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On the Legacies of Wartime Governance

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Abstract:

In conflict zones around the world, both state and non-state actors deliver governance at local levels. This paper explores the long-term impact of individual exposure to 'wartime governance' on social and political behavior. We operationalize wartime governance as the local policy choices and practices of a ruling actor. Building on detailed ethnographic and historical insights, we use survey data and a natural experiment to show that involvement in wartime governance by armed groups makes Angolan war veterans more likely to participate in local collective action twelve years after the end of the war. This effect is underpinned by social learning and a shift in political preferences, but has no bearing on political mobilization at the national level or cooperation within the family. Our study documents a wartime source of pro-social behavior among veterans and exposes challenges and opportunities for bottom-up approaches to post-conflict state-building and local development.

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1 Introduction

War strongly affects social and economic development via institutional change. External war is a central explanation for the emergence of effective state institutions that deliver economic prosperity and stability in modern European nations (e.g. Tilly, 1975; Besley and Persson, 2009; Gennaioli and Voth, 2015). Internal war, by contrast, is associated with debilitating state institutions (e.g. Bates, 2001; Besley and Persson, 2008; Chowdhury and Murshed, 2013), economic 'development in reverse' and conflict traps (e.g. Abadie and Gardeazabal, 2003; Collier, 2003; Rohner, Thoenig, and Zilibotti, 2013a).

Most existing models of these war impacts presume a unitary government that has the monopoly of force over the entire national territory and of investments in state institutions (Besley and Persson, 2008, 2011, 2014). Yet, states affected by internal war – the predominant form of violent conflict today – often lack de-facto control over substantial parts of the national territory as non-state rulers compete for power and governance is regionally fragmented (e.g. Kalyvas, 2006). Under certain conditions, non-state rulers have (or develop) the institutional capacity to successfully claim legitimacy, exert authority and enact policies of public good provision and taxation in a given territory (Arjona, Kasfir, and Mampilly, 2015; Sánchez de la Sierra, 2015). To date, the legacies of these prevalent forms of local institutional change in conflict zones are largely unknown.

This paper studies the causal long-term impact of local institutional change in civil wars on social and political behavior. Specifically, we investigate the link between exposure to 'wartime governance' by armed groups as a soldier and post-war civic engagement as a civilian more than a decade later.

We operationalize wartime governance as the local policy choices and practices by a ruling actor.[1] We focus on the supply of goods and services (to the governed populace) as these are at the heart of 'good governance' and an effective way of building legitimacy, enhancing local productivity and raising tax revenues (e.g. Levi, 1989; Berman, Shapiro, and Felter, 2011; Sánchez de la Sierra, 2015). The actual supply exposes significant numbers of soldiers to governance since the delivery of many local public goods, such as security or infrastructure, requires large-scale, collective and labor-intensive projects. To reflect demand side aspects, our approach to wartime governance also accounts for input and feedback from civilians.[2]

Civic engagement, and contributions to collective public good production in particular, are central to development outcomes, especially in post-conflict societies, for at least two

[1] Our conceptual approach to wartime governance is based on a ruling actor's 'institutional capacity', which constrains its policy choices and practices. This view is conceptually consistent with 'state capacity' constraining the policy choices of an incumbent national actor as in the framework of Besley and Persson (2011). The focus of the paper is on governance by civil war actors. The theoretical and conceptual arguments presented will, however, similarly apply local or regional governance in other forms of armed conflict.

[2] We will discuss this definition in further detail in Section 2 and Appendix B.

reasons. First, a key challenge faced by less developed and fragile regions is the failure of central governments to deliver public goods. Basic public goods and services are often produced locally and collectively, and civic engagement is paramount to their production and delivery. Second, high levels of collective public good production may also strengthen social norms of participatory citizenship and collective institutions, which are vital components of inclusive and cohesive societies.[3]

Theoretically, it is a priori unclear whether individual involvement in wartime governance increases or decreases long-term civic engagement. We propose and test four theoretical mechanisms: economic interactions, social attitudes, learning and political preferences. Testing the causal net relationship and these underlying mechanisms presents considerable data and identification challenges. First, high quality micro-level data on conflict experiences beyond violence are extremely rare. Second, unobserved individual-level factors may co-vary simultaneously with involvement in wartime governance and post-war civic engagement. To overcome these challenges we collected primary survey data from 759 war veterans and rely on a natural experiment in Huambo province, the center of the 1975-2002 Angolan Civil War. The research design built upon twelve months of ethnographic research of social behaviors among ex-combatants in Huambo province (Spall, 2015).

The survey dataset includes detailed information on war and military service experiences, pre-service background characteristics and post-war social behaviors in 2014. The main outcome variables distinguish participation in two stages of the post-war production of local public goods: planning and delivery. In the planning stage, we measure engagement in community meetings, the key deliberative institutions that organize collective action in Angola.[4] We assess the involvement of war veterans in actual public goods delivery by observing their participation in local collective initiatives that provide public security.[5] Informal security institutions play an important and positive role in the Angolan context, and participation in these collective organizations is open, voluntary and not compensated materially. Specific functions include preventive patrolling and the resolution of conflicts between villagers.[6] The good provided is of public nature because it benefits everyone in the village, and participation in these groups is hence a valid measure of being involved in the

[3] See, for instance, Sen (1970), Ostrom (1990), Putnam (1993), Guiso, Sapienza, and Zingales (2011); and Alesina and Giuliano (2015).

[4] See Rodella (2010) for a detailed study of local social capital in Angola.

[5] Security is one of the three most fundamental public goods (Hoffman, 2015) and is of particular importance in conflict-affected contexts, where formal justice institutions are severely dysfunctional, the state is limited in reach, and insecurity is pervasive (Bateson, 2013). The average level of development – especially in rural areas – remains extremely low in post-war Angola, and extensive preliminary and ethnographic research confirmed that security is a top priority among Angolans and defined it as a focal outcome of the study.

[6] One potential concern may be that these groups resemble criminal networks, and that participation in them may actually be 'bad'. Yet, these groups are neither organized nor perceived in any way like gangs or militias, and participation is universally viewed positively, as confirmed by qualitative fieldwork.

delivery of a key public good. The main treatment variable is an index measure of seven items of involvement in the delivery of these goods and services to local civilian populations.[7]

To establish causality, we exploit that the Angolan Civil War generated exogenous variation in the likelihood of being exposed to wartime governance. The 27-year war was an intense and dynamic case of mass militarization and military competition between two high-capacity actors, the *Movimento Popular de Libertação de Angola (MPLA)* government and the *União Nacional para a Independência Total de Angola (UNITA)* rebels. Both organizations emerged as national movements opposing the Portuguese colonial rule in a war of independence between 1961 and 1974, and from 1975 to 2002 invested heavily in their state-building missions (e.g. Pearce, 2011). While the MPLA was the nominal government, UNITA built a 'state in the state' (e.g. Roque, 2015). The MPLA relied on oil revenues and support by Cuba and the Eastern bloc, while UNITA drew its strength from diamond trading and its United States and South African allies. Both sides used their institutional capacity to enforce compulsory military service for all men at (approximately) the age of 19 in territories under their control (see Stojetz (2016) and Spall (2015)).

The extraordinary size, highly organized structure and advanced military technology of both armed forces resulted in (predominantly) large-scale conventional warfare and allowed both sides to secure and govern large territories across the country. The most hotly contested region was Huambo province in the center of the country. Territorial control was highly volatile in this region and all sub-regions changed hands multiple times over the course of the war. In an in-depth, retrospective study of civilians' wartime political identity in Huambo, many people "would talk of having been 'UNITA people' and 'MPLA people' at different stages of their lives [which] demonstrates that territorial control, more so than regional or ethno-linguistic background, became an important determinant of political identity" (Pearce, 2012, 463). As a consequence, many men from the same region, village and even family served on different sides, and state and rebel army recruits from this region were very similar in background. This means that conscription exogenously selected individuals into different military entry dates (via their date of birth) and different armies (via shifts in territorial control).

Our identification strategy exploits the *interaction* of a man's date of birth and the army he joined as a source of exogenous variation in the (ex-ante) likelihood of individual exposure to wartime governance. Technically, the strategy follows the same logic as a difference-in-differences design: we hypothesize that the expected difference in exposure to wartime governance between army assignments varies across birth cohorts.

[7]The seven items are: provision of services (such as providing access to education), building physical infrastructure (such as schools), provision of arms, help with conflict resolution between villagers (e.g. over land), provision of protection and security, requests by villagers for help with conflict resolution between villagers, and requests by villagers to protect the village. This list was inspired by leading accounts of local governance by armed groups (Mampilly, 2011; Arjona, Kasfir, and Mampilly, 2015). We will motivate and discuss the index and its component in more detail in Section 4 and Appendix B.

This interaction created significant variation in the likelihood of individual exposure to wartime governance for the following reason. As noted earlier, both groups in Angola had strong state-building missions and the objective of winning over people's 'hearts and minds' across the country. Thus, we expect that both the simple demand as well as the strategic incentives to engage and invest (more) in wartime governance will be higher after (more) gains of new territory. If the extent of gains of new territories by a given group varies over time, the expected level of public good provision by that group will then also vary over time. At the individual level, this suggests the soldiers who were (ex-ante) more likely to serve in periods in which their army would gain (more) new territory, would – on average – be more likely to be (more) exposed to wartime governance.

Due to UNITA's strength, its extent of territorial gains was often comparable to those of the MPLA. The literature on the Angolan war outlines two periods where external factors helped the MPLA gain significantly more territory than UNITA. First, the MPLA drew strength from a large number of Cuban troops deployed to Angola in the mid 1980s, which UNITA could not match consistently. Second, in the late 1990s the United States' withdrawal of support and international sanctions against the diamond trade led up to UNITA's defeat. Based on the compulsory nature of military service in the Angolan War, we expect that joining UNITA *in combination with* being born 20 years before one of these two distinct periods (the 'penalized group') made individuals significantly less likely to be exposed to wartime governance than others with a similar personal background.

The identifying assumption is that being selected into the penalized group of army and date of birth combinations is not correlated with unobservable, confounding individual traits or experiences. There are two main threats over the excludability of the instrument. First, individuals recruited in the penalized group may have differed systematically in background traits that correlate with the proclivity for civic engagement. Due to the conscriptive nature of (most) recruitment into either side throughout the war this scenario is unlikely. Nonetheless, we collected extensive retrospective data on individuals' pre-service background just before recruitment and control for these in our baseline regressions. Testing for selection issues directly, we show that no background characteristic is a robust predictor of recruitment into the penalized group or into UNITA in general. Second, the penalized group may simultaneously have been more likely to be exposed to other wartime experiences that correlate with civic engagement. Specifically, a recent set of studies links the experience of war violence to pro-sociality (Bauer et al., 2016). Securing territory just before, or in parallel to, involvement in wartime governance, the penalized group may have been more likely to fight at the front line more often and experience more battle-related violence than others. This scenario suggests that UNITA and MPLA soldiers from the same birth cohorts should have been at the same risk of exposure to this form violence, as it occurred between the two

groups, who used similar fighting technology and tactics. However, the logic of the instrument is built upon the *asymmetric* effect across army assignment within birth cohorts and should therefore isolate the effect of exposure to governance. To address this empirically, we collected detailed data on wartime experiences other than governance, including exposure to different forms of violence. We find that soldiers exposed to more wartime governance received – on average – more violence, when we include and average over all forms of violence received in an index. Yet, how the differential impact of the army assignment varied over birth cohorts is very different for (realized) exposure to any forms of violence than for (realized) exposure to wartime governance. Specifically, the instrumental variation is *not* correlated with any form of violence received, including battle-related violence. These results provide extensive evidence that the instrument is not correlated with pre-service background characteristics and experiences of violence.

Our main result is that individual exposure to wartime governance significantly increases a former soldiers' engagement in both planning and delivery processes of local public goods production in the long run. We show that this result is robust to the inclusion of local fixed effects, pre-service characteristics, (potentially endogenous) service and post-service control variables, non-linear model specifications, alternative specifications of the intercorrelations in the error term, alternative measures of exposure to wartime governance, and that it is not driven by a single wartime governance component alone. Based on instrumental variable (IV) estimates we further argue that the positive impact of exposure is not due to correlations with unobserved, confounding pre-service, service and post-service variables, or systematic measurement error.

Additional results reveal that the underlying causal mechanisms differ markedly across processes of planning and delivering local public goods. We find that the positive impact on community-meeting attendance is driven by a shift in individual political attitudes and preferences, whilst increased individual participation in local security groups is fostered by higher levels of interactions with other members of the armed group (learning mechanism). We find equally positive effects of individual exposure to wartime governance on other forms of local social participation and political attitudes, but we do not find evidence for increased mobilization in wider political processes beyond the local level, such as voting in presidential elections or participating in regional protests. We neither find strong effects on social cooperation within the family. We interpret these findings as evidence that exposure to wartime governance may stimulate a lasting interest in and engagement with local politics, governance and collective action.

This study is – to the best of our knowledge – among the first to discuss and quantify legacies of the local institutional changes in civil conflict, but it is related to several strands of literature. The paper complements an interdisciplinary literature studying the interrelationships between

war, institutions and development. Historical and macroeconomic approaches have focused on mutual reinforcement mechanisms at the *national* level (e.g. Tilly, 1975; Olson, 1993; Collier, 2003; Besley and Persson, 2008, 2009, 2010). A recent body of microeconomic studies has studied the consequences of exposure to *combat* and *violence* (e.g. Voors et al., 2012; Jha and Wilkinson, 2012; Rohner, Thoenig, and Zilibotti, 2013b; Callen et al., 2014; Grossman, Manekin, and Miodownik, 2015; Bauer et al., 2016; Couttenier et al., 2017), but has not been able to address the effects of institutional processes and changes that take place in conflict zones (Blattman and Miguel, 2010; Justino, Brück, and Verwimp, 2013; Balcells and Justino, 2014). An emerging literature in political science on 'rebel governance' has started to produce descriptive evidence on the forms of local governance, economies, and institutions that emerge in armed conflict (Mampilly, 2011; Arjona, Kasfir, and Mampilly, 2015),[8] but knowledge about the lasting impacts of these forms of governance is hitherto very limited. All of these literatures suffer from a dearth of high-quality micro-level data from conflict zones and have struggled to identify and disentangle the mechanisms interrelating war, institutions and development. We contribute to this line of research by producing an original micro-data set informed by ethnographic and psychological research, using historical insights to develop a new instrumental variable strategy and theorizing the causal mechanisms that link wartime governance to post-war socio-political behavior.

The paper also advances a large literature on the processes that shape behavior and development. This literature has focused on the historical causes and consequences of behavioral and cultural traits (Putnam, 1993; Guiso, Sapienza, and Zingales, 2011; Alesina, Giuliano, and Nunn, 2013), and those of formal and informal institutions (North, 1990; Acemoglu, Johnson, and Robinson, 2001; Dell, 2010). What is much less understood is how institutions and culture interact (see Alesina and Giuliano, 2015), which is analytically complicated by the fact that traits among a certain population are typically (and often necessarily) elicited in the population's institutional environment, which may have moved *simultaneously* with or been shaped by behavioral traits. Our strategy to untangle these effects is methodologically related to the recent paper by Lowes et al. (2017). Lowes et al. (2017) examine the impacts of exposure to state formation in the Kuba Kingdom by comparing Kuba vs. non-Kuba descendants that reside in the same location *outside* the historical Kuba Kingdom. In our case, three out of four soldiers returned to their community of origin after military service 'somewhere else'. Using location fixed effects, our study thus allows to compare the behavior of many individuals that reside in and are from the same location, but isolate the effects of variation in institutional exposure on their behavior.[9]

[8] A related literature studies the mechanics and dynamics of networks and fighting between armed actors (e.g. Christia, 2012; Bueno De Mesquita, 2013; König et al., 2017).

