereotypes, this approach predicts a positive effect on, i.e. more, domestic violence today.*

Violence-based approaches

We argue that an interpretation of domestic violence as behavior that is not triggered by payoff-relevant considerations suggests a third and fourth potential mechanism. Such a view of domestic violence includes short-term causes as emotional cues, as in the pioneering work by Card and Dahl (2011). Yet, we argue that also long-term effects of this broad nature may underpin patterns of domestic violence, based on previous exposure to violence, and potentially create cycles or traps of violence. Put in terms of social learning theory, individuals may acquire behaviors through modeling and reinforcement contingencies in the context of social interactions (Bandura 1973; economics: Banerjee 1992 and Bikhchandani et al. 1992). Models of intergenerational transmission of violence through this lens emphasize that witnessing acts of violence at a young, e.g. between parents, may result in violent behavior as an adult. Similarly, psychologists and economists have argued that perpetrating violence may also lead to more violence later by permanently manipulating and breaking down natural obstacles to violence (Grossman and Siddle 1999, Rohfls 2010). This specifically includes results of military strategies of making violence normative within the collective (Littman and Paluck 2015) and/or processes of "socialization into practice" as in Wood (2015). The general and dominant sociological view of these phenomena includes normative and non-normative processes. Akers (2011), 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). Specifically, the exposure to sexual violence against women may permanently break down natural barriers to violence against women (psychological), make soldiers align their behavior in such a way (non-normative) or truly internalize cultural norms of using violence against women (normative). We will refer to this mechanism by the umbrella term "socialization into gender-based violent practice". Irrespective of the nature of the atomic process, this mechanism suggests: *If the exposure to gender violence socializes soldiers into gender-based violent practice, this approach predicts a positive effect on, i.e. more, domestic violence today.*

Yet, as Littman and Paluck (2015) also point out, perpetrating violence is for most people an aversive and distressing experience in the first place. World War II soldiers reported not firing or deliberately misfiring even in the line of fire (e.g. Grossman and Siddle 2000), and recent econometric evidence on US veterans suggests that exposure to combat, rather than total time of deployment, causes psychological distress later (Cesur et al 2013). The nature of violence suggests that the exposure to gender violence against civilians may be particularly distressing, which may result in regret or guilt (**ref**). *If the exposure to gender violence is perceived as distressing, this approach predicts a negative effect on, i.e. less, domestic violence today.*

3 CONTEXT

History

Between 1975 and 2002, two highly organized and capable military factions fiercely battled for territorial control and their respective nation-building missions in the Angolan Civil War. These were the Movimento Popular para a Libertação de Angola (MPLA) and the União para a Independência Total de Angola (UNITA). Both organizations emerged as national movements opposing Portuguese rule in the war of independence from 1961 to 1974, next to the Frente Nacional para a Libertação de Angola (FNLA). By the time independence finally arrived in 1975, the three movements had started fighting among themselves for control of the new nation. FNLA was to fold soon, while MPLA, run by upper-class "assimilados" seized control of the capital Luanda and became "the government". UNITA presented itself as a "party of *all* Angolans" and seized control of much of the Southern and Eastern territory, and became "the rebels".

Both parties managed to secure international allies and access to natural resources. MPLA, relying on assistance from Cuba and the Eastern bloc and oil revenues, fought UNITA, backed by South Africa and the US and diamond funding, in a cold-war proxy-war from 1975

to 1991. The cold-war proxy phase was characterized by large-scale frontline fighting, including the biggest conventional battle of the Africa since the end of World War 2, in Kuito Canavale in 1987. The cold-war phase ended in 1991, when ceasefire was agreed in the Bicesse accords. After abortive elections in 1992, the MPLA and UNITA returned to war, now without (overt) support by international allies. Extremely violent episodes and see-saw battles ensued, only interrupted by a failed peace agreement in 1994. In 2002, MPLA secured a clear and undisputed victory when UNITA's leader Jonas Savimbi was assassinated in an ambush. This was followed by rapid mass demobilization on both sides. Angola has since recorded more than decade of political stability and absence of violence.

Relevance

Angola's recent history offers an ideal setting for our study. From 1975 to 2002 it experienced a very long, complex and intense war, where territorial control was highly volatile. As Pearce (2012) notes, the end of the war 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 at all. At certain points during the war, the government held only about 20% of its nominal territory. Yet, it was fought by the same two factions throughout, both with sufficient capacity to enforce compulsory military service for young men (Stojetz 2015). This combination suggests the population of former soldiers is a) large and b) likely to contain substantial variation in individual military experiences.

During the war, both sides engaged heavily and often systematically with local populations under their control in a variety of constructive and destructive ways, including sexual abuse of civilian women. These relationships between civilians and combatants ranged from peaceful interactions such as the provision of public services by the armed group, protection from enemy violence or taxation, to extensive violence against civilians, including large-scale massacres (Ziemke 2008, Pearce 2012).

We focus the study on the Central Highlands and Huambo province[5] for three main reasons. First, it was at the center of the war. It was in Huambo City, Angola's second largest city, that UNITA proclaimed their own government on the day MPLA declared independence of Angola on 11 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, it was *not* at the center of the preceding colonial war from 1961 to 1974. Most of the anti-colonial activity and guerilla fighting took place near the coast and international borders, i.e.

[5] This province occupies a territory slightly larger than Belgium.

either movement started their activities and mobilization strategies only when the civil war started (Pearce 2012). Third, Huambo province is the most densely populated region in Angola, yet ethnically homogenous, and has a clear urban center surrounded by largely rural and remote areas.

We study outcomes twelve years after the end of the war in 2002. While Angola has now experienced more than decade of enormous economic growth, this was based almost exclusively on crude oil revenues. State institutions and their reach remain extremely weak, preventing effective political and economic development (Soares de Oliveira 2013). In comparison to countries of comparable development status, the number of NGO and foreign aid projects and their influence are also extremely small in Angola (Soares de Oliveira 2012). 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. This is important for 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.

Research design and data

The population of this study are all living males who were ever part of an armed group during the war and reside in Huambo. The data is based on a sample of 759 Angolan war veterans and comes from survey data, collected by the authors in the Study of Angolan Ex-Combatants (POEMA)[6]. The quantitative component of POEMA was supported by an anthropological companion study, based on twelve months of fieldwork preceding the survey. Ethnographic fieldwork started in January 2012 and was carried out by researchers from Sussex University (Spall 2014). Qualitative accounts 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.

Quantitative fieldwork started in March 2013 and ended in February 2014. It included survey data collection in partnership with the local 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. With DW's support, the authors recruited, screened, trained, and managed a team of enumerators to conduct interviews based on electronic close-ended questionnaires.