[9] A scenario that would invalidate this logic is that soldiers served (mostly) in the location where they reside today. As military service involved enormous amounts of mobility, we argue that this very unlikely.

Our findings suggest that local collective action in post-conflict societies is endogenous to wartime institutions, policies and experiences, which has important implications for policies aimed at building institutional capacity and social cohesion in these contexts. First, we contribute to understanding why individuals and local populations may respond differently to behavioral and governance interventions, and to producing assistance schemes that are more tailored to needs based on past experiences.[10] Second, we document a wartime source of pro-social behavior among veterans. This insight challenges some (negative) premises of current reintegration programs and should be leveraged by innovative new interventions.[11]

The paper proceeds as follows. Section 2 discusses the theoretical mechanisms underpinning the causal link from wartime governance to post-war contributions to collective good production (our main outcome). Section 3 describes the Angolan context and the natural experiment exploited for identification. Section 4 discusses the survey data, as well as the main specifications and assumptions of the econometric analysis. Section 5 presents the main results. Section 6 discusses empirical evidence on the underlying mechanisms. Section 7 reports findings on related social and political outcomes. Section 8 concludes and discusses policy implications.

2 Theoretical framework

Mechanism 1: Economic interactions. Wartime governance can be understood as part of the economic interactions between combatants and civilians. Economic cooperation is crucial in many contexts, as armed groups often rely on economic support from civilians to feed and sustain the group.[12] In the case of Angola, economic interactions were likely critically

This intuition is confirmed by retrospective survey-data on the respondent's location at 15 pre-specified, known dates of the war, which suggests that no soldier served in his home community, and that soldiers spent overall extremely little time 'at home' during military service (not reported).

[10] In particular, participatory development projects have become the centerpiece of recent international assistance programming, and often seek to order to build institutional capacity and development 'from below', by stimulating civic participation and inclusion. Over the past decade, the World Bank has injected more than $85 billion of aid into such projects, but interventions have struggled to enhance participation, and individuals' and communities' responses are far from uniform (Mansuri and Rao, 2012; Casey, Glennerster, and Miguel, 2012; Fearon, Humphreys, and Weinstein, 2015; Berman, Downey, and Felter, 2016). Understanding variation in intrinsic factors conducive to civic participation across individuals is therefore crucial for understanding the impacts of these interventions and improving their effectiveness.

[11] In post-conflict contexts, veterans' post-war trajectories are key elements in the transformation of a conflict-affected to a peaceful state, where former fighters are often considered a primary threat to political stability, social cohesion and economic development (e.g. Blattman, Jamison, and Sheridan, 2017). They are therefore a focal point of large-scale development assistance, as in demobilization, disarmament and reintegration programs (DDR).

[12] From a game theoretical perspective, the essence of these economic interactions may in certain situations be modeled as a Prisoner's Dilemma. In the case of two players, both can choose between cooperation and defection. The payoff will be higher if both cooperate than if both defect. Defecting when the other player cooperates will lead to the maximal payoff, while cooperating when the other player defects will lead to the

important as food was often extremely scarce (UNICEF, 1998).[13]

The idea of the 'economic interactions' mechanism is that individual long-run behavior depends on how the other group in the repeated interactions (civilians) has behaved towards the individual (direct experience) or towards others in the individual's group (indirect experience). Reciprocal motives then underlie strategic actions as mimicking ('tit-for-tat') or punishing others, which is usually costly (Nowak, 2006; Dreber et al., 2008). We assume that wartime public good provision is a group policy, so that an individual soldier was usually not able to 'punish' or 'reward' civilians during these repeated interactions, based on his beliefs. Two scenarios may shape long-run behavior. First, the soldier may perceive that the armed group offered *more* to civilians than they received in return; for instance, via taxes or other forms of material support, such as food or shelter. In this case, the soldier's belief in the post-conflict period will be that civilians owe him effort, possibly 'for life', and he may thus be *less* likely to engage in the production of public goods. Second, the soldier may perceive that the armed group offered *less* to civilians than they received in return. Hence, the soldier's belief in the post-conflict period will be that he owes civilians effort. It is thus plausible that soldiers will feel that they owe civilians, fostering a positive impact on individual engagement in public good provision today.

Mechanism 2: Social attitudes. Wartime governance can also be modeled as a social interaction between combatants and civilians. A considerable body of research documents an 'in-group bias', i.e. that humans are often more likely to adopt attitudes that favor other members of their own social reference group (e.g. Goette, Huffman, and Meier, 2006). In-group bias has been identified as both a cause and consequence of war, with a focus on ethnic identification (Bowles, 2006, 2009; Choi and Bowles, 2007; Bauer et al., 2014a). In-groups groups defined by civilian and combatant status have been less explored, but such group divisions and social attitudes may carry over to the post-war period (Bauer, Fiala, and Levely, 2017).

Repeated, positive social interactions with civilians may *weaken* biased attitudes and increase a soldier's motivation to contribute to a public good that will benefit civilians in the post-war period. As in the post-war period basically all are civilians, such an outcome and disposition is effectively similar then to that of 'collectivism' as opposed to 'individualism' which, in cross-cultural psychology, is a central form of cultural variation (Alesina and Giuliano, 2015; Heine, 2015). Theories of collectivist cultures emphasize the embeddedness of

minimal payoff (e.g. Mas-Colell, Whinston, and Green, 1995). In the absence of normative mechanisms of cooperation, natural selection favors defection (Bowles and Gintis, 2011). However, evolutionary game theory has proposed that defection may not occur due to the existence of mechanisms such as direct and indirect reciprocity defined by repeated interactions (Axelrod and Hamilton, 1981).

[13] More than half of survey respondents report that they were starving at critical levels "often" or "very often", which emphasizes the vital importance of economic support by civilians during the war.

individuals (civilians) in a larger group and acting in the group's interest, in contrast to cultures of individualism (Gorodnichenko and Roland, 2011, 2015). These mechanisms suggest, that positive social interactions with and attitudes toward civilians during wartime may result in more participation by former soldiers in collective institutions that benefit civilian populations in the post-war period.

Mechanism 3: Learning. A related set of studies provides theory and evidence that systems of (within-group) cooperation may persist over long periods of time. Much of this literature is motivated by the influential model of 'democratic capital' by Persson and Tabellini (2009), which posits that a polity may accumulate 'experience with democracy' which in turn may set it on paths that make transitions out of democracy less likely.[14] Interpreted through the lens of social learning theory, individuals acquire behaviors through modeling and reinforcement contingencies in the context of social interactions (Bandura, 1973; Banerjee, 1992; Bikhchandani, Hirshleifer, and Welch, 1992). The dominant sociological view of the underlying process emphasizes both normative and non-normative learning mechanisms. 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). Irrespective of the nature of the learning process, this mechanism suggests, then, that 'local governance may beget local governance', leading learning individuals to participate in the organization of public goods today. In contrast to Mechanism 2, this mechanism is not a function of attitudes towards other groups (and its members), but rather emphasizes the importance of within-group learning.

Mechanism 4: Political preferences. An alternative and popular interpretation of the persistence of systems of government, and their broader effects, is that exposure to certain modes of governance changes individuals' deeper political attitudes and beliefs (e.g. Alesina and Fuchs-Schündeln, 2007; Fuchs-Schündeln and Schündeln, 2015). With respect to 'state-like' governance, Depetris-Chauvin (2015) provides suggestive evidence that individuals living in regions that were more exposed to indigenous structures that resemble basic state institutions articulate higher trust in local councilors and traditional leaders.[15] Gennaioli and Rainer (2006, 2007) show that forms and quality of local governments are associated with their history of state centralization. Hariri (2012) argues that early pre-colonial forms of statehood outside Europe spurred long-run persistence of traditional, authoritarian rule. This mechanism emphasizes that the delivery of wartime governance is part of a local political system. More exposure to

[14] Giuliano and Nunn (2013) present evidence for such an effect at the village level. They document the persistence of village-level traditions of democracy and argue that these may scale-up to the national level.

[15] Notably, Depetris-Chauvin (2015) finds no significant effect on trust in general and in *national* politicians.

such a regime may mold political attitudes and preferences in the long run and lead to more engagement in the collective organization of public goods.

3 The Angolan Civil War

3.1 Setting

Angola's recent history sadly offers the ideal setting for our study. The 27-year civil war was a long, intense and dynamic case of mass militarization and military competition between two high-capacity actors: the MPLA government and the UNITA rebels. Both sides had emerged as national movements opposing the Portuguese colonial rule in a war of independence between 1961 and 1974. From 1975 to 2002, the MPLA and UNITA fiercely competed in the Angolan Civil War, both supported by natural resource revenues and strong international allies.[16]

Wartime politics. While the war also presents a case of brutal and prolonged violence, which especially toward the end of the war included intense violence against civilians (Ziemke, 2008), both sides also engaged heavily and systematically with local populations under their control in benign ways. Territorial control was highly volatile, but the competitors remained the same two actors throughout the war, both with sufficient capacity to enforce compulsory military service for young men (see Stojetz (2016) and Spall (2015)). Both invested heavily in their state-building missions and UNITA built a 'state in the state' (Roque, 2015). The literature emphasizes that both actors used elaborate strategies to legitimize their missions, win 'hearts and minds', and establish local political systems that resembled state functions and institutions (Parsons, 2006; Pearce, 2011). Active soldiers were required to help their units establish local monopolies of violence, recruit other young men for (compulsory) military service and regulate social and economic civilian life, most importantly by the delivery of public goods and services – large-scale, collective and labor-intensive projects.[17] This configuration already suggests that the population of former soldiers (from either side) is likely to be very large and to contain substantial variation in individual exposure to wartime governance.

Huambo province. We focus the study on the Central Highlands and Huambo province ('Huambo' hereafter) for three main reasons.[18] First, this region was at the center

[16] The MPLA strongly relied on oil revenues and support by Cuba and the Eastern bloc, while UNITA drew its strength primarily from diamond trading and its United States and South African allies.

[17] Based on the data described in the next section, Table A1 presents aggregate data on our main measures of exposure to wartime governance and related forms of interactions with civilians. Each entry denotes the fraction of soldiers that were involved in a given activity. The results reveal that a substantial number of soldiers on both sides experienced these activities.

[18] Huambo province is roughly of the size of Switzerland (see map in Figure A1).

and the mostly contested region of the war. In Huambo City, Angola's second largest city, UNITA proclaimed their own government on the same day the MPLA declared the independence of Angola in Luanda, on 11 November 1975. Most parts of the vast Central Highlands, and especially in Huambo, changed hands many times. Therefore, many men from the same municipality, village and even family served on different sides, and state and rebel army recruits from this region were essentially identical in background. On the one hand, this allows to directly compare the members, practices and dynamics of the rival organizations. In combination with conscription by either side this also produced a natural experiment, which generated an exogenous source of variation in the likelihood of exposure to wartime governance (as discussed further below). Second, this region was *not* at the center of the colonial war from 1961 to 1974. Most anti-colonial activity and guerrilla fighting took place near the coast and international borders, and both movements initiated their large-scale activities and mobilization strategies in Huambo only when the civil war started. At the same time, the rushed exodus of the Portuguese administration and its Angolans employees led to a literal collapse of the national state. Thus, the operations we study started in an institutional vacuum and in the absence of previous engagement in the region (which might have affected the relations between combatants and civilians). Third, Huambo province is the most densely populated region in Angola, but ethnically homogeneous. While the Angola literature suggests that ethnicity was never at the root of the conflict (e.g. Pearce, 2012), this design also allows to rule out potential confounding individual factors related to ethnicity.

Appendix A provides additional information about the two actors and the civil war and discusses why local collective action is crucial for local development outcomes in post-war Angola.

3.2 Natural experiment in exposure to wartime governance

In this section we describe in three steps how the civil war produced an exogenous source of variation in the individual likelihood of being exposed to wartime governance. We first discuss how the civil war exogenously sorted individuals into different armies and made a man's date of birth a strong (exogenous) predictor of his date of entry into the army. Second, we explain how variation in military success at the armed group level converted the (exogenous) interaction between the armed group and date of birth into a relevant source of variation in the likelihood of exposure to wartime governance. Third, we discuss the main threats to the exclusion restriction and how we address them.

3.2.1 An exogenous source of variation

Variation 1: Army. The literature on war politics in the Angolan Central Highlands argues *against* strong 'selection' into either side based on conventional, individual factors associated with rebel recruitment or mobilization into armed groups (as e.g. in Weinstein, 2007; Berman and Laitin, 2008). The literature documents 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 also emphasize that both sides were able to exert strong control over territories and their populations.

Information from our survey (see Section 4) on recruitment date by army and region presented in Figure A2 is consistent with these findings.[19] For each region, we observe considerable variation over time in how likely, i.e. frequent, it was ex-post 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 A2 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 was recruited into and reject joint significance, across specifications with and without fixed effects of recruitment date and region (and classical standard errors).[20] In the absence of any fixed effects, the negative adjusted R^2 in column 1 demonstrates that the predictive power of the set of background variables is essentially null. Consistent with 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

[19] To 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 distributions of entry dates.

[20] It 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.

and the historical literature, we conclude that the armed group an individual was recruited into was plausibly determined exogenously.[21]

Variation 2: Date of birth. What determined the *date* of military entry? In state-controlled regions, military service was compulsory by law for men in their late teenage years. UNITA, which effectively built a 'state in the state', also enforced mass enlistment. We thus expect the age distribution of UNITA soldiers at entry to be consistent with conscription, and thus similar to that of MPLA soldiers.[22] In particular, for soldiers of either side, date of entry into the armed group and date of birth should be highly correlated. Figure A3 shows the distribution of age at entry and reveals 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 when an individual entered an army, and b) we should see a similar pattern in mean exposure to wartime governance 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. To establish causality, we exploit natural variation in the (ex-ante) likelihood of individual exposure to wartime governance, based on the *interaction* of date of birth and the army he joined. Dates of birth will be grouped into four specific bins, as discussed in the next section. As both date of birth and army are plausibly determined exogenously, interactions between the two are also plausibly exogenous. It is worth noting that this holds even if doubts remained whether or not the armed group assignment was determined exogenously.[23] Technically, the strategy follows the same logic as a difference-in-differences estimator: we hypothesize that the expected difference in exposure to wartime governance due to army assignment varies with (grouped) dates of birth.[24] Econometrically, we exclude the interaction of the army and (grouped) birth-cohort indicators in the 'second-stage' of an IV estimation while including the main

[21] We acknowledge that concerns may remain about the exogeneity of the armed group indicator and argue further below that the actual instrument is also valid if exogeneity does not hold.

[22] These arguments are explored in detail in a companion paper (Stojetz, 2016).

[23] A few recent papers have logically similar arguments for using the 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.

[24] The 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).

terms. In the remainder of this section we motivate and introduce the grouping of birth cohorts and discuss threats to the excludability of the interaction.

3.2.2 A relevant source of variation

Variation in wartime governance at the group level. Descriptive evidence from conflict zones shows that armed actors across the world provide goods and services to local populations. This includes armed actors as diverse in nature as the successful Eritrean People's Liberation Front insurgency, the transnational Islamic State, or bandits in stateless areas in Eastern Congo (Mampilly, 2011). Dominant explanations in the literature include that many armed groups do so to win the 'hearts and minds' and political support of local populations (Berman, Shapiro, and Felter, 2011; Arjona, 2014), support recruitment (Weinstein, 2007; Berman and Laitin, 2008), and to increase economic activity and the ability to collect taxes (Sánchez de la Sierra, 2015).[25]

In theory, these mechanisms may hold for groups acting in an institutional vacuum. Yet, as in the case of Angola, conflict actors often compete with rival actors for territorial control and regional or national governance, which may increase incentives to provide public goods even further. The work by Besley and Persson (2011) reiterates previous arguments by historians that external war, or the threat of external war, generally creates a demand for spending in common interest internally, which facilitates investments in state building. While less studied, a similar incentive logic should apply to competing actors in internal conflicts.

This raises the question of when these competing actors will be more likely to provide public goods. Conflict scholars emphasize the role of territorial control. Controlling territory and engaging with local populations are central objectives of warfare and counterinsurgency (Toft, 2014; Kalyvas, 2006; Arjona, 2014), and armed actors that control territory are much more likely to provide public goods than those without territorial control.[26] We postulate therefore that more *new* territory means more demand for public good provision, especially for armed groups with particular interests in securing legitimacy and voluntary support.[27] As a result, we expect that an armed group is more likely to invest and engage more in public good delivery in times of more territorial gains. If the armed group wants to meet the higher demand for governance from territorial gains, its soldiers need to supply more public goods.

[25] Such public investments are just one example of strategies employed by armed groups to legitimize claims and achieve political and economic goals, and some groups may not provide any public goods at all (see e.g. Mampilly, 2011; Stewart, 2016). Yet, providing public goods and services is one particularly dominant strategy, the essence of 'good governance' and an effective way of securing voluntary compliance (Levi, 1989; Berman, Shapiro, and Felter, 2011).

[26] Stewart (2016) argues that secessionist insurgencies that control territory are 46% more likely to provide public goods to civilians than those that do not.

[27] We look at the absolute extent of wins. The argument may also hold for 'net gains'. A caveat here is that group may also lose territory, in which case the net gain in territory may actually be negative.

Based on this logic, we hypothesize that a soldier who serves in a period when his armed group gains (more) new territory will be more likely to be exposed to (more) wartime governance than an identical soldier who serves at a different time.

To explore temporal variation in territorial expansions by the two Angolan actors we use the conflict event dataset by Ziemke (2008), which includes information on major territorial gains, coded by date and actor. In the plots displayed in Figure 1a we observe three types of variation: (i) across actors, (ii) across time (within actors) and (iii) in the difference between actors over time. The data confirm historical evidence of two distinct periods when UNITA managed to capture large parts of territory (Pearce, 2011). The first period includes the first years of the civil war until the early 1980s (time period 'I'), when the MPLA was at the brink of losing the war early (Maier, 1997). The second period is a rapid and massive growth of UNITA-held territory, when UNITA surprised the MPLA in a large-scale operation shortly after the elections in 1992 (time period 'III'). This period extends until the end of 1994, when a ceasefire was agreed in the *Lusaka Protocol*. A third strong UNITA campaign, following the end of the formal ceasefire in 1998, was quickly and strongly overturned by the MPLA, leading to the MPLA's final victory in 2002 (time period 'IV'). During period IV and in the mid 1980s (time period 'II'), when the MPLA was supported by large numbers of Cuban troops, UNITA was not able to match the territorial gains by the MPLA. We thus expect the amount of wartime governance provided by the two groups to have been roughly the same during periods I and III, due to UNITA's comparable strength in these periods, and the MPLA's supply to have succeeded UNITA's considerably during periods II and IV.

Variation in exposure to wartime governance at the individual level. If our hypothesis is correct, we do not expect much difference in exposure between UNITA and MPLA soldiers who served in periods I and III. For periods II and IV, we expect that an active UNITA soldier would – on average – be *less* exposed to wartime governance than an MPLA soldier. Figure 1b presents local polynomial regressions of the individual wartime governance index on the 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: for entry dates falling roughly into periods II and IV the index is consistently higher for the MPLA as compared to UNITA, while for periods I and III this is clearly not the case.

While the date of entry only makes it more likely that an individual served during a certain period, it might still be determined by confounding factors. We therefore consider the exogenous variation in date of birth, which strongly predicts date of entry as explained above, due to conscription. We categorize the date-of-birth distribution into four 'pooled birth cohort' indicators ('Bin 1'–'Bin 4'), directly corresponding to the four time periods defined above ('I'–'IV'). The three cut-off points separating Bins 1 to 4 are the three cut-off points

separating periods I to IV, shifted by exactly 20 years.[28]

Figure 1c displays polynomial smooths of the wartime governance index over date of birth, which reveal the expected result: the patterns are similar to those for date of entry in Figure 1b. Being born into Bins 2 or 4 involved (ex-post) a clear 'penalty' in realized wartime governance for UNITA soldiers. As before, we observe no or even a slightly reversed pattern for Bins 1 or 3. Figure A6 plots the probability density functions of the wartime governance index for each year of birth bin separately. While the MPLA distribution is relatively stable across bins, the probability mass for UNITA is noticeably shifted to the left in Bins 2 and 4, as compared to the distribution in Bins 1 and 3. These shifts demonstrate that UNITA's relative institutional disadvantages in periods II and IV are (ex-post) indeed reflected in the realized exposure of Bin 2 and 4-individuals, which were more likely to have served in periods II and IV.

The patterns in these unconditional relationships suggest that the Angolan war created an informative and exogenous source of variation in exposure to wartime governance based on the interaction between the army the individual joined and his year of birth. To be precise, we expect that soldiers who joined UNITA and were born into 'pooled birth cohorts' 2 and 4 were significantly less likely to be exposed to wartime governance, compared to all other soldiers.

3.2.3 Exclusion restriction

The identifying assumption is that the combination of being recruited into UNITA plus being born into Bins 2 or 4 is uncorrelated with confounding factors. There are two main threats over the excludability of the instrument. First, individuals who were born into Bins 2 or 4 and recruited by UNITA may have differed systematically in background traits that correlate with the proclivity for civic engagement. Second, this sub-group may have simultaneously been more likely to be exposed to other wartime experiences that correlate with civic engagement, notably inter-group violence when fighting over territorial control. As laid out in the introduction, neither scenario is likely, due to the conscriptive nature of (most) recruitment into either group and the fact that inter-group fighting violence should affect UNITA and MPLA soldiers of the same cohort equally. Nonetheless, we collected detailed self-reported data on pre-service background just before recruitment, which we also control for in our baseline regressions, and on wartime experiences other than governance – especially different forms of violence – to test these concerns empirically. A third concern may be that the identifying variation simultaneously selected individuals into confounding violent interactions with civilians. While possible, these interactions are (almost) exclusively forms of one-sided violence and mean that armed group members perpetrate (rather than receive) acts of violence. However, the existing literature emphasizes that *receiving* violence affects social behavior, as opposed to perpetrating violence (Bauer et al., 2016). A priori, it is therefore also unlikely that violent interactions

[28] As noted above, the 20 year-shift is motivated by the (rounded) overall mean of age at entry.

with civilians invalidate the IV strategy even if one-sided violence was correlated with the instrumental variation. Nonetheless, we also collected data on these experiences to test this issue empirically.[29]

4 Empirical framework

4.1 Research design and data

The population of this study are all living males who were ever part of an armed group during the Angolan war and reside in Huambo province. We use primary survey data from a sample of 759 Angolan war veterans from 34 different localities, which we collected in the Study of Angolan Ex-Combatants (POEMA). 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 were used to assess the validity of 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.

The main outcome variables of interest capture individual engagement in the collective production of local public goods twelve years after the end of the war. We measure involvement in the planning stage by whether the individual participates in community meetings, and use engagement in local self-security groups as a measure of participation in the delivery of a public good (security). The key treatment variable is a soldier's recalled involvement in local governance practices with his armed group(s) during the war. We build an index of wartime governance exposure as the simple average over seven items: provision of services (such as providing access to education), building physical infrastructure (such as schools), provision of arms, help with conflict resolution between villagers (e.g. over land), provision of protection and security, requests by villagers for help with conflict resolution between villagers, and requests by villagers to protect the village.[30] In Appendix B we present more detailed information on the sampling strategy, interviews, motivations behind and the nature of our key measures; and discuss summary statistics (Table A3), alternatively constructed indices of exposure,[31] and

[29] We discuss the empirical evidence related to the exclusion restriction in Section 5.3.

[30] This list was inspired by leading accounts of local governance by armed groups (Mampilly, 2011; Arjona, Kasfir, and Mampilly, 2015).

[31] As our index is based on frequency measures of exposure, we (carefully) condition on length of service in the standard specifications, which may itself be endogenous. As we will show, omitting the length of service variable leaves the main effect unchanged, both in terms of magnitude and statistical significance. Alternative indices include different weighting schemes based on principal component analysis and the method suggested

potential issues of recall bias.

4.2 Econometric specifications

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

$$Participation_i = \beta Experience_i + \gamma' X_i + \varepsilon_i \quad (1)$$

Here, *Participation* is the indicator of individual engagement in collective public good production today (in the main analysis); *Experience* denotes experience with wartime governance; X is a flexible vector of additional control variables; ε is the error term.

The identifying assumption for a causal interpretation of OLS estimates of β is that experience with wartime governance and the error term are not correlated, conditional on the control variables we include. The set of control variables we include is flexible across specifications. First, all specifications include community fixed effects to purge the results from systematic variation across localities. 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.[32] Third, we sequentially add potentially confounding contemporaneous (socioeconomic) variables that have been linked to civic engagement and might simultaneously co-vary with wartime governance experience.[33] Fourth, we carefully explore the effect of controlling for potential wartime confounders, such as experiences of violence, bearing in mind that these may not be determined exogenously themselves.[34]

Even after carefully controlling for these factors, we may measure exposure to wartime governance with systematic error or may not be able to control for all individual factors that are associated with participation in both wartime governance and post-war civic engagement. Either would result in spurious estimates, and we rely on IV estimates to mitigate these concerns.

IV estimation. The instrumental-variables (IV) strategy is based on the same linear models as in Equation 1 (the 'second stage'), where the effect of interest β is estimated by IV/2SLS regression. To test the relevance of the instrument(s), we use OLS to estimate δ, as

by Anderson (2008) as well as directly adjusting the standard index for length of service.

[32] Pre-treatment variables were collected based on recall questions included in the survey. The individual reference point was the time just before joining an armed group (for the first time), which respondents remembered very well.

[33] Potential confounders include assets, education, wealth or place of birth (see e.g. Blattman, 2009).

[34] We pay most attention to victimization, i.e. 'receiving violence', which has been linked to post-war social behavior and attitudes (e.g. Bauer et al., 2016; Rohner, Thoenig, and Zilibotti, 2013b; Couttenier et al., 2017). Other potential confounders include perpetration of violence (e.g. Humphreys and Weinstein, 2006), army characteristics (e.g. Akerlof and Kranton, 2000; Chen and Li, 2009), and length of service in an armed group (e.g. Gilligan and Samii, 2015; Bauer, Fiala, and Levely, 2017).

specified in the following 'first-stage' equation:

$$Experience_i = \delta(UNITA_i \times BIN_i) + \lambda' X_i + \nu_i \quad (2)$$

Here, *Experience* refers to experience with wartime governance; X is the full vector of control variables; ν is the error term. UNITA is the armed group indicator and BIN is a flexible categorical variable built from the four Bin indicators, defined by the date-of-birth cut-offs defined in the previous section. 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 *UNITA* indicator with the Bin 24 indicator, which equals one if the respondent belongs to Bin 2 or 4.

5 Results

5.1 OLS results

The OLS results suggest that individual exposure to wartime governance is positively associated with participation in both the collective organization and delivery of public goods twelve years after the end of the war. Table 1 shows strongly significant estimates from a parsimonious model, with the full wartime governance index as the treatment, no additional control variables and Huber-White standard errors (columns 1 and 4). While index measures are widely used in econometric analysis (e.g. Acemoglu, Johnson, and Robinson, 2001; Bellows and Miguel, 2009), they are built upon scales that may not reflect constant effects.[35] Inspired by psychological analyses, which often define "high-intensity" as above the 75th percentile, we divide the sample at the 25th, 50th (median) and 75th percentiles of the distribution of the wartime governance score. Columns 2 and 5 reveal that for both outcomes the significant effect is driven by the top quartile.[36] The magnitude of its impact remains almost the same if the top quartile is compared to the other quartiles combined (columns 3 and 6). To capture the relevant jump in response, ease interpretation and avoid imposing linearity, we collapse the wartime governance index into a coarse binary indicator, defined by the 75th percentile. We interpret exposure above the 75th percentile as 'high exposure to wartime governance' and use this coarsened treatment variable in the main analysis.[37]

[35] For instance, moving from 1 to 2 on an index will likely not have the same effect as moving from 3 to 4.

[36] See Figure A7 for a graphic illustration of the equivalent regressions using octiles instead.

[37] While we are confident to capture a treatment effect concentrated around the 75th percentile, our estimates may be susceptible to coarsening bias (Marshall, 2016). We address this in the robustness checks and report key results also for the full index measure.

Robustness. In Table 2 and 3 we 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.[38] The sequential inclusion of other (potentially endogenous) war experiences does not change the estimates noticeably. For both outcome measures, the only variable that appears to slightly affect the magnitude of the coefficient is a measure of how much violence a soldier 'received' (victimization).[39]

In Appendix C.1, 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). We also report that the positive association with post-war participation holds for the continuous wartime governance index, and demonstrate its robustness to three alternatively constructed indices (Table A6). Looking at the components of exposure separately, we find markedly moderate intercorrelations (Table A7), lending further support for our preferred method of index construction, and substantial correlations with post-war participation (Table A8), suggesting that the index effect is not driven by a single component alone. 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 governance 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 governance may be a) a function of individual background characteristics and b) correlated with other wartime experiences, especially exposure to battle-related violence. If such background characteristics or simultaneous experiences have an impact on the outcome variables, simple OLS estimates that are not corrected for these confounding factors will be biased.

Table A9 compares individual background and military traits of soldiers highly exposed

[38] For 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.1. 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 available upon request).

[39] If the victimization variable is correlated with the error term, and is correlated with our explanatory variable of interest, this may bias the estimate of interest. We consider and address these concerns further below.

to wartime governance to the traits of those less exposed, based on raw survey data collected for this purpose. The top panel suggests that the two sub-samples do not differ much in terms of background characteristics that could have determined selection in more involvement with delivering wartime governance. The only statistically significant difference is that highly exposed veterans had slightly better schooling. The middle panel presents summary statistics on the armed group and the pooled birth cohort indicators used in the IV analysis and shows that veterans belonging to the exposed sub-sample are not distributed differently across armed groups and Bins. By contrast, the bottom panel reveals systematic differences in military traits. Exposed soldiers trained slightly longer, were marginally less likely to be an infant (the lowest role in hierarchy), more likely to serve in areas where combat took place and in slightly more battalions.

To explore correlations with the experience-based control variables included in Tables 2 and 3 (violence received, witnessed and perpetrated, and length of service), Figure A8 presents residual-residual plots, where variation from the full set of other covariates is partialled out. The plots confirm that victimization (violence received) is correlated with experience with wartime governance, while all others are not. As a growing body of literature also links victimization to pro-sociality (Bauer et al., 2016), we pay particular attention to this variable and address potential issues econometrically in two ways. First, we exclude 'violence received' from the econometric model, shifting it into the error term. If the exclusion restriction holds, the instrumental variable strategy then recovers a causal estimate. Second, as a robustness check, we estimate a model that includes 'violence received' and treats it as endogenous, i.e. we instrument for it.[40]

5.2 Informativeness of the instrument

Table 4 presents first-stage and reduced-form results to assess the informativeness of the instrumental variable. As expected, the combination of joining UNITA and being born into Bins 2 or 4 predicts significantly less experience with wartime governance (column 1). Following Angrist and Pischke (2008), we choose our single best instrument for our main specifications. We collapse the Bin 2- and Bin 4-dummy variables into the binary indicator variable Bin 24, which compares being born in pooled cohorts 2 or 4 to all others, and interact Bin 24 with the UNITA dummy variable to form the binary instrumental variable for the main analysis. Columns 2 and 3 show that the binary instrument is highly informative, with and without the inclusion of post-treatment controls. The F-statistics of about 72 and 106 suggest that our instrument is not 'weak' and mitigate concerns of associated bias (Staiger and Stock, 1997). The reduced form regressions in columns 4 to 6 demonstrate a

[40]The results are discussed in Section 5.4 and Appendix C.2.

robust negative association of the IV with either outcome variable. The coefficients are stable across specifications, and – despite the rich specifications and restrictive assumptions – are statistically significant at conventional levels.