[6] The main analysis of this article focuses on the sub-sample of 578 veterans who have a partner. Imputation analysis to analyze the full sample and possible selection into 'not having a partner' is on-going.

To ensure as representative a sample as possible, the survey employed three levels of randomization. Thirty-three survey clusters were drawn in a two-stage process involving the *comuna* and *bairro* levels. In the absence of systematic and reliable veteran population data, up-to date data of the total population were used from the ongoing Angolan census. At each survey site, we engaged with village chiefs, village coordinators and local administrations to produce listings of all former soldiers residing in the enumeration area (EA). Results were cross- and double-checked to develop credibly complete listings of the local veteran population. The sample was stratified by rural/urban according to aggregate census estimates and MPLA/UNITA to ensure comparability of the two armies. Conditional on the reliability of the general population as a proxy for the ex-combatant population, used in the first stages, the sampling strategy is self-weighting and ensures that the geographic spread of clusters across the province is representative. Assuming complete lists the EA-level sample is representative of the EA-level veteran population.

Sampled veterans had to complete two interviews. First, a private household-level interview, together with their (main) cohabitant partner. Second, a private individual interview which included extensive modules on pre-military service, military service and war experiences. Sampled veterans' (main) cohabitant partners were interviewed simultaneously, with a focus on intra-household and family outcomes.

Domestic violence indicator (outcome).

Our main outcome variable is a binary measure, which 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. To elicit these data, the recommended Demographic and Health Surveys questionnaire and protocols were used.

Exposure to wartime sexual violence (treatment).

Our key explanatory variable is a binary measure, which equals one if the veteran reports having exposed to a situation where a civilian woman was abused, during the war.

UNITA

UNITA is a dummy indicator for a veteran's main armed group. It equals one if his main army was UNITA, and zero means MPLA.

Pooled year of birth cohorts

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[7].

Table 1 reports summary statistics for our main variables of interest and principal control variables. The statistics reveal that 44 percent of all partners report having experienced (any form of) domestic violence in the last 12 months. 21 percent of all veterans report having been exposed to at least one situation where a civilian woman was abused, during the war. The mean respondent is in his late forties, and served in the military for about 11 years during the war[8]. An overview of the distribution across pooled year of birth cohorts is presented in the first column of the bottom panel of **Table 3**.

4 IDENTIFICATION STRATEGY

The principal question of the paper is whether soldiers' exposure to wartime sexual violence against women predicts their propensity to commit violence against their intimate partners after the war. The main difficulty in isolating a causal link is that we have to assume that variation in exposure to sexual violence is not independently distributed from factors that also co-vary with perpetrating violence against intimate partners.

Our basic assumption to establish causality is that a soldier's individual exposure to sexual violence will, on average, vary with the extent of sexual violence against civilian women perpetrated by his army during his tenure. Specifically, we exploit three types of variation in sexual violence: 1- by army, 2- over time (within an army) and 3- the difference between armies is time-variant. Informed by the Angola literature, we argue that there were two periods of increased sexual violence against civilians by MPLA. The main hypothesis is then that someone who joined MPLA and, in addition, was born about 19 years before one of the two periods, was significantly more likely to be exposed to sexual violence during his tenure, compared to all others. The identifying statistical assumption is that this source of variation in sexual violence is valid and informative.

4.1 Theory

Source of variation 1: Army

The determinants of variation in exposure to sexual violence at the soldier level are largely unexplored and rigorous evidence absent. Systematic patterns in individual data will, at least partially, likely be related to variation in practice of sexual violence at the armed group level,

[7] All results are not sensitive to these cut-offs.
[8] The median age is 49 years; the median tenure is 9.58 years (not reported)

which a nascent literature has started to analyze conceptually and empirically (Cohen 2013). Which of the two rival movements, MPLA or UNITA, an individual joined is therefore potentially a source of variation in exposure to sexual violence. The Angola literature on war politics in the Central Highlands, the center of the war, argues against strong "selection" into either side based on conventional factors associated with rebel recruitment or mobilization into armed groups. Based on careful historical and ethnographic techniques Pearce (2009, 2012) finds that control of local territory shifted frequently and concludes that "a person's first contact with any political formation was as likely to have been with UNITA as with the MPLA" (Pearce 2012). With regard to the relationship between civilians and combatants, he argues 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." (Pearce 2009). Stojetz (2015) studies recruitment processes in detail and provides econometric evidence that the single, main predictor of army joined is the interaction between place of origin and date of birth. Put simply, the patterns suggest that an individual joined the army in control of his community in his late teenage years. Table 2 presents results from a simple regression of the army indicator on the full set of family background characteristics, inspired by models of rebel recruitment (e.g. Blattman and Annan 2010). The results confirm that no family background characteristic is a robust predictor of which army a soldier joined, with and without pre-service location fixed effects, and classical standard errors. As more than 95 percent of all sampled veterans served in only one army, we conclude that the army indicator may be a plausibly exogenous source variation in experience of sexual violence. Yet, we acknowledge that concerns may remain that a soldier's army was not entirely determined randomly.

Source of variation 2: date of birth

Why and when a specific armed group perpetrates (more) sexual violence are open questions, but emerging evidence suggests that state forces often use sexual violence as a tool of torture of not only detainees but also civilians (Cohen, Green and Wood 2013). Given the high level of state institutional capacity of both actors in the Angolan war, we expect to see more sexual violence at the armed group level during times of (more) operations against civilians.

The recent Angola literature describes three episodes of reportedly increased operations against civilians by MPLA, whereas not much evidence exists on variation in violence against civilians by UNITA. The first episode of increased MPLA violence against civilians spans a couple of years in the late seventies and early eighties, where MPLA faced internal tensions and certain factions mounted challenges to MPLA's leadership (Pawson 2014). Starting in

1977, these developments involved street demonstrations, the breaking open of a prison, the brief hijacking of the state radio station, and the killing of several prominent party leaders. A campaign of repression and operations against civilians ensued. While often cited as a key turning point in Angola's political history, almost no detail of these events is uncontested and still a taboo for many Angolans (Pearce 2015b). The second episode covers a brief period after UNITA rejected the results of the presidential elections in 1992. Hostilities erupted in Luanda and rapidly spread to other parts of the country. MPLA forces reportedly massacred several thousand to tens of thousands of individuals suspected to be a member or supporter of UNITA, including the infamous the Halloween Massacre (e.g. Pereira 1994). This episode seems distinct due to its brutality and extent of fatal violence. Finally, MPLA sharply increased violence against civilians during the last two years of the war, between 2000 and 2002. While this period is generally often referred to as 'confusao', meaning a chaotic situation, an overwhelming number of reports by human rights organization and observers noticed and lamented this change in behavior by MPLA (e.g. Fonseca et al 2001).