Robustness. To test the hypothesized origin of the informative variation directly and mitigate concerns that it is (somehow) driven by confounded comparisons across armies, Table A10 shows 'first-stage and reduced-form' results for the effect of birth into Bins 2 and 4 *within* the UNITA sub-sample. As argued theoretically in Section 3 and suggested graphically by Figure A6, Bins 2 and 4 both induce significant reductions in the likelihood of being exposed to wartime governance (columns 1 and 2) and of participating in post-war public good production (columns 3 to 6) among those who were recruited into UNITA.

5.3 Validity of the instrument

As a first test to assess the validity of the instrument, Table A11 compares raw means in individual background and military service characteristics between the two sub-samples defined by the value of the binary instrument ('balance table'). Similar to the unconditional comparisons based on actual exposure to wartime governance in Table A9, backgrounds do not differ across the two sub-samples, but now individual self-reported military traits are not significantly different from each other either.[41]

Sorting based on confounding background characteristics. Table A12 shows that no pre-service variable is systematically associated with the value of the IV in multiple regression models. All presented coefficients are estimated from linear probability models and not significant across specifications. Column 1 presents baseline results, while the model in column 2 includes recruitment region fixed effects. Dividing the sample into individuals born into Bins 2 or 4 and those that were not, we find that the absence of a systematic correlation between background characteristics and joining UNITA holds for both sub-groups separately (columns 3-6). 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 pre-service covariates. The BMA results confirm that *no* background characteristic predicts i) the value of the IV (Figure A9), ii) UNITA recruitment among those born into Bins 2 or 4 (Figure A10a) and among others (Figure A10b) or iii) birth into Bins 2 and 4 among UNITA recruits (Figure A11a) and among MPLA recruits (Figure A11b).

[41] Given the number of characteristics, we would expect to see significant differences due to chance. It is hence not surprising that the difference in formal training before treatment is significant. Yet, the difference is only marginally significant in statistical and economic terms.

The instrument is based on a higher *likelihood* of having served in periods II and IV (via birth into Bins 2 and 4) rather than having actually served. Nonetheless, we repeat the BMA analyses of Figure A10 and Figure A11 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 A12 and Figure A13 show that there were no differences in background across army and dates of entry, further corroborating the argument that there was generally 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. Figure 2 displays running means of exposure to six dominant forms of inter-group and one-sided violence over year of birth. It is apparent that the temporal patterns within and between groups are very different from that of exposure to wartime governance. For instance, the strong 'penalty' in exposure from having joined UNITA out of Bin 2 is not present in any of the variables.

Table A13 contrasts mean experiences of the six specific experiences and four index-based measures of battle-field and one-sided violence between the sub-samples defined by the value of the IV. The results corroborate that exposure to no form of battle-related and one-sided violence as well as no general index of exposure to violence is correlated with the IV. The results reported in the top panel reveal that the sign of the difference changes across different forms of exposure for both battle-related and one-sided violence, measured as the total number of situations experienced. No difference is statistically significant, even though this might be expected due to chance. The differences shown in the bottom panel show that no frequency-based index-measure of exposure to violence is economically or statistically significant either. While all indices are scaled equally, the observed differences are markedly small compared to wartime governance. The findings specifically document that the full index measure of (any) violence received, which was correlated with the exposure to wartime governance, is not correlated with the instrument. Thus, the instrumental variation allows to isolate the causal effect of exposure to wartime governance in two-stage estimates.

5.4 IV results

Table 5 displays IV and corresponding OLS estimates of the impact of exposure to wartime governance on post-war community meeting attendance and security provision. The IV estimates confirm the positive and significant effect on public good production 12 years after the end of the war. The IV estimates of about .32 (community meeting attendance) and .22 (local security provision) both exceed the OLS estimates and are robust to the inclusion of post-war control variables.

Robustness. As shown in Table A14 these results obtain when we use the continuous measure of exposure. While the single instrument is preferable in terms of approximate bias, the coefficients for the binary measure of exposure are also stable across two alternative, additive instrument specifications for both outcomes (Table A15). For models in which violence received is included and treated endogenously, the magnitude and statistical significance of the effect of interest does not change noticeably either (Table A16, A17). Appendix C.2 discusses these robustness tests in more detail. While only suggestive, these results add to recent evidence that casts some doubt on the conjecture of universally positive impacts of exposure to violence on pro-social behavior (Adhvaryu and Fenske, 2014; Gáfaro, Ibáñez, and Justino, 2014; Child and Nikolova, 2016).

5.5 Village level

An interesting and important question from a development point of view is whether the behavioral relationships we find scale up to more aggregate levels. Appendix C presents a simple analysis at the village level based on village-level means from individual responses (Figure A14). While not statistically significant, the results suggest a similarly positive relationship between exposure to wartime governance and civic participation at the village level (Figure A15), which further strengthens confidence in the existence and relevance of the underpinning micro-level effect.

6 Mechanisms

To explore the explanatory power of the theoretical mechanisms introduced in Section 2, we primarily rely on a set of auxiliary data on additional wartime interactions with civilians we collected for this purpose.[42] Results are reported in Table 6.

Mechanism 1: Economic interactions. To assess the relevance of the economic interactions mechanism, we analyze whether the magnitude of the effect documented in the sections above depends on the level of productive inputs to the group. This supply-side impact is measured by taxes collected from and food and services (voluntarily) delivered by civilians. Armed groups often provide public goods and services in exchange for taxes (Mampilly, 2011; Sánchez de la Sierra, 2015), while food and manpower/labor are the most fundamental resources to run and sustain an armed group. It thus likely that soldiers

[42] Similar to our treatment measures, these variables were elicited via a five-point frequency scale, ranging from "never" (=0) to "extremely often" (=4). We also use additional military and post-service outcomes, as will be explained below. For comparability of effects, all auxiliary variables are standardized to zero mean and unit standard deviation, unless noted otherwise.

genuinely appreciated the voluntary supply of productive inputs, as opposed to having to coerce them from civilians.[43] Yet, we find that the impact of exposure to wartime governance is *not* significantly different among those who collected taxes or were offered food more regularly (Panel A). The interaction coefficients are moderate in magnitude and statistically insignificant, and do not support the economic interactions mechanism.

Mechanism 2: Social attitudes. To assess the explanatory power of a change in social attitudes towards civilians, we first study the impact of having attended social events together, such as weddings or sports/games – activities with no or little economic and political meaning, which are however quite common during the war. We further analyze a subjective measure of civilian 'compliance', assessing to what extent civilians were not very cooperative, did not follow rules well or even resisted. The interactive effects are weak in general, and that of compliance is slightly negative and marginally significant for local security provision (Panel B). Based on our theory, we would expect a positive rather than a negative impact of higher perceived compliance. In combination with the moderate magnitude of all coefficients, we interpret these weak findings as evidence against a mechanism rooted in a reduction of social bias.

Mechanism 3: Learning. The third theoretical mechanism is based on social learning and emphasizes the collective and interactive aspect of wartime governance within the group. In the behavioral/interactional version of the mechanism, individuals may simply 'imitate' the behavior of their peers, while in the normative form of the mechanism, group- or unit-specific norms may exist that promote behavior related to civilians and are reinforced by the interaction of its members.[44] If such an effect exists, we would expect it to be stronger among those who a) operated in combat areas most, as these interactions are likely to be particularly important and intense when soldiers' lives are on the line and/or b) still interact a lot with former group comrades in the post-war period.[45] The results suggest that – as predicted by theory – the impact on local security provision is substantially stronger among those that still

[43]Food in particular was often extremely scarce in the Angolan conflict (UNICEF, 1998). In our survey, 53% of respondents report that they were "often" or "very often" "that hungry or thirsty that you ate or drank things you would never have imagined to ever eat or drink." In addition, 39% recall at least one situation where they were "that hungry or thirsty that you thought you might die within the next hour."

[44]This is similar in nature to a normative logic in collective violence, which many fear may spill over into the post-war period and create cycles of violence (Littman and Paluck, 2015). Further below we also discuss a direct measure of the extent of social norms set by the group.

[45]The question used to construct a binary combat unit dummy was: "Did your unit(s) usually operate (more) in combat areas or in non-combat areas?". Factional ties are also commonly assumed to proxy for strength of previous norms in reintegration programming. In Sierra Leone – a roughly comparable context – Humphreys and Weinstein (2006) show that ties are not correlated with economic reintegration which strengthens the proxy assumption. The exact question to assess tie strength was: "How often do you usually meet or spend time with people you met in your faction during the war?" (five-point scale).

have strong ties to former comrades today and also among combat area unit veterans (Panel C). Interestingly, we find negligible and not statistically significant heterogeneous impacts on community meeting attendance.

Mechanism 4: Political preferences. The final theoretical mechanism is based on a shift in political preferences and emphasizes the 'political system' aspect of local wartime governance. To test the relevance of this mechanism, we analyze whether the magnitude of the effect depends on the extent to which soldiers taught political ideas and gave strategic instructions to local civilians, such as how to act when a different group seizes control of their village. Panel D reveals that both of these activities increase the effect on participation in meetings significantly. In contrast, we find no economically or statistically significant interaction effects with respect to local security provision.

Summary. Taken together, these results suggest that the positive effects of wartime governance on planning and delivering local public goods mask important differences between the two outcomes when considering the underlying mechanisms. Participation in planning activities seems to be driven largely by social learning processes, whereas participation in the delivery of public goods is shaped by changes in political preferences.

Additional tests. Panel E of Table 6 reports additional heterogeneity results to test the validity of these two mechanisms further and examine factors that may exacerbate or weaken their relevance. First, we hypothesize that social learning processes and political preferences may depend on how armed groups ruled civilian populations (rows 1–3). In particular, we expect that promoting behavioral norms to regulate social behavior may be related to either mechanism.[46] The results confirm this and show strong and positive interaction effects on local security provision and community meeting attendance. To understand the role of qualitative aspects of the rule by the armed group, we analyzed the impact of relatively more ideological and violent types of rule.[47] The results suggest that neither a more ideological nor a more violent rule are significant sources of heterogeneity.

The strength of the long-term effect of exposure to wartime governance may also be related to the rank and number of battalions served by ex-soldiers (rows 4–5). Having reached a higher rank or having served in more battalions significantly increases the effect of interest on community meeting attendance, but also significantly reduces the effect on local security provision. An interpretation consistent with our two mechanisms is that higher ranks

[46] Question: "How often did your unit impose behavioral rules, e.g. about stealing, crime or violence?" (five-point scale).

[47] Questions: "How often did your unit impose rules that had ideological aspects, such as what food to eat and clothes to wear?" (five-point scale) and "How often did your unit use violence to impose or enforce rules?" (five-point scale).

as well as exposure to more (and likely different) types of battalions may propel a change in political preferences, for instance through more experience and insights into group strategies and policies. Yet at the same time, both may crowd out the social learning effect, e.g. through reduced time with other fellow soldiers in higher ranks and through volatility in battalion membership.

Next, we explore whether the impact via either mechanism is weaker for rebel veterans, stronger for longer tenures or dissipates over time (rows 6–8). Being a UNITA veteran may depress the long term impact of wartime governance, if being part of the winning side affects revealing preferences and social behaviors in the post-war period. The negative coefficients reported in row 6 are consistent with a negative impact of being a UNITA veteran, but are not statistically significant. Longer tenures may reinforce both changes in political preferences and mechanisms based on the interactions with other soldiers. We find that more time in the military is indeed associated with a (marginally) stronger effect on community meeting attendance, while the interaction coefficient is small and insignificant for local security provision. The impact is not significantly weaker among those whose army exit dates back longer, which suggests that the effects of wartime governance on both outcomes are persistent.

As a final test for the political preference mechanism we draw on insights from the nascent literature on the effects of exposure to social, political and economic institutions on the formation and shifts of preferences. A large literature argues that behavioral parameters are more (or most) likely to change between the ages of 18 and 25 (e.g. Giuliano and Spilimbergo, 2014). Table A18 presents results from sub-sample regressions for individuals that joined the army a) at age 17 or younger, b) between ages 18 and 24 and c) at age 26 or over.[48] As predicted by the literature, the effect of exposure to wartime governance for community attendance is highest in the 18-24 range (columns 1-3), while for contributions to local security this is not the case (columns 4-6).

7 Political mobilization and social cooperation

In this section, we explore further implications of wartime governance exposure on other forms of political mobilization and social cooperation, using additional data we collected data on a range of related outcomes. We collapse the outcome variables into four main additive and standardized indices (z-scores): political participation and attitudes, protesting, social group participation, and family cooperation.[49] Given the lack of reliable policy answers to the

[48] Using 17-24, 17-25 and 18-25 as middle categories gives nearly identical results.

[49] Some of these indices were adapted from the pioneering work of Blattman, Fiala, and Martinez (2014). We report results on the indices and the full set of components separately.

challenges of enhancing social cohesion and political stability in post-conflict settings, these implications are of particular interest to development policy.

Political mobilization. Panel A of Table 7 reveals that exposure to wartime governance is associated with a strong average increase in political mobilization (column 1). Yet, there is important variation in the effects across different aspects of mobilization. The positive overall effect is driven by a strong increase in political group participation (column 2) and the interest in politics (column 5).[50] We find no effects on electoral participation, proxied by registering and voting in national elections (but should notice that around 97% of respondents report to have registered and voted).[51] Looking at governance preferences, about 58% of respondents express that local governance systems set up by armed groups during the war are better than governance by 'normal' state authorities. While exposure to wartime governance significantly increases the probability of favoring such forms of (local) governance, we see no effect on voting against the ruling MPLA in the national elections. Second, we look at political protests as a form of political mobilization and collective action (Tilly and Tarrow, 2015). Before the 2012 elections, several marches against government politics took place in Huambo and Luanda.[52] As reported in Panel B of Table 7, we find a positive association between wartime governance exposure and the composite 'protesting index' (column 1), including both attitudes towards and actual participation in protests. The positive overall effect is driven by two variables: strong attitudes that protesters are justified to use violent tactics in political demonstrations (column 5), and that the police are justified to respond violently to protesters (column 6). We find, however, a weak but negative effect on staying at a (hypothetical) protest that turns violent (column 7).[53] One interpretation of these results is that the exposure to wartime governance shifts a soldier's attitudes toward the political or strategic use of violence, but he does not become more 'prone' to violence in general. In addition, we find that exposure to wartime governance is *not* associated with actual participation in protests, including the marches in Huambo preceding the elections, any other demonstrations in the past year, and hypothetical protests in the future (columns 2 to 4).

We conclude from these results that individual exposure to wartime governance stimulates an interest in and engagement with politics and political collective action at the local level,

[50] General interest in politics was measured by the answer on a five-point scale to the (deliberately unspecific) question: "How interested are you in politics?". 'Interest in politics' equals one for replies "Quite interested" and "Very interested," and zero else. Participation in local political groups, was measured by a binary indicator of group membership, which equals one for being a member, and zero if not.

[51] These numbers seem very high, yet are comparable to findings in other post-conflict settings (see e.g. Blattman, Fiala, and Martinez, 2014).

[52] Media reports describe former combatants as the main social group participating in these marches. The government responded to these demonstrations with repression, including crackdowns, incarcerations and violence (e.g. FT, 2012; ISS, 2012).

[53] Question: "Imagine that you are at a protest and it turns violent, would you stay or leave?".

but it does not appear to affect taking part in actual action at the regional or national levels.[54]

Social cooperation. Panel A of Table 8 reveals that exposure to wartime governance is – on average – also associated with a strong increase in social participation at the local level (column 1). The positive overall effect is driven by a strong increase in participation in various non-religious groups (columns 2–4). These positive effects stand in deep contrast to a strongly negative effect on participation in religious groups and communities (column 5). Religious communities are key networks and central elements of social capital in most developing countries, and a focus of the anthropological study supporting this project (Spall, 2015). The ethnographic results describe how Angolan soldiers rely on religious engagement and religiousness as *the* primary mechanism of signaling a certain lifestyle and living up to ideals of masculinity and senior male status. In a framework of pro-social behavior, these results clearly emphasize the salient role of social reputation, consistent with arguments in the theoretical literature in economics (e.g. Bénabou and Tirole, 2006). Their motivation to participate in religious organizations could hence be largely driven by reputational concerns. Conversely, Spall (2015) suggests that the 'social benefits' of participation in non-religious groups are distinctly lower, and therefore arguably less strategic in terms of social reputation. These insights lead us to conclude that the dynamics of wartime governance may reduce the incentives (or needs) of soldiers' incentives to rely on religious participation as a strategic means to improve social integration. Looking at the family level, Panel B in Table 8 documents weak and slightly *negative* effects of wartime governance exposure on cooperation within the family (column 1). The negative association appears to be driven by a reduction in caring about the spouse and children (column 4). The results also reveal a similarly negative but insignificant effect on harmony, and no effects on living together or being married (columns 2,3 and 5).[55] This suggests that the positive effects of wartime governance exposure on social participation do not include family relationships. One potential explanation is that being more active in the community may crowd out kin relationships, for instance via time constraints.

Taken together, the results on social participation strengthen our finding that individual exposure to wartime governance can foster social engagement, even beyond contributions to local public goods. Interestingly, these positive effects are not reflected in within-family cooperation – like the political effects, the social effects are confined to the community-level.

[54]This insight complements findings by Depetris-Chauvin (2015) who documents that a positive effect of a local history of indigenous state-like structures on trust in policy actors is confined to the local level. Individuals from regions with a long history of statehood trust local policymakers more, but there is no effect on trust in national policymakers.

[55]The caring and harmony variables are subjective four-point measures reported by the veteran's partner in a separate interview, and confined to respondents who have a partner.

8 Conclusion

In conflict zones around the world, institutions change and both state and non-state actors deliver governance at regional and local levels. This paper provides a first stepping stone towards understanding the micro-foundations of the legacies of local institutional change during violent conflict.

We operationalize 'wartime governance' as the local policy choices and practices by a ruling actor and focus on the provision of public goods and services to the governed populace, as it is at the heart of 'good' and 'state-like' governance. The key result is that soldiers' exposure to wartime governance by armed groups strongly affects their long-run social and political behavior. Based on ethnographic and historical insights, we use primary survey data and a natural experiment to show that involvement in wartime governance makes Angolan veterans more likely to participate in the collective production of local public goods more than a decade after the end of the Angolan war. We find that the underlying causal mechanisms differ markedly across outcomes of planning and delivering local public goods: a shift in political preferences increases community meeting attendance, while social learning fosters participation in local security provision. We find similar impacts of individual exposure to wartime governance on other forms of social participation and on political attitudes, but do not find any evidence for effects on mobilization in a wider political sense (in the form of voting in national elections or regional protesting), or on social cooperation within the family. We interpret these findings as evidence that exposure to wartime governance stimulates lasting interests and participation in *local* politics, governance, and collective action.

While the empirical analysis focuses on the case of Angola, we draw on the 'rebel governance' literature to argue that many aspects of our treatment – wartime governance – are qualitatively similar in many conflict zones across the world. We therefore expect our findings to have two externally valid and important policy implications for state-building and local development in post-conflict societies. First, policy designs often emphasize the importance of breaking ties between former fighters, assuming that clustering may fuel the risk of recurring violence. We document a source of pro-social behavior among veterans and find that remaining factional ties may in fact (also) reinforce the positive impact on contributing to local public goods and services. Second, community-driven development programs and initiatives to build national state capacity 'from the bottom' have struggled to deliver an impact, with results varying substantially across contexts. Our findings can help to understand why certain groups respond differently to interventions related to governance and participation. Conceptually, we show that some of the variation may originate from systematic differences in experiences related to forms of institutional change that took place during the conflict. Specifically, civil wars may

spur institution-building processes that foster lasting preferences for *local* and *collective* forms of governance, possibly at the expense of national governance. Hence, settings characterized by strong institutional change during conflict may take longer to build strong and cohesive national states in the post-conflict period.

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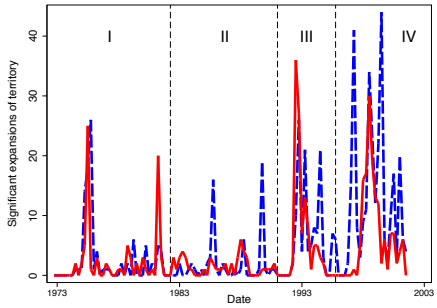
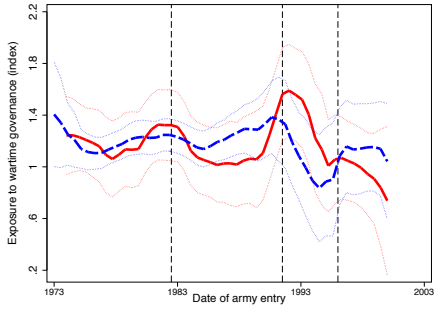
Figures

Figure 1: First-stage mechanism

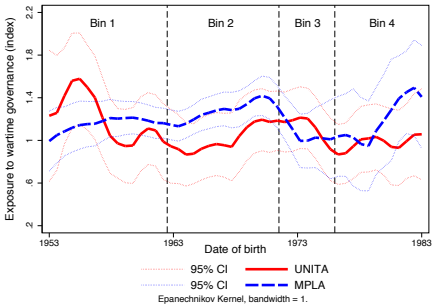
(a) Event data: Significant expansions of territory over time



(b) Survey data: Exposure to wartime governance and date of entry

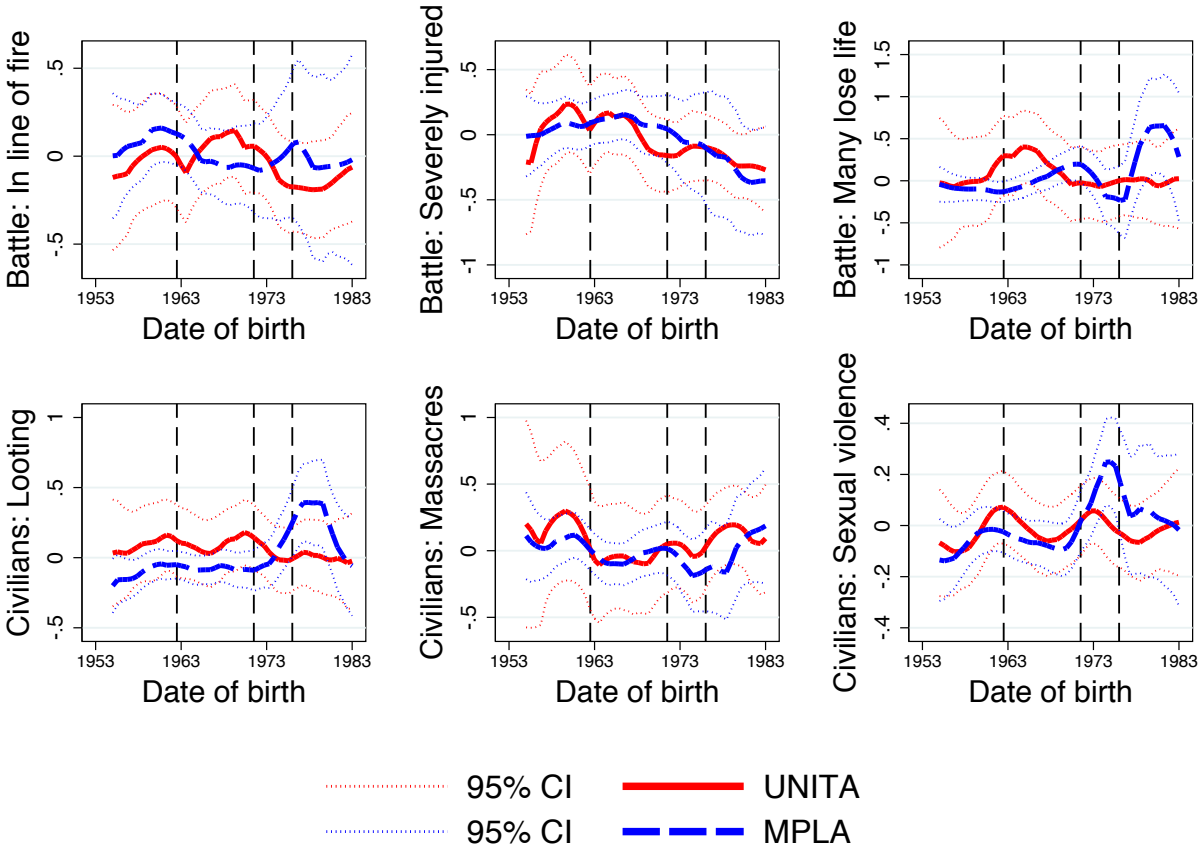


(c) Survey data: Exposure to wartime governance and date of birth



Notes: The figure motivates graphically that the identifying interaction between the armed group indicator and (groups of) date of birth is a relevant source of variation in the likelihood of exposure to wartime governance. Figure 1a plots conflict event data on major territorial gains by MPLA (blue) and UNITA (red) over time. Figure 1b plots our own survey data on exposure to wartime governance reported by MPLA soldiers (blue) and UNITA soldiers (red) over reported date of armed group entry (local polynomial regressions). Figure 1c plots our own survey data on exposure to wartime governance reported by MPLA soldiers (blue) and UNITA soldiers (red) over reported date of birth (local polynomial regressions). The scale of the x-axis in Figure 1c is shifted by exactly twenty years, the overall rounded mean of age at armed group entry.

Figure 2: Exclusion restriction: intergroup violence and violence against civilians



Notes: The figure illustrates graphically that the identifying interaction between the armed group indicator and (groups of) date of birth is *not* a relevant source of variation in the likelihood of exposure to potentially confounding wartime experiences of violence. The subfigures plot exposure to six different forms of violence (standardized to zero mean and unit standard deviation) reported by MPLA soldiers (blue) and UNITA soldiers (red) over reported date of birth (local polynomial regressions).

Tables

Table 1: Wartime governance and participation in public good production (OLS)

	Community Meetings			Local Security		
	(1)	(2)	(3)	(4)	(5)	(6)
Wartime governance (index)	0.080***			0.071***		
	(0.019)			(0.011)		
—WG index quartile 2		0.011			–0.015	
		(0.041)			(0.024)	
—WG index quartile 3		0.028			–0.023	
		(0.040)			(0.023)	
—WG index quartile 4 (top)		0.183***	0.171***		0.142***	0.153***
		(0.042)	(0.036)		(0.024)	(0.021)
Mean dep.var.	0.224	0.224	0.224	0.064	0.064	0.064
Observations	760	760	760	760	760	760

Notes: The table shows OLS estimates of Equation 1; no control variables included. *Wartime governance (index)*: full index built from frequency of exposure to seven dimensions of wartime governance. *WG index quartile x*: indicator of quartile x (1 = Yes, 0 = No), defined by the 25th, 50th, and 75th percentiles of the distribution of the wartime governance index. The reference category is quartile 1 (bottom). Significance levels:* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Classical standard errors in parentheses.

Table 2: Community meeting attendance: robustness to control variables

	Community meeting attendance								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
WG high	0.173***	0.173***	0.173***	0.155***	0.173***	0.168***	0.166***		0.135***
	(0.043)	(0.042)	(0.043)	(0.047)	(0.043)	(0.040)	(0.038)		(0.041)
Length of service		0.003							0.000
		(0.003)							(0.002)
Violence witnessed			–0.013						–0.030
			(0.016)						(0.022)
Violence received				0.037				0.061**	0.056**
				(0.028)				(0.025)	(0.026)
Violence perpetrated					–0.004				–0.009
					(0.011)				(0.013)
Radio						0.044			0.042
						(0.032)			(0.036)
Wealth						0.022***			0.020***
						(0.006)			(0.005)
Education						0.087***			0.084***
						(0.029)			(0.031)
Born here						–0.029			–0.017
						(0.022)			(0.020)
Age							0.006		–0.001
							(0.018)		(0.014)
Age^2							–0.000		–0.000
							(0.000)		(0.000)
UNITA (main)							0.013		0.022
							(0.046)		(0.046)
YOB-Bin 2							–0.136***		–0.126***
							(0.030)		(0.034)
YOB-Bin 3							–0.200***		–0.199***
							(0.064)		(0.067)
YOB-Bin 4							–0.214***		–0.228***
							(0.077)		(0.073)
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760	760
R^2	0.09	0.09	0.09	0.09	0.09	0.11	0.11	0.07	0.14

Notes: The table shows OLS estimates of Equation 1; the dependent variable is the indicator of community meeting attendance; the vector of included control variables varies across columns. *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: indicator of year of birth bin x (1 = Yes, 0 = No). The sample is grouped into four bins (1 – 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, *YOB-Bin 4* is the youngest group. Note that *age* in this specification captures within *YOB-Bin* variation. *Pre-Controls*: vector of eight pre-service family background characteristics. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro* of residence (= PSU) and municipality of recruitment.

Table 3: Local security provision: robustness to control variables

	Local security provision								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
WG high	0.153***	0.153***	0.153***	0.141***	0.153***	0.153***	0.150***		0.135***
	(0.033)	(0.033)	(0.033)	(0.035)	(0.033)	(0.033)	(0.034)		(0.037)
Length of service		0.002							0.002
		(0.002)							(0.002)
Violence witnessed			–0.002						–0.019
			(0.012)						(0.013)
Violence received				0.025				0.047***	0.033**
				(0.017)				(0.017)	(0.016)
Violence perpetrated					0.003				0.003
					(0.005)				(0.006)
Radio						–0.012			–0.010
						(0.032)			(0.032)
Wealth						0.003			0.002
						(0.006)			(0.006)
Education						–0.004			–0.007
						(0.019)			(0.017)
Born here						–0.014			–0.011
						(0.027)			(0.024)
Age							–0.002		–0.003
							(0.015)		(0.013)
Age^2							0.000		0.000
							(0.000)		(0.000)
UNITA (main)							–0.008		–0.015
							(0.022)		(0.026)
YOB-Bin 2							–0.006		–0.005
							(0.018)		(0.017)
YOB-Bin 3							–0.037		–0.033
							(0.052)		(0.051)
YOB-Bin 4							–0.038		–0.034
							(0.060)		(0.058)
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760	760
R^2	0.12	0.12	0.12	0.12	0.12	0.12	0.13	0.07	0.14

Notes: The table shows OLS estimates of Equation 1; the dependent variable is the indicator of local security provision; the vector of included control variables varies across columns. *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: indicator of year of birth bin (1 = Yes, 0 = No). The sample is grouped into four bins (1 – 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, *YOB-Bin 4* is the youngest group. Note that *age* in this specification captures within *YOB-Bin* variation. *Pre-Controls*: vector of eight pre-service family background characteristics. *Post-Location*: full set of *comuna* fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 4: First-stage and reduced form

	Wartime governance high				Community meetings		Local security	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
UNITA x YOB-Bin 2	–0.237***							
	(0.064)							
UNITA x YOB-Bin 3	–0.027							
	(0.095)							
UNITA x YOB-Bin 4	–0.296***							
	(0.043)							
UNITA x YOB-Bin 24		–0.246***	–0.239***	–0.237***	–0.072**	–0.082***	–0.054**	–0.053**
		(0.029)	(0.020)	(0.023)	(0.034)	(0.029)	(0.024)	(0.025)
Post-Controls	No	No	No	Yes	No	Yes	No	Yes
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760
R^2	0.05	0.05	0.07	0.07	0.10	0.12	0.07	0.07
F(UNITA x COB = 0)	13.70	71.92	130.11	97.04				

Notes: The table shows first-stage and reduced-form effects of the interaction of the armed group and the year-of-birth indicator variables (as specified in the row headers); the vector of included control variables varies across columns. Columns 1–4 show OLS estimates of Equation 2; the dependent variable is the high exposure to wartime governance indicator. Columns 5–8 show OLS estimates of a 'reduced-form' version of Equation 2; the dependent variables are the indicators of participation in community meetings (5–6) and local security provision (7–8). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: indicator of year of birth bin (1 = Yes, 0 = No). The sample is grouped into four bins (1 – 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, *YOB-Bin 4* is the youngest group. *YOB-Bin 24* collapses *YOB-Bin 2* and *YOB-Bin 4* into a joint bin. *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Length of Service*: length of wartime military service (in years). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses; in columns 1 and 2 clustered by municipality of recruitment, in columns 3–7 two-way clustered by *bairro* of residence and municipality of recruitment.