What does this mean for army members? If levels of sexual violence at the armed group level are indeed higher in periods when it launches more operations against civilians we expect, on average, more exposure to sexual violence among MPLA soldiers who were more likely to serve in the periods outlined above (compared to MPLA serving in other periods).

Based on this logic, variation in date of military entry should, on average, be associated with variation in exposure to sexual violence. Yet, the date of military entry may not be orthogonal to unobservables that also influence domestic violence today. To mitigate these concerns and emphasize the underlying experimental nature of this variation, we instead consider variation in date of birth of soldiers in the sample, which is determined exogenously. In state controlled regions, military service was compulsory by law for men in late teenage years during the Angolan war. The Angola literature points out that also the rebel group was a high-capacity organization which resembled a "state in the state" (Pearce 2012). If the rebel group also recruited "like a state", the age pattern should be similar to that of state conscription, which is argued to be true in Stojetz (2015). In this case, date of entry into the armed group and date of birth should be highly correlated, making date of birth a robust predictor of exposure to sexual violence. **Figure 1** reveals that there is indeed a very close relationship between date of birth and date of military entry in both armies.

Identifying variation: the interaction of army and date of birth

The arguments above suggest that army control and date of birth could be two sources of variation in exposure to sexual violence. Yet, while plausibly determined exogenously, either could potentially have a *direct* effect on post-war outcomes. For instance, belonging to a

certain army may produce a social marker in the post-war period that affects the use of domestic violence, which may similarly also vary with age. In this situation, army and year of birth would be "good" control, but "bad" instrumental variables.

Based on the logic of violent episodes by MPLA above, we therefore leverage variation based on the *interaction* of the army and year of birth, i.e. we allow the differential between armies to vary over time, specifically across pooled year-of-birth cohorts. In econometric terms, we exclude the interaction of army and date of birth-bins in the "second-stage" of the IV-estimation, while including the main effects.

4.2 Illustration

Violence against civilians (ACLED data)

To explore temporal variation in violence against civilians by the two Angolan factions we first use conflict event data collected by Jennifer Ziemke (2008). Ziemke's meticulous work generated a high quality conflict event database for the Angolan which includes data on major territorial gains, coded by date and actor. One limitation is that the data are almost exclusively based on Portuguese and Portuguese-speaking sources, which favor MPLA.

Figure 1 plots these data for both factions. We observe the three expected types of variation: (i) across armies, (ii) over time (within armies) and (iii) how the differential between armies varies over time. The data confirm the historiographic evidence for two of the three distinct periods of increased violence against civilians by MPLA: post-electoral violence in 1992, and at the very end of the war. Given the controversies around the events in the late seventies, it is not surprising that the ACLED data do not show these events. The overall extremely low levels of MPLA violence against civilians up to 1990 also suggest that MPLA events may be slightly underreported until 1990.

Sexual violence against civilian women (survey data)

Turing to individual survey data, the left graph in **Figure 2a** presents a local non-parametric regression of self-reported exposure to sexual violence on date of birth. Due to a very limited number of observations per year of entry, the standard errors are obviously large. However, the visual intuition is striking. Keeping mind that the mean age of entry is around 19, the graph clearly suggests a higher mean exposure for MPLA soldiers (blue) who were more likely serve a) in the late seventies and b) at the very end of the war. This pattern is consistent with an interpretation of the 1992 post-electoral violence as fundamentally different from the other two episodes of violence against civilians, in terms of sexual violence. Secondly, the temporal variation for UNITA soldiers is very different, which means that the

interaction of which army a soldier joined and when he was born is potentially an *informative* source of variation in exposure to sexual violence.

A critical challenge is to isolate variation in exposure to sexual violence especially from variation in related forms of violence against civilians and war violence more generally. **Figure 2** therefore plots similar graphs of exposure to other forms of war violence, based on individual survey data. Starting with variation in MPLA soldiers (blue lines), these unconditional plots reveal two main insights. First, violence not directed at civilians (**Fig 2c**) and witnessing civilians deaths have clearly different patterns (**Fig 2b, left**). Second, temporal variation in exposure to massacres (**Fig 2b, right**) and especially looting (**Fig 2a, right**) appear similar to the variation in exposure to sexual violence among MPLA soldiers. Yet, turning to comparisons across armies, Figures 2a and 2b reveal that the temporal variation in looting and massacres among UNITA soldiers clearly differ from that in sexual violence. This means that the interaction of which army a soldier joined and when he was born is potentially not only an informative but also *valid* source of variation in exposure to sexual violence.

If the patterns in these unconditional relationship hold, we expect that the interaction of being "born into" a five year-bin corresponding to either period 1 or 3 from above, i.e. shifted by the mean age of entry of about 20 years, and being MPLA, on average, significantly *increases* the probability of being exposed to sexual violence. As we define the army indicator is a binary UNITA indicator, we expect a significant *decreases* in the probability of being exposed to sexual violence, if the interaction is defined based on this dummy.

4.3 Econometric framework

OLS

We start with a simple linear regression model, and assume that experience with wartime sexual violence and the error term are statistically independent, conditional on other control variables we include (conditional independence assumption (CIA)). If this holds, the regression derivative equals the average causal effect (ACE), conditional on this set of controls. We sequentially add variables that may correlate with both wartime sexual violence and partner violence today and hence potentially bias the simple bivariate estimate. First, all specifications include location fixed effects to purge systematic variation across locales. Second, we include pre-military service, and therefore pre-treatment, household background characteristics and location fixed effects to control for pre-existing differences. Third, we carefully consider (potentially confounding) socioeconomic variables that have been linked to perpetrating partner violence and might simultaneously co-vary with wartime sexual

violence. Fourth, we carefully explore potential wartime confounding variables as experiences of violence, again bearing in mind that these may not be determined exogenously themselves. To implement the strategy we estimate the following equation using OLS:

$$D = \beta W + \gamma' X + \varepsilon \qquad (1)$$

Here, D is the indicator of contemporaneous violence against intimate partners, W denotes exposure to wartime sexual violence. X is a flexible vector of the controls variables described above, and ε is the error term.

We turn now to the error term covariance matrix. Errors may not be homoscedastic and correlated within "clusters". For example, it may be reasonable to assume that observations on individuals drawn from the same pre-service location (cluster1) are correlated with each other, but observations on individuals from different locations are not. Similarly observations on individuals sampled in the same current location (cluster2) are correlated with each other, but observations on individuals from different locations are not.