Table 5: Participation in local public good production (IV)

	Community Meetings				Local Security			
	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) OLS	(6) OLS	(7) IV	(8) IV
WG high	0.161***	0.154***	0.302**	0.344***	0.152***	0.152***	0.225**	0.221**
	(0.041)	(0.039)	(0.134)	(0.122)	(0.034)	(0.035)	(0.093)	(0.102)
Post-Controls	No	Yes	No	Yes	No	Yes	No	Yes
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760
R^2	0.12	0.14	0.10	0.11	0.13	0.13	0.11	0.12
1st stage F-stat(IV)			130.11	97.04			130.11	97.04

Notes: The table shows OLS and IV estimates of Equation 1; the dependent variables are the indicators of participation in community meetings (1–4) and local security provision (5–8); the vector of included control variables varies across columns. *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Length of Service*: length of wartime military service (in years). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 6: Mechanisms of impact

	Community meetings	Local security
	(1)	(2)
Panel A: Economic interactions		
WG high x Voluntary supply	0.017	–0.051
	(0.070)	(0.053)
WG high x Collected taxes	–0.014	0.031
	(0.029)	(0.022)
Panel B: Social attitudes		
WG high x Socializing	0.001	0.021
	(0.029)	(0.031)
WG high x Compliance	–0.035*	–0.038
	(0.021)	(0.031)
Panel C: Learning		
WG high x Factional ties	–0.005	0.075***
	(0.042)	(0.019)
WG high x Combat areas	0.023	0.049**
	(0.033)	(0.021)
Panel D: Political preferences		
WG high x Taught political ideas	0.052**	0.007
	(0.021)	(0.039)
WG high x Strategic instructions	0.062***	0.013
	(0.021)	(0.016)
Panel E: Additional tests		
WG high x Social regulation	0.082***	0.083***
	(0.033)	(0.029)
WG high x Ideological rule	–0.006	–0.015
	(0.035)	(0.040)
WG high x Violent rule	0.034	0.024
	(0.038)	(0.026)
WG high x Rank	0.085***	–0.038**
	(0.028)	(0.019)
WG high x No of battallions	0.063***	–0.021***
	(0.023)	(0.007)
WG high x UNITA	–0.013	–0.033
	(0.044)	(0.027)
WG high x Time in military	0.066*	–0.007
	(0.036)	(0.026)
WG high x Time since left military	–0.014	0.024
	(0.036)	(0.018)

Notes: The table shows interactive effects of exposure wartime exposure and a set of other variables (as specified in the row headers) on post-war participation in public good production (as specified in the column headers). Each cell shows a coefficient from a separate linear regression; model specification as in column 1 of Table 5 plus the main effect of the interacted variable. All interacted variables are standardized to zero mean and unit standard deviation. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 7: Political mobilization

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Politics							
	All	Participation			Attitudes		
	Index z-score	Political group	Election: registered	Election: voted	Interest in politics	Wartime governance	Did NOT vote MPLA
WG high	0.540***	0.165***	–0.000	0.014	0.300***	0.085**	–0.013
	(0.088)	(0.032)	(0.008)	(0.011)	(0.050)	(0.033)	(0.020)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	0.075	0.208	0.979	0.971	0.343	0.579	0.219
Observations	760	760	760	759	760	760	666
Panel B: Protests							
	All	Participation			Attitudes		
	Index z-score	Huambo	Other	Future	Violence protesters	Violence police	Violence stay
WG high	0.255***	–0.006	0.012	0.024	0.155***	0.176***	–0.050*
	(0.091)	(0.012)	(0.021)	(0.040)	(0.035)	(0.037)	(0.028)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	0.015	0.050	0.034	0.175	0.206	0.112	0.152
Observations	760	744	759	747	728	734	693

Notes: The table shows estimates of the effect of exposure wartime exposure on a set of post-war variables related to political mobilization (as specified in the column headers). Each cell shows a coefficient from a separate linear regression. In each panel, the z-score in column 1 is constructed by adding up the values of the indicators in columns 2-7, and standardizing the sum. *Std. controls*: Same specification as in column 1 of Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table 8: Social participation and cooperation

	(1)	(2)	(3)	(4)	(5)
Panel A: Groups					
	Index z-score	Cultural	Spiritual	Sports	Religious
WG high	0.441***	0.198***	0.080**	0.093***	–0.065***
	(0.125)	(0.032)	(0.037)	(0.036)	(0.019)
Std. controls	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	0.023	0.064	0.020	0.067	0.475
Observations	760	760	760	760	760
Panel B: Family					
	Index z-score	Lives with partner	Married	Caring z-score	Harmony z-score
WG high	–0.179**	0.014	–0.046	–0.273**	–0.148
	(0.070)	(0.014)	(0.030)	(0.118)	(0.092)
Std. controls	Yes	Yes	Yes	Yes	Yes
Mean dep.var.	–0.004	0.954	0.408	–0.006	–0.005
Observations	760	760	760	566	578

Notes: The table shows estimates of the effect of exposure wartime exposure on a set of post-war variables related to social participation and cooperation (as specified in the column headers). Each cell shows a coefficient from a separate linear regression. Panel A: The z-score in column 1 is constructed by adding up the values of the indicators in columns 2-5, and standardizing the sum. Panel B: The z-score in column 1 is constructed by standardizing the values of the indicators in columns 2-3, adding these values to those of the standardized indicators in columns 4-5, and standardizing the sum. *Std. controls*: Same specification as in column 1 of Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Appendix

A Background

A.1 War of independence: 1961–1974

Both the MPLA and UNITA emerged as nationalist movements opposing the Portuguese colonial rule in a war of independence between 1961 and 1974. They fought alongside other movements, most importantly the *Frente Nacional para a Libertação de Angola (FNLA)*. On 25 April 1974, the Portuguese government was overthrown in a coup d'état in Lisbon, which was led by Portuguese military generals who were ideologically opposed to the regime and its colonial endeavors. In January 1975, Portugal and the three Angolan movements signed an agreement for the transition to Angolan independence and the quick withdrawal of the colonial administration. In the following months inter-movement fighting erupted. FNLA was to fold away soon, while the MPLA seized control of the capital Luanda and UNITA held large Southern and Eastern territories. Led by upper-class 'assimilados', the MPLA declared Angolan independence in Luanda, on 11 November 1975, and formally became 'the government' of the new Angolan nation. On the same day, UNITA proclaimed their own government in Huambo, presenting itself as the 'true party of *all* Angolans', and formally became 'the rebels'.

A.2 Civil war: 1975–2002

From 1975 to 2002, the MPLA and UNITA fought the Angolan Civil War. Both parties managed to secure strong international allies and consistent 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 the diamond trade (e.g. Guidolin and La Ferrara, 2007; Berman et al., 2017). While most international support on either side was economic and political in nature, Cuba also sent large numbers of soldiers, which fought for the MPLA during the 1980s. This Cold War 'proxy-war' lasted 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. A series of violent episodes and see-saw battles ensued, only interrupted 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 abruptly ground to a halt and a Memorandum of Understanding was signed in April 2002.

A.3 Post-war period: since 2002

The 2002 Memorandum of Understanding prescribed rapid mass demobilization, which was implemented on both sides. Angola has since recorded more than a decade of absence of large-scale collective violence.

We study the effects of exposure to wartime governance twelve years after the end of the war. 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.[56] State institutions and their influence remain weak, preventing effective political and economic development (e.g. Maier, 2013).[57] Specifically, regional penetration by the central state remains extremely limited outside the capital and governance in many regions strongly depends on collective cooperation and coordination locally. Our setting thus allows us to investigate important long-run effects on post-war contributions to local public good production, which is crucial for welfare.

B Data and variables

B.1 Data collection (POEMA)

The data and analysis presented in the paper are part of the Study of Angolan Ex-Combatants (POEMA), a multidisciplinary project integrating qualitative and quantitative research. Ethnographic fieldwork started in January 2012 and was carried out by a social anthropologist for 12 months (Spall, 2015). We conducted quantitative fieldwork from March 2013 until February 2014 and collected the survey data in partnership with the local Angolan NGO Development Workshop (DW). While NGO activity is generally low in Angola, DW has operated in Angola – and especially Huambo Province – for more than 25 years, and was instrumental for making this survey possible.[58] With support from DW, we recruited, screened, trained, and managed a team of enumerators to do personal interviews based on electronic closed-ended questionnaires.

We use data from two separate interviews sampled veterans were asked to complete. First, a private household-level interview. For partnered veterans, this interview was done together with their (main) cohabitant partner. Second, a private individual-level interview. The resulting data captures information from three different stages of a respondent's life: just before wartime military service; during wartime military service; and today, i.e. twelve years after the end of the war. The pre-service and service information is based on recall, and we worked with a team of psychologists to design the survey in a way that minimizes recall biases in responses and optimizes the reliability of the data. In Section B.5 we explain in detail why we trust that our results are not driven by recall bias.

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. PSUs (*bairro*) were *comunas* (the lowest tier formal administrative unit) in rural areas, and boroughs in urban areas. EAs were either one settlement or multiple very small settlements in rural areas, and equivalent to neighborhoods in urban areas. In the absence of systematic and reliable veteran population data, we used general population data from the ongoing Angolan census to randomly draw PSUs and EAs, with weights proportional to the population. At each survey site, we engaged with local

[56] There were certain periods during the war when government controlled just 20% of its nominal territory (Soares de Oliveira, 2013).

[57] Despite an average annual GDP growth rate of 10.3% between 2000 and 2014 that was almost exclusively based on revenues from crude oil exports, Angola had the highest under-five mortality rate of all countries in the world in 2014 (WDI, 2015). In addition, the number of NGOs, foreign aid projects and their influence are also extremely small in Angola, as compared to countries of comparable development status (Soares de Oliveira, 2011).

[58] While well-known, DW have not implemented aid assistance or any development programs related to civic participation in Huambo province.

administrations, chiefs and coordinators to produce listings of all former soldiers residing in the EA. Results were double- and 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 Huambo 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.

B.2 Data

Measures of planning and implementing the delivery of public goods (outcome). The outcome variables of interest are individual engagement in planning and implementing the delivery of local public goods. We follow the literature and use binary indicators of participation (e.g. Bellows and Miguel, 2009), which are least prone to measurement error. We measure involvement in the planning stage via individual participation in community meetings – the deliberative institutions where planning takes place – via the question: "Did you attend any community meetings in the past year?" (1 if "Yes"; 0 otherwise). Participation in the delivery of public goods is captured through a binary variable based on the question: "Are you a member of a local self-security group or initiative?" (1 if "Yes"; 0 otherwise).

Measures of experience with wartime governance (treatment). The key explanatory variable is a soldier's experience with wartime governance of civilians by their armed group. The design of the survey module to measure exposure to wartime governance was guided by leading research on the effectiveness of governance by armed groups, which emphasizes the role of group capacity to deliver social goods and services, such as security and dispute resolution; investments in infrastructure and human capital, such as schools and access to safe water; and mechanisms to foster civilian feedback in governmental issues (Mampilly, 2011; Arjona, Kasfir, and Mampilly, 2015). Based on this framework the local ethnography preceding the survey, informal interviews, and extensive pre-testing identified seven specific aspects of governance, which were salient during the Angolan war. In the survey, we asked respondents about the overall frequency with which they experienced each aspect during their time with the armed group, coding seven variables:

Service provision. Question: "Overall, how frequently did you/your unit provide or help with access to schooling, water, electricity, communication or deliver related services?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Building infrastructure. Question: "Overall, how frequently did you/your unit build local facilities, such as schools, health posts, community centers, or buildings for religious purposes?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Provision of arms. Question: "Overall, how frequently did you/your unit provide arms to civilians to defend themselves?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Help with conflict resolution. Question: "Overall, how frequently did you/your unit help with the resolution of disputes between civilians, including conflicts over land or water?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Protection and security. Question: "Overall, how frequently did you/your unit protect civilians from war violence/attacks by armed groups?" (Five-point scale, ranging from "never"

(=0) to "extremely often" (=4)).

Requests for help with conflict resolution. Question: "Overall, how frequently did villagers approach/turn to you/your unit for help with the resolution of disputes between civilians, including conflicts over land or water?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

Requests for protection and security. Question: "Overall, how frequently did villagers approach/turn to you/your unit for protection of civilians from war violence/attacks by armed groups?" (Five-point scale, ranging from "never" (=0) to "extremely often" (=4)).

B.3 Index of experience with wartime governance

In our main analysis, we use a summary index built from the seven wartime governance indicators for two reasons. First, a composite measure reflects the diverse aspects of wartime governance. Second, an index may also improve statistical performance by mitigating concerns of and smoothing over measurement error in the different items.

While we expect that some of the dimensions of wartime governance above will be correlated, we hypothesize that each item captures a *different* aspect of governance, irrespective of its correlation with other items. We have therefore constructed an index that assigns equal weights to all items, which is technically similar to previously used indices of victimization (Voors et al., 2012; Bellows and Miguel, 2009). Alternative approaches calculate weights based on the variance-covariance matrix of the domains, including principal components (Filmer and Pritchett, 2001) and the Anderson (2008) method. In contrast to assigning equal weights, these approaches are implicitly based on the assumption that highly correlated items measure the same aspect and 'penalize' a given item based on intercorrelations with other items.

As a robustness check, we present and discuss results from these alternative weighting schemes and also investigate the effects and influence of each item separately in Section C.1. As our index is based on frequency measures of exposure (and we thus include length of service as a control variable in standard specifications), Section C.1 therefore also presents results from a tenure-adjusted index of exposure to wartime governance.

B.4 Summary statistics

Table A3 reports summary statistics for our main variables of interest and principal control variables. The figures show that there is considerable variability in experience with wartime governance. The mean respondent has an exposure to wartime governance score of 1.16 (SD=.77). The mean score of a respondent above the 75^{th} percentile of the score distribution is 2.35 (SD=.37). Similarly, we observe substantial variation in engagement in public good production today. About 22% of all respondents attend community meetings, while 6% contribute to local security. The low rate of participation in local security provision can be interpreted as support for our assumption that participation is voluntary and unpaid. Summary statistics for the individual components of the wartime governance index and selected related non-violent wartime interactions are reported in the bottom panel of Table A3.