To correct for these patterns in the error term, in our standard specifications we use robust standard errors, corrected for clustering at the pre-service and post-service location levels (e.g. Cameron et al. 2011). There are other approaches to dealing with clustering that put more structure on the matrix and are more efficient but less robust. A commonly used alternative is the Moulton (1986) approach to obtaining consistent standard error, which is in effect to specify an error components structure. In addition, either number of clusters is below 50 which raises the concern potential concern that we underestimate standard errors as a consequence of the clustering. As a robustness check we show that our results do not change when we use Huber-White, Moulton-corrected, or wild cluster bootstrap standard errors. Similarly, our estimates of the standard error do not change noticeably across these specifications, when we cluster only at the pre-service location level, only at the current location level or use simply robust (non-clustered) errors. *Results are available upon request.*

Endogeneity bias

Yet, there is reason to suspect non-orthogonality between regressors and errors. If CIA fails, then the equality of the regression derivative and ACE fails. The OLS is not appropriate as the OLS estimator will be biased and inconsistent. To mitigate concerns of endogeneity bias, we use an instrumental-variables strategy (IV), based on the identifying variation described in section 4.1. However, the use of IV estimation to address this problem must be balanced against the inevitable loss of efficiency vis-a-vis OLS. When W and e are actually not

correlated at all, the asymptotic variance of the IV estimator is always larger than that of the OLS estimator (e.g. Wooldridge 2003).

Table 3 explores individual traits associated with exposure to sexual violence during the war, based on our survey data, and compares raw means between the subs-samples defined by the binary exposure to sexual violence-indicator. The top panel suggests that the two sub-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 having been part of a unit operating mostly in combat areas. The bottom panel presents summary statistics on the five-year-bins of pooled birth cohorts to reveal 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 different individual pre-service traits than then non-exposed. A veteran exposed to sexual violence is more likely to report having been a student at the time of recruitment, having had any schooling, and having been significantly stronger, physically and mentally, as compared to their peers. 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 should be interpreted with caution, it suggests two central 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 may. The latter supports concerns that W and e may not be distributed independently, and IV estimation is necessary.

IV

As usual, instrumental variables must satisfy two requirements: they must be correlated with the included endogenous variable, and orthogonal to the error process.

IV- Relevance

We test the relevance condition by examining the fit of the “first stage regressions”. If the excluded instruments have little explanatory power (“weak instruments”), this produces finite sample bias in the estimated IV coefficients and conventional asymptotics fail. Since the size of the IV bias is increasing in the number of instruments (e.g. Hahn and Hausman 2002), being parsimonious in the choice of instruments is favorable. As the baseline we therefore use a single instrument, which is median-unbiased (e.g. Angrist and Pischke 2008).

The generally adopted rule of thumb for a single endogenous regressor is that an F-statistic below 10 is cause for concern (Staiger and Stock (1997)).

Based on the logic presented in section 4.1, the excluded instrument is a binary indicator that equals one if a veteran's army was UNITA and he was born in a period captured by bins 3,7 or 8.

We estimate the following "first stage equation" using OLS:

$$W = \delta\,(\text{UNITAxCohort}) + \lambda' X + \nu \qquad (2)$$

Here, W denotes exposure to wartime sexual violence, X is the full vector of control variables, including the main effects of UNITA and Cohorts, and ν is the error term. Unless explicitly stated, the fifth cohort is the reference category[9].

Table 4 presents first stage results. As expected, columns 1 and 2 show that being with UNITA and born in cohorts 3, 7 or 8 predict a substantial decrease in exposure to sexual violence. Following Angrist and Pischke (2008), we choose our single best instrument for our main specifications. We collapse the birth cohort dummy variables into a binary indicator "Bin3/7/8", which compares being born in cohort 3,7 or 8 to all others, and interact this dummy with the UNITA dummy variable to form our single instrumental variable. Columns 2, 3 and 4 show that the instrument is strongly relevant, with and without the inclusion of (basic) post-treatment controls, with F-statistics above 10.

IV- Validity

The identifying assumption of the IV-strategy is that the distribution of the instrument is uncorrelated with the distribution of the error term in Equation 1. We present four sets of results related to this condition, based on our baseline binary instrument.

First, **Table 5** repeats the exercise of Table 2, and compares raw means in individual characteristics between the sub-samples now defined by the binary instrument[10]. There is, a priori, no reason to expect any differences in means across the two groups. A for the actual exposure to sexual violence, 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.

[9] This bin was chosen as veterans born in these years were most likely to serve in the late 80's which was characterized by large-scale conventional battlefield war.
[10] We omit the army and year-of-birth indicators on which the instrument is built.

Second, we investigate whether family background characteristics, such as religion or demographic indicators predict the value of the instrumental variable. Inspired by models of rebel recruitment (e.g. Blattman and Annan 2010) we estimate the following multivariate specification:

$$\text{UNITAxCohort378} = \pi'T + \nu \qquad (3)$$

Here, UNITAxCohort378 (the IV) is the dependent variables, X is a vector of twelve pre-service background characteristics, ν is the error term. We expect that no component of π is statistically significant from zero. There is, a priori, again no reason to expect any significant effects. The results displayed in **Table 6** confirm that no family background characteristic predicts the value of the instrument variable, with and without pre-service location fixed effects.

Third, as we want to isolate variation in sexual violence, we estimate the "first stage" Equation 2, using the full set of war and war-related outcomes as the dependent variables in separate regressions. Based on the graphical illustration in Section 4.1 of the unconditional relationships between the outcomes and the UNITA indicator over time, we expect that no statistically significant correlation exists between the instrument and any of these outcomes. The results displayed in **Table 7** confirm this. In particular, the IV is not a relevant source of variation in exposure to looting and massacres, as we expected based on the very different patterns over time among MPLA veterans (the red lines in **Figure 3**).

Finally, to strengthen confidence in our instrumental variable further, we estimate the "first stage" Equation 2, now using post-war outcomes, which may correlate with intimate partner violence, as the dependent variables in separate regressions. Even if the IV does not predict any other relevant war experiences, it must not be correlated with any relevant factor, hidden in the error term of Equation 1. The results displayed in **Table 8**confirm that the IV does not predict variation in any of these outcomes.

IV- Estimation

For our main results, we produce IV estimates of Equation 1 using the "First-stage" equation 2. In the presence of heteroscedasticity 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 specification we estimate the IV coefficients using robust standard errors, corrected for clustering at the pre-service and post-service location levels, as in OLS regression.

5 MAIN RESULTS [VERY PRELIMINARY]

5.1 OLS

Table 9 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 this result is remarkably robust in terms of statistical significance, as well as in magnitude, when we include a series of control variables. Notably, no other forms of intense war violence, which have been linked to other post-war outcomes (e.g. Blattman 2009, Bellows and Miguel 2009) neither change the main coefficient significantly, nor have a significant effect on domestic violence in these models. Similarly, basic post-controls are carefully introduced, but do not change the coefficient of interest either.

5.2 IV

IV- and corresponding OLS-estimates for models including the UNITA and pooled cohort main effects are displayed in **Table 10**. The IV estimates confirm the positive effect of exposure to wartime sexual violence on domestic violence 12 years after the end of the war, found by OLS. Inclusion of a measure of civilian targeting, which is likely the most related wartime experience, does not alter results substantially. IV estimates exceed the OLS estimates and are not statistically significant, which, due to the parsimonious specifications and the moderate sample size, is not surprising. To gain efficiency, we use the three interaction terms separately as instruments in column 5, and observe a considerable gain in efficiency, while the coefficient only changes marginally (and adds confidence in our IV estimates). Column 6 uses the full set of interactions as instruments, which are likely not valid. Yet, as expected we observe even more gains in efficiency and the estimate in column 6 attains statistical significance. Even though, our preferred IV specifications do not attain statistical significance, we conclude that there is strong evidence that the positive effects of exposure to wartime sexual violence on perpetrating post-war domestic violence has a causal interpretation.

6 MECHANISMS

Based on the theoretical arguments in section 2, a positive link between exposure to wartime sexual violence and post-war domestic violence is consistent with a mechanism based a) on gender-based violent practice and b) norms of gender and masculinity.

The dominant view in economics of domestic violence is economic bargaining, yet theory would predict a negative link, unless the exposure to sexual violence *increases* the veterans economic bargaining power. **Table 11** reports the direct effects of exposure to violence on specific outcomes of or related to economic bargaining, and provides further evidence against such a mechanism. We find no significant effect on the veteran's cash income, his labor force participation, and relative cash income (relative to his partner's).

Table 12 looks at sexual and non-sexual physical domestic violence separately, non-physical domestic conflict (disputes), and domineering behavior- all outcomes reported by the veteran's partner. The main, positive effect on physical domestic violence turns out to be driven by an increase in non-sexual domestic violence (column 1). The effect on sexual violence is small and not statistically significant (column 3). Turning to non-physical domestic conflict, column 5 shows that more exposure to wartime sexual violence is also associated with a higher level of disputes between partners**.** Interestingly, we find a weakly negative effect on domineering behavior by the veterans. All results hold when we controls for the same of post-war controls as in Table 9.

The results in Table 12 point away from an underlying mechanism based on models of gender and masculinity, which would predict a positive effect on domineering. Yet "socialization into gender-based violent practice" may also be a normative or behavioral/interactional process. **Table 13** reports insights on the direct role of three specific factors related to a normative or behavioral/interactional process: total time with the armed group, total time since leaving the group, and rank. In a normative interpretation we would expect a stronger effect among those who interact more or longer with other (similar) soldiers, or that it possibly vanishes over time. On the other hand, if low rank soldiers are ordered to strategically rape, we might expect the main effect to interact with rank. Yet, as reported in Table 13 we find no statistically significant interaction, suggesting that effects are persistent and not (strongly) normatively reinforced by interactions with fellow soldiers.

Taken together, these findings are in favor of an underlying effect based on "socialization into gender-based violent practice". One explanation consistent with the results is a lasting reduction or even breakdown of natural obstacles to gender-based violence.

7 CONCLUSION

This paper introduces and studies a new long-term link between exposure to wartime violence and post-war behavior. Our key result is that exposure to wartime sexual violence significantly increases a soldier's propensity to commit domestic violence more than a decade after the end of the war. We find strong effects on non-sexual physical violence, similar and

related effects on disputes between intimate partners, no effects on sexual violence, and a slightly negative effect on domineering behavior. Taken together, we interpret these findings as in favor of an underlying effect based on gender-based violent behavior rather than living up to gender norms. One explanation consistent with the results would be a lasting reduction or even breakdown of natural obstacles to gender-based violence. We find no evidence for alternative theories of domestic violence, based on a reduction in economic bargaining power or high levels of distress. This supports a perspective of domestic violence as expressive behavior that is not triggered by payoff-relevant considerations.

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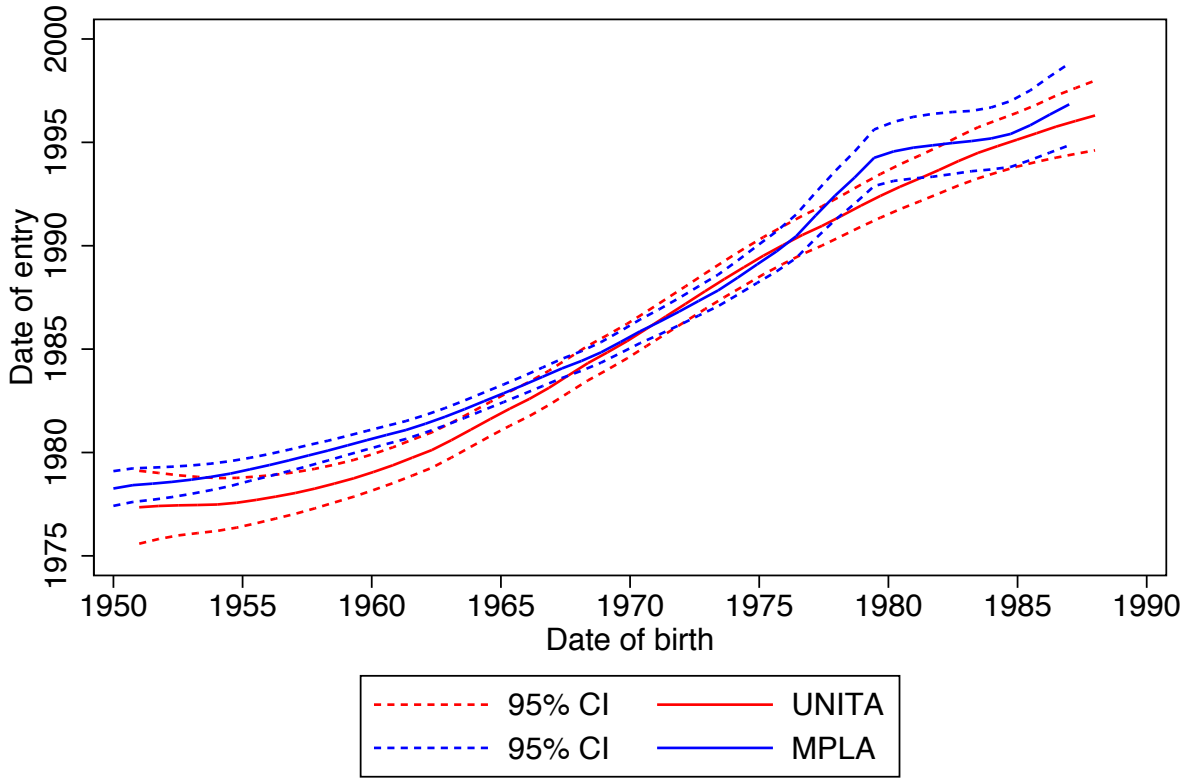
Tables and Figures

December 31, 2015

Figures



Notes: Local non-parametric regression. Kernel = Epanechnikov. Bandwidth (UNITA) = 3.56, Bandwidth (MPLA) = 3.02 [default]

Figure 1: Local polynomial smooth of date of military entry vs date of birth, split by army.