B.5 Recall bias

Recall bias poses a threat to any study using retrospective data, and one key source of error is misreporting due to incorrect memory and lack of knowledge. A main concern is that respondents may misreport because they need to recall information and situations from a long time ago, or because they did not perceive these as important. Psychologists have emphasized and developed data collection techniques based on the premise that individuals may recall information better when they can relate them temporally to incisive events (e.g Freedman et al., 1988). Being enlisted into the military is a defining life event that should hence increase a respondent's ability to recall outcomes just before this happened reasonably accurately. Similarly, situations and events during military service are plausibly incisive experiences themselves and therefore less prone to memory failure. Two observations from fieldwork strengthen the assumption that the recalled service and pre-service information is reliable. First, "I don't know" or "I can't remember" responses were very rare. Second, the complementary ethnographic research by Spall (2015) gathered vast and detailed information on 'life before the military' and 'life in the military', which all interview partners were able and willing to provide.

A second important concern is that respondents may misreport their date of birth, which is a key variable in the study. To address this issue, we asked for information on both date of birth and current age, each in a separate interview. In the initial household interview with the veteran and his partner (if in a partnership), information on the ages of all household members was collected, including the veteran's. In the individual interview with the veteran, we asked for his date of birth. If necessary, enumerators provided help to determine the year of birth relying on an extensive list of important events in Angolan history. This entry was then immediately compared to the age entry (from the first interview) by the enumerator. For inconsistent answers, enumerators revisited the questions and provided assistance in determining correct and consistent answers. As an additional quality test, we check whether ages that are multiples of 5 years are more frequent, but the distribution of age answers does not support such an 'age heaping' (Figure A4).

A final concern is that individuals may refuse to answer or misreport for personal reasons, which is especially relevant for sensitive and traumatic experiences. A well-documented behavior is "embroidery" in reporting personal experiences in such cases (e.g Mausner and Kramer, 1985). Yet, veterans exhibited a remarkable openness toward discussing their lives during the war in qualitative interviews, including very personal experiences. In the quantitative component, no respondent opted to not answer questions regarding pre-military life, skip questions or quit the interview. As expected, a few respondents did opt to skip specific questions on military life, but the number of such cases was low and not systematic, i.e. not concentrated in specific questions or respondent characteristics. For these reasons, we trust that the vast majority of the questions were not particularly sensitive (in this context) or difficult to answer, and that our data are not affected by systematic misreporting. Even if misreporting was systematic, it is unlikely that it occurred in a way that would meaningfully affect our main statistics and estimates.

C Additional results

C.1 OLS

To account for potential intra-cluster correlation in the error term, we report Cameron, Gelbach, and Miller (2011)-type standard errors for our main specifications, two-way clustered at the PSU (N=22) and municipality of recruitment (N=38) levels. Table A4 presents p-values of our coefficient of interest based on alternative standard errors. In column 1, standard errors are one-way clustered by the location of current residence. Because the numbers of clusters may be 'small' (e.g. Cameron, Gelbach, and Miller, 2011), a potential concern is that we incorrectly inflate (or deflate) standard errors as a consequence of clustering. Column 2 therefore presents p-values based on one-way clustered standard errors that are parametrically corrected by the Moulton-method (e.g. Angrist and Pischke, 2008), and column 3 reports standard errors based on wild cluster bootstrapping.[59] In Columns 4 and 5 we report Huber-White and classical standard errors. The estimated standard errors do not vary noticeably across these methods, and all p-values for the coefficient of interest are (well) below .01.

As the main outcome variables are dichotomous, we test whether the main result is robust to non-linear model specifications. Table A5 reports average marginal effects estimated for a logit model. Across different specifications and outcomes, these are very similar – in significance and magnitude – to the coefficients in the linear probability model.

Next, we investigate additional results for the continuous index and from alternative weighting schemes for its construction. Table A6 shows that the positive association with post-war participation holds for the continuous wartime governance indices when standard controls are included, and demonstrate its robustness to three alternatively constructed indices, including principal component analysis, the method suggested by Anderson (2008), and a tenure-adjusted standard index. The standardized coefficients and standard errors demonstrate that our results are not sensitive to how the index measure is built.

While an index is advantageous for reasons presented earlier, the aggregate measure may mask interesting differences in influence across items. Table A7 reveals that the intercorrelations between the items are moderate in magnitude, ranging from .17 to .59. This corroborates our assumption that each experience covers a different aspect of governance and may not necessarily occur simultaneously with others. Table A8 displays results from separate regressions of the outcomes on the disaggregated (standardized) items, using standard specifications. We observe that each single component is a positive predictor of both steps of public good production and statistically significant, with the exception of service provision on community meeting attendance. These results suggests that our main result is not driven primarily by responses to a single question. As all items are standardized, we also observe that the magnitude of the disaggregated effects is relatively similar across items.

[59] We 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.

C.2 IV

In our main IV-specification we rely on a single instrument, which is favorable in terms of approximate bias (Hahn and Hausman, 2003; Angrist and Pischke, 2008). Yet, if our IV-strategy is valid, alternative instrument specifications should not produce estimates that are noticeably different in magnitude. Table A15 shows that an additive specification of the interaction terms of the armed groups indicator with bins II and IV (columns 2 and 5), produces very similar results, in terms of both economic and statistical significance, which also holds when the interaction term with bin III is controlled for separately (columns 3 and 6).

In our main IV-specification we deliberately left out the 'violence received' variable (shifted it into the error term) and showed that the identifying variation credibly produces estimates with a causal interpretation from these models. As a last robustness check for the validity of the IV results, we turn to (more speculative) models that include violence received as an endogenous regressor. The historiography of the war and the logic that underlies our IV strategy suggest that the interaction of the armed group indicator with bin III ma be positively correlated with the likelihood receiving violence. Table A16 confirms this intuition, and shows that the strongly significant correlation is robust to the inclusion of the Bin 24-factorial (column 3) and post-war outcomes (column 4). The associated F-statistic is moderate and ranges from 5.8 to 7.5. Yet, as our effect of interest is that of individual exposure to wartime governance rather than violence, finite-sample bias in the estimate of the violence received coefficient is not a first-order concern.

Table A17 displays IV- and corresponding OLS-estimates for community meeting attendance and security provision based on models that include the violence received variable. Columns 2, 4, 6 and 8 include both endogenous regressors and we report standard, Sanderson-Windmeijer and Angrist and Pischke F-statistics for each (Sanderson and Windmeijer, 2016; Angrist and Pischke, 2008).[60] For our purposes, the key observation is that the estimates from these models confirm the robust and positive effect of exposure to wartime governance on post-war participation we document in the main analysis. Specifically, the inclusion of violence received does not markedly change the magnitude or statistical significance of the estimate from the main analysis.

While the impact of experiences of violence on civic participation is not the focus of this study, we observe a positive correlation with post-war participation, which, however, disappears when we include exposure to wartime governance, for both OLS and IV estimation. While only suggestive, these results contrast with positive effects on pro-social behavior found in previous studies (e.g. Bauer et al., 2016), where endogeneity bias of the exposure to violence variable is often not directly accounted for. At the same time, it is important to note that our results for violence received need to be interpreted with caution. Most importantly, the instrument for violence is not 'strong' in the sense of Staiger and Stock (1997), and may not be valid. Yet, overall these results add to some recent evidence that casts some doubt on the conjecture of a positive, robust, and dominant impact of experiencing war violence on social behavior and attitudes (Rohner, Thoenig, and Zilibotti, 2013b; Adhvaryu and Fenske, 2014; Gáfaro, Ibáñez, and Justino, 2014; Child and Nikolova, 2016; Couttenier et al., 2017).

[60] These should be interpreted with caution as we estimate models with clustered standard errors while the basis for these test statistics are models with i.i.d. errors.

C.3 Village level

An interesting and important question from a development point of view is whether the behavioral relationships we isolated at the individual level scale up to and obtain at more aggregate levels. We thus explore whether the average contribution to public good production is higher in a village with a higher share of 'high-exposure' types in the village (based on our ex-combatant samples). If high-exposure types strongly cluster in certain villages, the sample may include 'extreme' villages with shares of zero or one. Figure A14 reveals that we do not observe such villages, and the village-level shares of high-exposure types are relatively narrowly distributed around the overall individual mean of being a high-exposure type of about one quarter.

Simple linear regression suggests that the village-level share of high types is positively associated with the village-level mean of contributing to public good production. Figure A15 shows scatter plots of the residuals and linear projections. The correlations are estimated at .3 for community-meeting attendance and .2 for local security provision. The effects are not statistically significant, which is not surprising given a sample size of 34 and the inclusion of fixed effects and robust standard errors clustered at the *comuna*-level. The positive relationship, however, strengthens confidence that the effect of interest is present and relevant for local development outcomes.