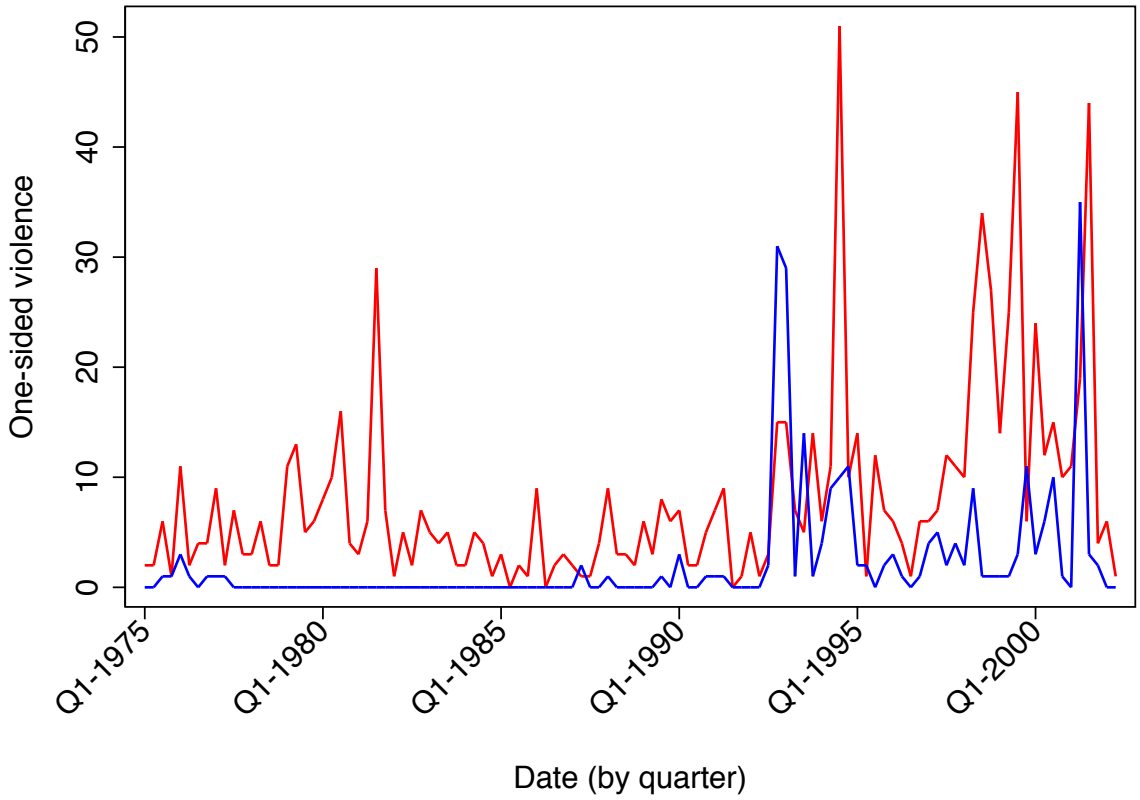
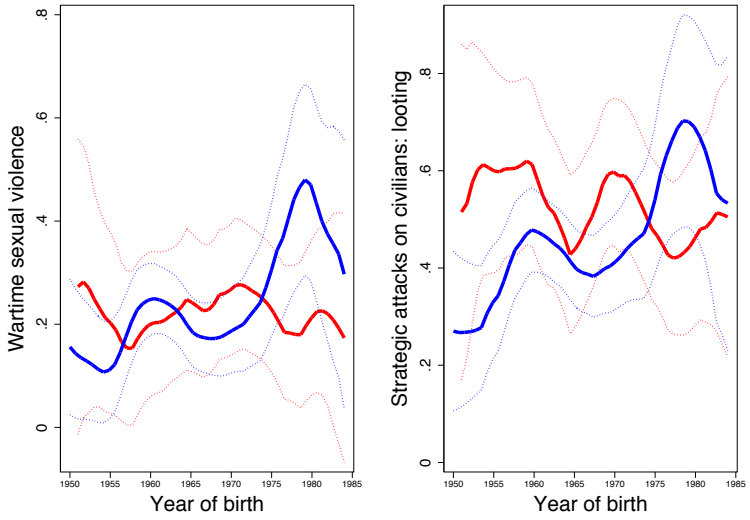
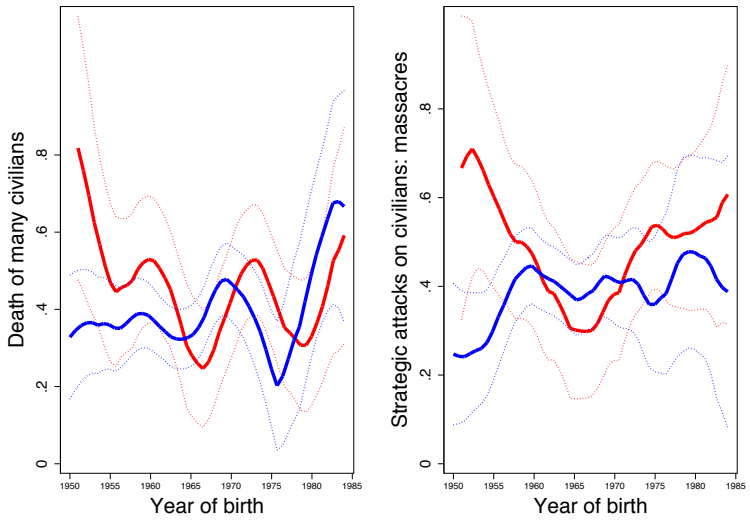


Figure 2: Event data: (Any) violence against civilians. Blue = MPLA, Red = UNITA

(a) Non-fatal violence against civilians.



(b) Fatal violence against civilians.



(c) Fatal violence against combatants.

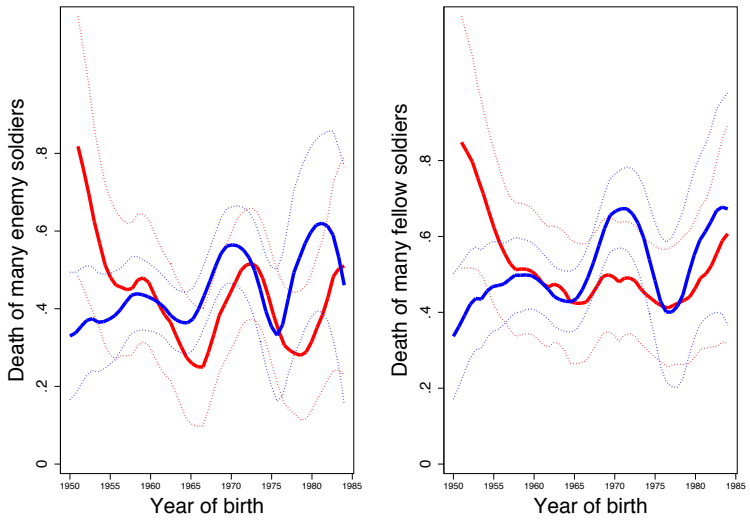


Figure 3: Exposure to different forms of extreme violence (survey data). Blue = MPLA, Red = UNITA

Tables

Table 1: Summary statistics: binary analysis

	Mean	SD	MIN	MAX
Key dependent variable				
Spousal violence in last 12 months (0/1)	0.44	0.497	0	1
Eight individual acts				
Pushed you, shook you, or threw something at you (0/1)	0.35	0.479	0	1
Slapped you (0/1)	0.43	0.495	0	1
Twisted your arm or pulled your hair (0/1)	0.15	0.360	0	1
Punched you with his fist or with something that could hurt you (0/1)	0.24	0.428	0	1
Kicked or dragged you (0/1)	0.28	0.447	0	1
Tried to choke, strangle or burn you (0/1)	0.12	0.320	0	1
Threatened or attacked you with a knife, gun, or other weapon (0/1)	0.06	0.233	0	1
Physically forced you to have sexual intercourse(0/1)	0.12	0.327	0	1
Key explanatory variable				
Situations where a civilian woman was sexually abused (0/1)	0.21	0.410	0	1
Other key variables				
Age	49.56	9.682	26	86
UNITA (main)	0.30	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			

Note: Spousal violence indicator equals one if spouse experienced any of the specific violents in the last 12 months.

Table 2: Selection into army.

	UNITA first army	
	(1)	(2)
Household size	−0.002	−0.003
	(0.823)	(0.645)
Church = Catholic (d)	0.006	−0.039
	(0.934)	(0.605)
Church = IECA (d)	−0.045	−0.025
	(0.572)	(0.752)
Number of rooms	0.010	−0.009
	(0.542)	(0.585)
Land size	−0.005	−0.002
	(0.578)	(0.798)
Livestock	−0.002	−0.002
	(0.376)	(0.352)
Farmer(d)	0.102	0.037
	(0.212)	(0.655)
Radio (d)	−0.047	−0.023
	(0.244)	(0.550)
HH members had joined (#)	0.001	−0.005
	(0.969)	(0.722)
Ever displaced (d)	−0.023	−0.027
	(0.565)	(0.485)
Father's schooling (d)	−0.016	−0.042
	(0.713)	(0.334)
Mother's schooling (d)	0.001	−0.004
	(0.988)	(0.922)
Pre-Location FE	No	Yes
Observations	578	578

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Who is exposed to sexual violence?

	Sample			Difference	
		Exp. to sexual violence			
	Full	Yes	No	Diff	p
Military					
UNITA (main)	0.30	0.34	0.29	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
Rank: private	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 (pre-service)					
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 (1-10)	5.12	5.13	5.12	0.00	0.99
Self: strength (1-10)	6.00	6.50	5.87	0.63***	0.01
Self: mental (1-10)	6.24	7.07	6.03	1.04***	0.00
Birth cohorts					
YOB $\leqslant$ 1950	0.12	0.11	0.12	−0.01	0.83
1950 $>$ YOB $\leqslant$ 1955	0.09	0.06	0.09	−0.04	0.14
1955 $>$ YOB $\leqslant$ 1960	0.16	0.13	0.17	−0.05	0.17
1960 $>$ YOB $\leqslant$ 1965	0.23	0.24	0.23	0.02	0.66
1965 $>$ YOB $\leqslant$ 1970	0.18	0.19	0.18	0.01	0.72
1970 $>$ YOB $\leqslant$ 1975	0.12	0.14	0.11	0.03	0.41
1970 $>$ YOB $\leqslant$ 1980	0.06	0.08	0.05	0.02	0.29
YOB $>$1980	0.04	0.06	0.04	0.01	0.45

Note: 'UNITA' equals one if main army was UNITA (zero means MPLA). '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 traininging' 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. 'Birth cohorts' are pooled birth cohort indicators and equal one if was born in indicated range.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: First-stage and reduced form

	First-stage			
	(1)	(2)	(3)	(4)
UNITA x Bin3/7/8			−0.223***	−0.208***
			(0.068)	(0.053)
UNITA x Cohort 1	−0.124			
	(0.111)			
UNITA x Cohort 2	−0.027			
	(0.099)			
UNITA x Cohort 3	−0.152	−0.095***		
	(0.094)	(0.028)		
UNITA x Cohort 4	−0.042			
	(0.066)			
UNITA x Cohort 6	0.015			
	(0.146)			
UNITA x Cohort 7	−0.482***	−0.440***		
	(0.174)	(0.136)		
UNITA x Cohort 8	−0.197**	−0.200***		
	(0.080)	(0.050)		
Age+Age2	No	Yes	No	Yes
Post-Location	No	Yes	No	Yes
Pre-Location	Yes	Yes	Yes	Yes
Observations	578	578	578	578
F(UNITA x COB = 0)	10.85	24.79	10.86	15.26

Note: 'Cohort' denotes pooled year of birth-cohorts (reference cohort:"1965 $>$ YOB $\leqslant$ 1970") 'UNITA x Bin3/7/8' denotes the interaction of being UNITA and being born pooled year of birth-cohort 3,7 or 8.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered at the current and pre-service location levels.

Table 5: Are individual traits correlated with binary instrument?

	Sample			Difference	
		Excl. var. equals one			
	Full	Yes	No	Diff	p
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

Note: '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 traininging' 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.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: IV robustness check 1: Correlation with pre-service variables (multivariate analysis).

	UNITA x Cohort3/7/8	
	(1)	(2)
Household size	−0.002	−0.003
	(0.534)	(0.469)
Church = Catholic (d)	−0.032	−0.038
	(0.405)	(0.322)
Church = IECA (d)	−0.046	−0.052
	(0.274)	(0.214)
Number of rooms	−0.011	−0.013
	(0.223)	(0.170)
Land size	0.002	0.002
	(0.648)	(0.677)
Livestock	−0.001	−0.001
	(0.522)	(0.435)
Farmer(d)	0.008	−0.007
	(0.852)	(0.883)
Radio (d)	−0.032	−0.032
	(0.144)	(0.149)
HH members had joined (#)	−0.005	−0.006
	(0.547)	(0.480)
Ever displaced (d)	0.014	0.020
	(0.550)	(0.389)
Father's schooling (d)	−0.006	−0.015
	(0.801)	(0.539)
Mother's schooling (d)	−0.006	−0.014
	(0.820)	(0.570)
Pre-Location FE	No	Yes
Observations	578	578

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: IV mechanics: Correlation with forms of battle violence and violence against civilians.

	(1) Fellow soldiers died	(2) Enemy soldiers died	(3) Civilians died	(4) Looting	(5) Civilian massacres	(6) Wartime sexual violence
UNITA x Bin3/7/8	−0.037	−0.057	−0.009	0.040	−0.090	−0.221***
	[0.784]	[0.619]	[0.936]	[0.730]	[0.310]	[0.000]
UNITA+Cohort	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578
R^2	0.05	0.02	0.04	0.02	0.02	0.03

Note: '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. 'Cohort' denotes pooled year of birth-cohorts (reference cohort:"$1965 > \text{YOB} \leqslant 1970$") 'UNITA x Bin3/7/8' denotes the interaction of being UNITA and being born pooled year of birth-cohort 3,7 or 8.

p-values in brackets.

Table 8: IV robustness check 2: Correlation with post-war variables.

	(1) Woman earns more	(2) Any formal education	(3) Asset index	(4) Born here	(5) Radio
UNITA x Cohort3/7/8	0.002 [0.985]	0.008 [0.802]	0.055 [0.487]	−0.087 [0.345]	0.011 [0.934]
UNITA+Cohort	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578

Note: 'Woman earns more' equals one if woman earns more cash incom. 'Any formal education' equals one if soldier received any formal education. 'Asset index' is a household assets measure, based on 19 items. 'Born here' equals one if soldier lives in the comuna where he was born. 'Radio' equals one if the household possesses a radio device.

p-values in brackets.

Table 9: OLS: Spousal violence (1/0) and exposure to wartime sexual violence (1/0)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Wartime sexual violence	0.119**	0.130**	0.130***	0.130***	0.130***	0.132***	0.126***	0.121**
	(0.054)	(0.059)	(0.047)	(0.047)	(0.047)	(0.050)	(0.044)	(0.048)
UNITA (main)			0.009	0.009	0.008	0.009	0.010	0.007
			(0.047)	(0.047)	(0.071)	(0.047)	(0.048)	(0.048)
Length of service					0.000			
					(0.005)			
Fatal violence...								
..fellow soldiers						−0.012		
						(0.042)		
..enemy soldiers							0.016	
							(0.039)	
..civilians								0.041
								(0.039)
Post-Controls	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578	578	578	578
R^2	0.09	0.14	0.18	0.18	0.18	0.18	0.18	0.19

Note: 'Wartime sexual violence' equals one if respondent reports having experienced at least once a situation where a civilian woman was sexually abused, during wartime. '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. 'UNITA' equals one if main army was UNITA (zero means MPLA). '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.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered at the current and pre-service location levels.

Table 10: IV: Spousal violence (1/0) and exposure to wartime sexual violence (1/0)

	Dep. var.: Spousal abuse					
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	IV	IV	IV	IV
Instrument(s)			3/7/8	3/7/8	3+7+8	Full
Wartime sexual violence	0.130**	0.115**	0.350	0.387	0.375	0.437*
	(0.059)	(0.056)	(0.602)	(0.666)	(0.288)	(0.224)
Civilians targetted		0.044		−0.032		
		(0.040)		(0.187)		
Cohort + UNITA	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	0.15	0.19	0.12	0.15	0.11	0.09

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered at the current and pre-service location levels.

Table 11: Economic bargaining power and exposure to wartime sexual violence (1/0).

	(1) Cash income (log)	(2) In labor force	(3) Woman earns more	(4) Cash income difference (lev.)	(5) Woman's share of cash income
Wartime sexual violence	0.367	−0.030	−0.008	−1963.771	0.017
	(0.374)	(0.019)	(0.074)	(2720.735)	(0.050)
Age	Yes	Yes	Yes	Yes	Yes
Cohort + UNITA	Yes	Yes	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes	Yes	Yes
Observations	578	578	578	578	578
R^2	0.19	0.18	0.14	0.12	0.15

Note: Column 1: cash income earned by veteran in last 4 weeks (log). Column 2: Indicator equals one if veteran is in labor force, zero otherwise. Column 3: Indicator equals one if woman earned more cash income in last 4 weeks, zero otherwise. Column 4: Absolute difference between man's and woman's earned cash income in last 4 weeks. Column 5: Woman's share of total earned cash income in last 4 weeks.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered at the current and pre-service location levels.

Table 12: Different forms of domestic conflict and exposure to wartime sexual violence (1/0).

	Physical				Non-physical			
	(1) Non-sexual	(2) Non-sexual	(3) Sexual	(4) Sexual	(5) Disputes	(6) Disputes	(7) Domineering	(8) Domineering
Wartime sexual violence	0.128***	0.139***	0.006	0.004	0.127**	0.128**	−0.012**	−0.012***
	(0.050)	(0.044)	(0.024)	(0.023)	(0.058)	(0.059)	(0.005)	(0.001)
Post controls	No	Yes	No	Yes	No	Yes	No	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort + UNITA	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	578	578	578	578	577	577	578	578
R^2	0.15	0.17	0.16	0.16	0.19	0.20	0.16	0.19

Note: The "spousal violence" indicator includes acts of (non-sexual) physical violence and sexual violence. The indicator in Columns 3 and 4 includes acts of (non-sexual) physical violence only, the indicator in columns 5 and 6 includes acts of sexual violence only. "Disputes" equals one if woman reports that disputes of the couple occur 'often' or 'very often', zero otherwise. "Domineering" equals one if woman reports any of five non-physical acts by the man of seeking control over her behavior, zero otherwise (none).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered at the current and pre-service location levels.

Table 13: Heterogeneity.

	Dep. var.: Domestic violence		
	(1)	(2)	(3)
Wartime sexual violence x Time in military	−0.001		
	(0.005)		
Wartime sexual violence x Time since left military		0.003	
		(0.010)	
Wartime sexual violence x Low rank			0.014
			(0.113)
Cohort + UNITA	Yes	Yes	Yes
Post-Location	Yes	Yes	Yes
Pre-Location	Yes	Yes	Yes
Observations	578	578	578
R^2	0.15	0.15	0.15

Note: 'Time in military' denotes length of wartime military service [in years]. 'Time since left military' denotes time since left military [in years]. 'Rank:private' equals one if was private (low rank).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered at the current and pre-service location levels.