D Additional figures

Figure A1: Map of Huambo province

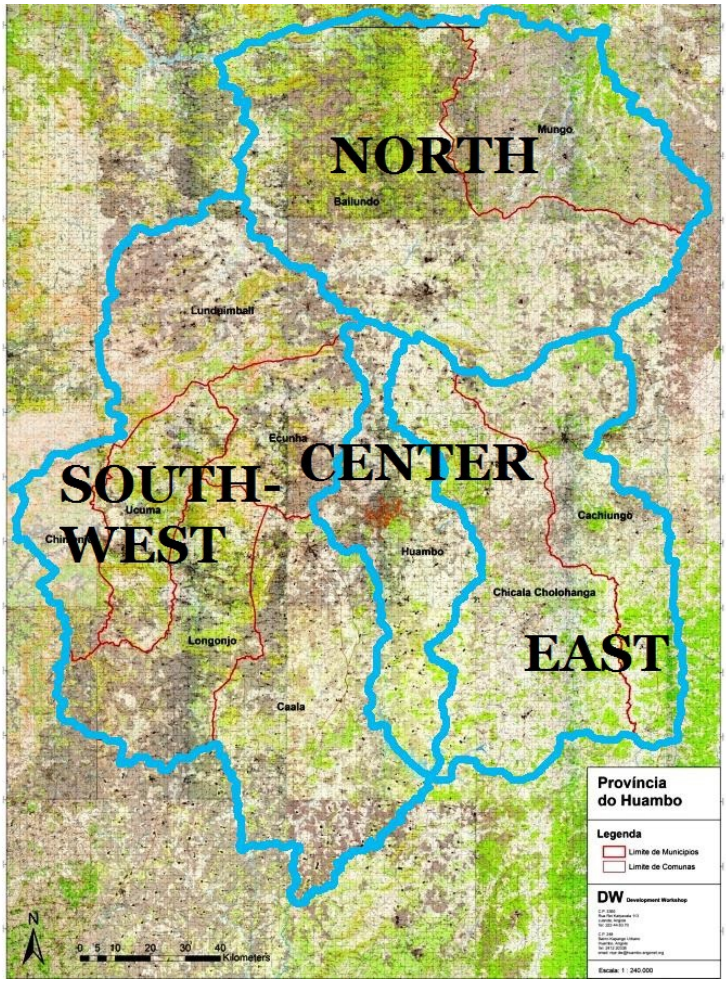


Figure A2: Distribution of date of entry by region of recruitment

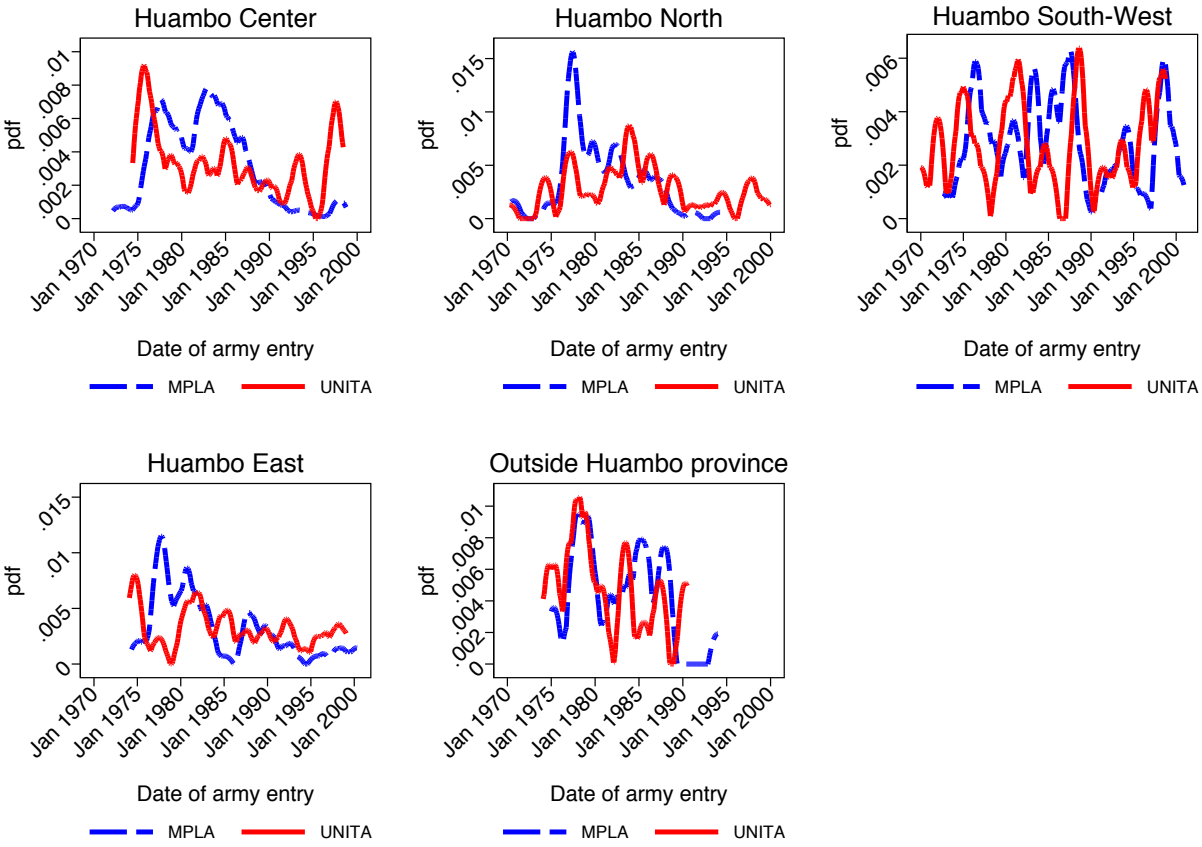


Figure A3: Distribution of age at first entry into armed group

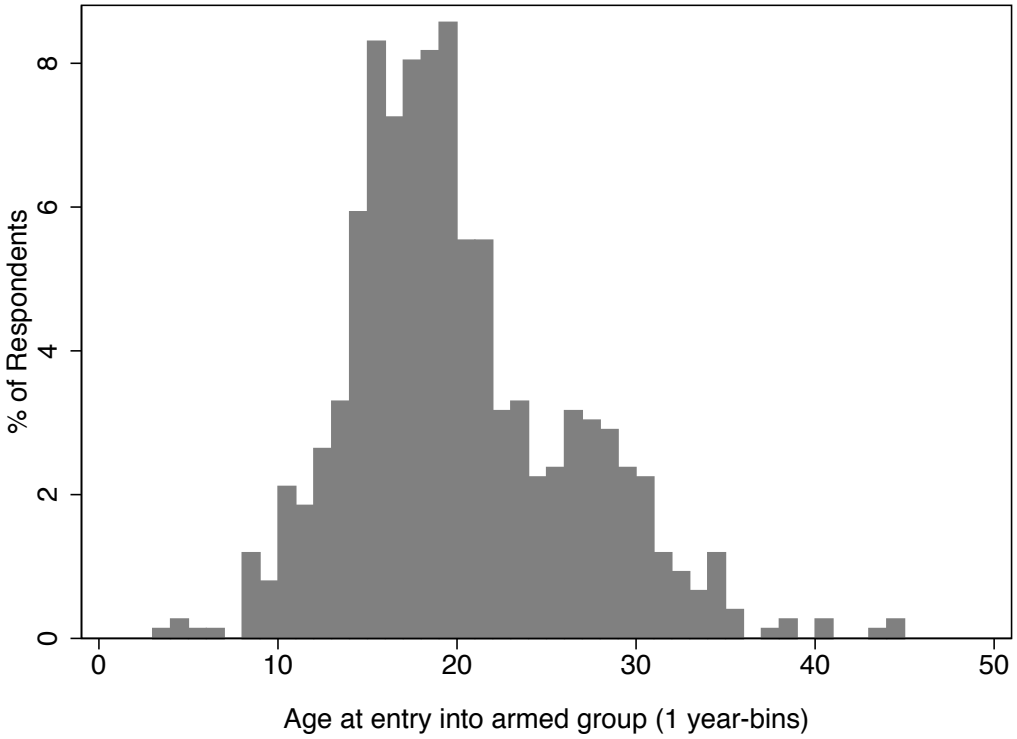


Figure A4: Distribution of year of birth

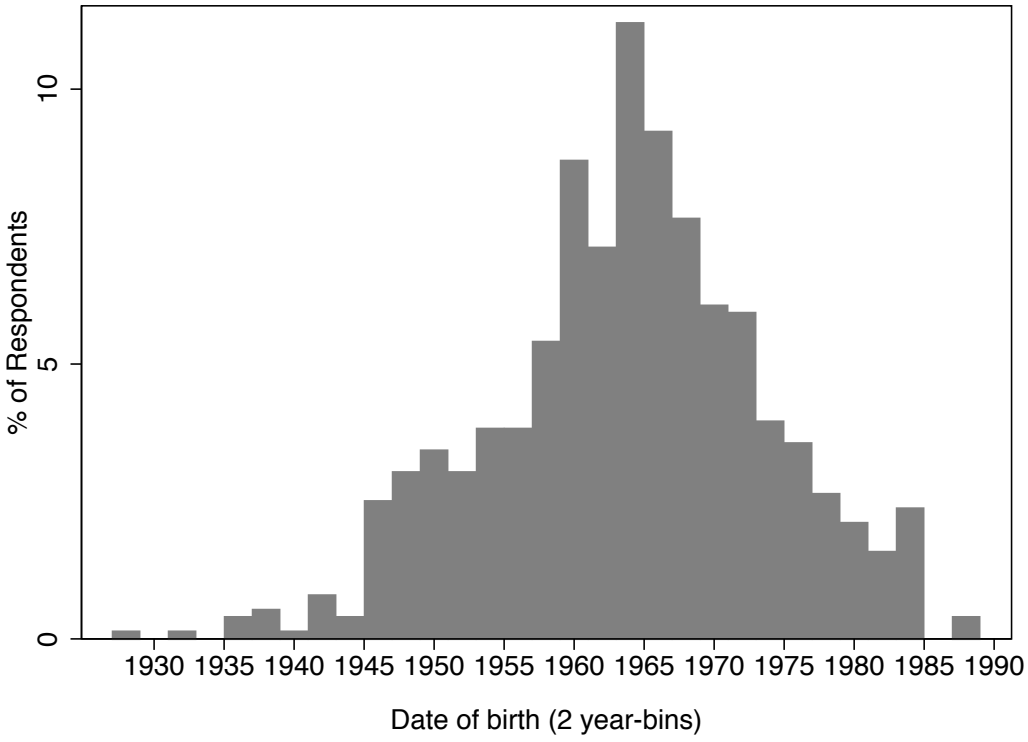


Figure A5: Date of military entry vs date of birth

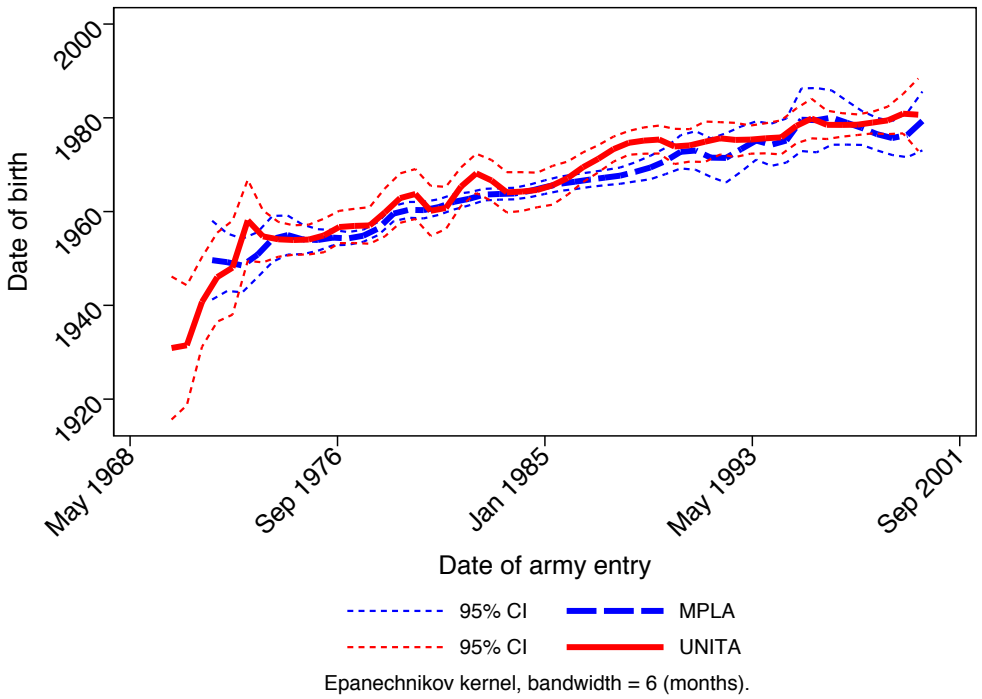


Figure A6: Probability density function across YOB-Bins

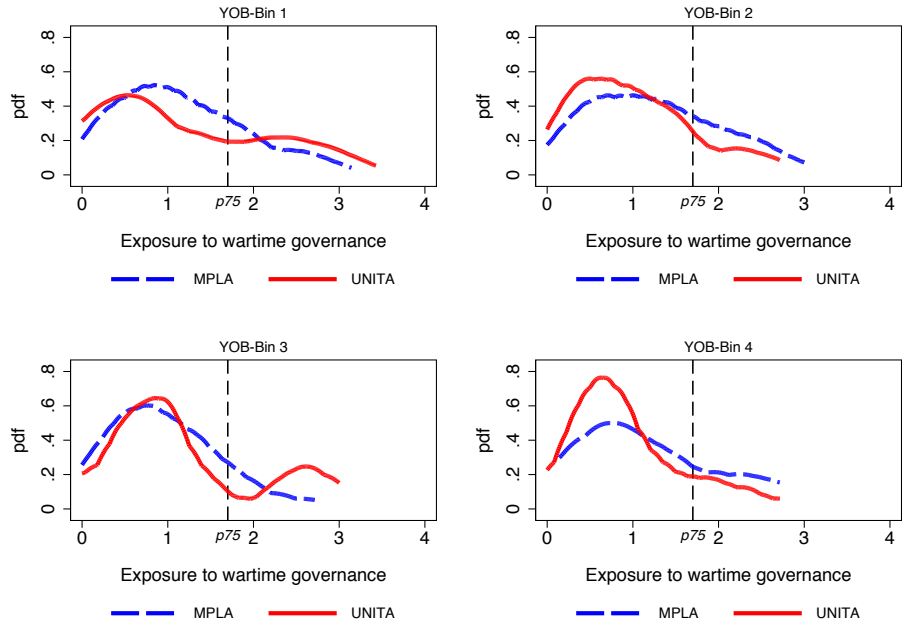


Figure A7: Effects of octiles of exposure to wartime governance

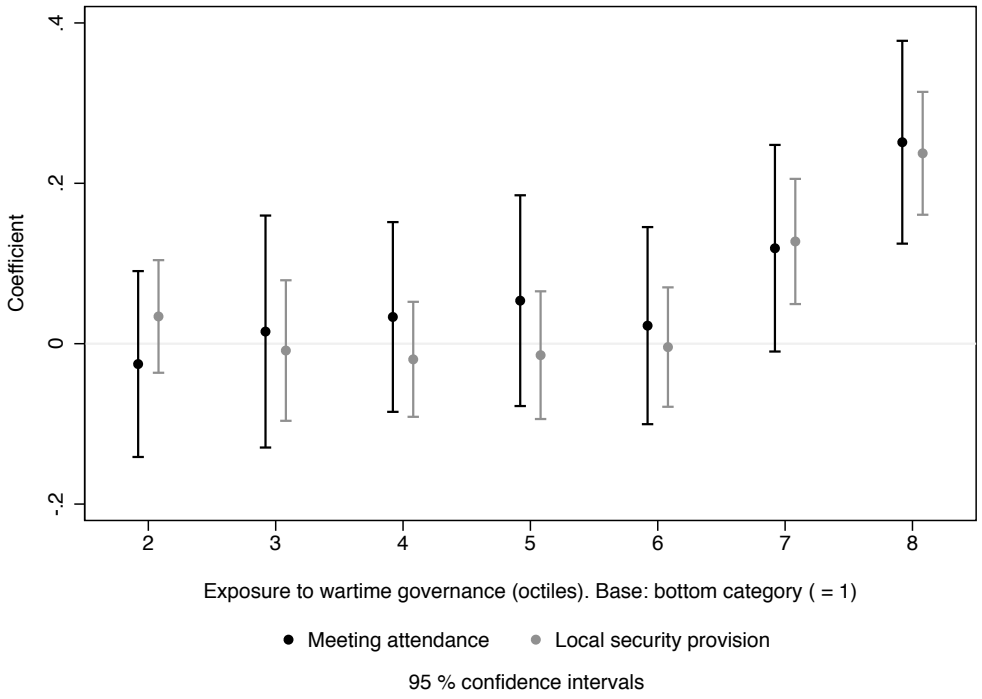
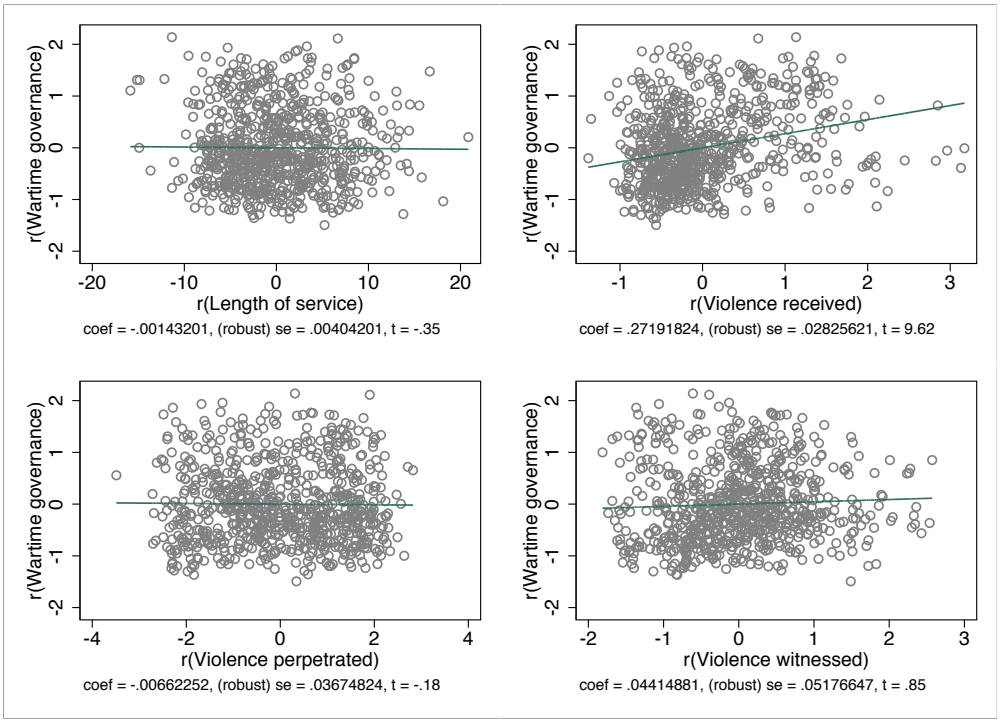


Figure A8: Residual-residual plots (other war experiences)



Notes: In each plot all standard regressors are partialled out. Top left: wartime military service in years. Top right: Violence received, measured by an average index based on 9 violent acts. Bottom left: Violence perpetrated, measured by an average index based on 3 violent acts. Bottom right: Violence witnessed, measured by an average index based on 5 violent acts. All 17 violent acts measured via a five-point frequency scale, ranging from never (= 0) to extremely often (= 4), based on survey instruments calibrated by a team of psychologists.

Figure A9: Predicting UNITA x YOB-Bin 24 (Bayesian Model Averaging)

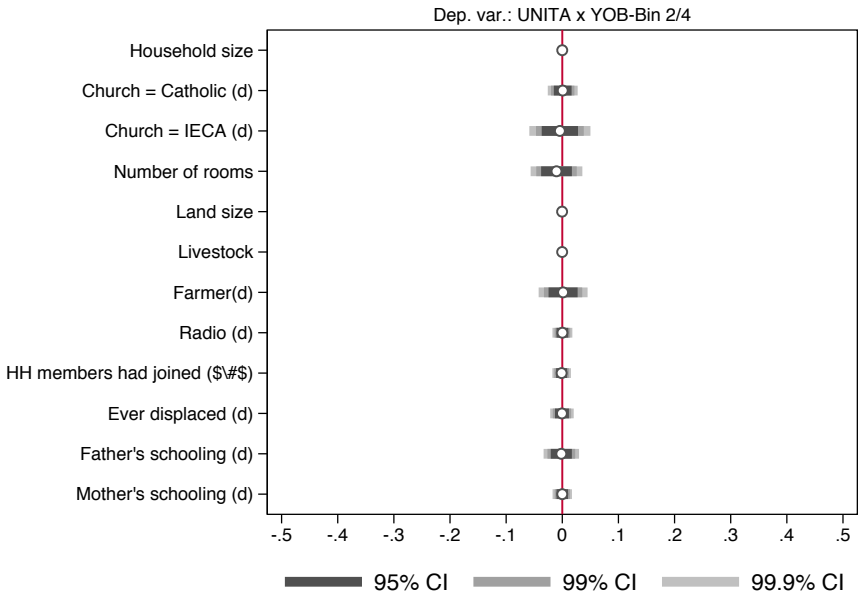
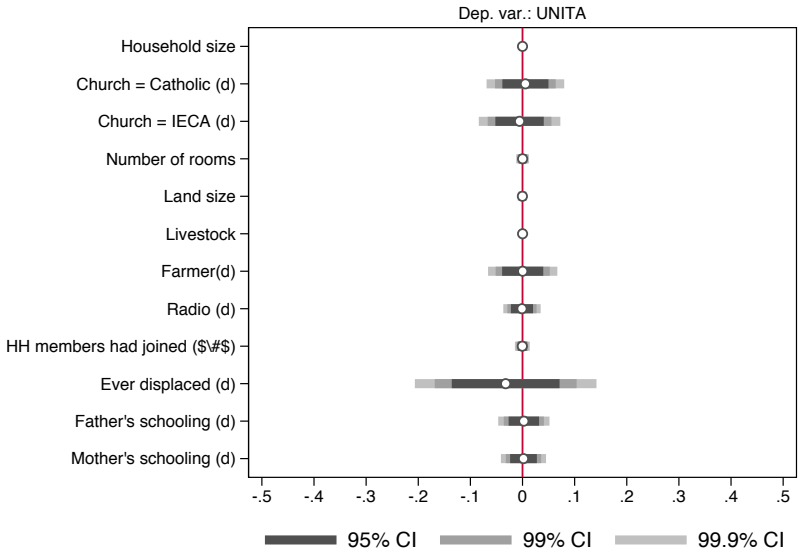


Figure A10: Predicting UNITA by date of birth (Bayesian Model Averaging)

(a) Sub-sample: YOB-Bin 24 = 0



(b) Sub-sample: YOB-Bin 24 = 1

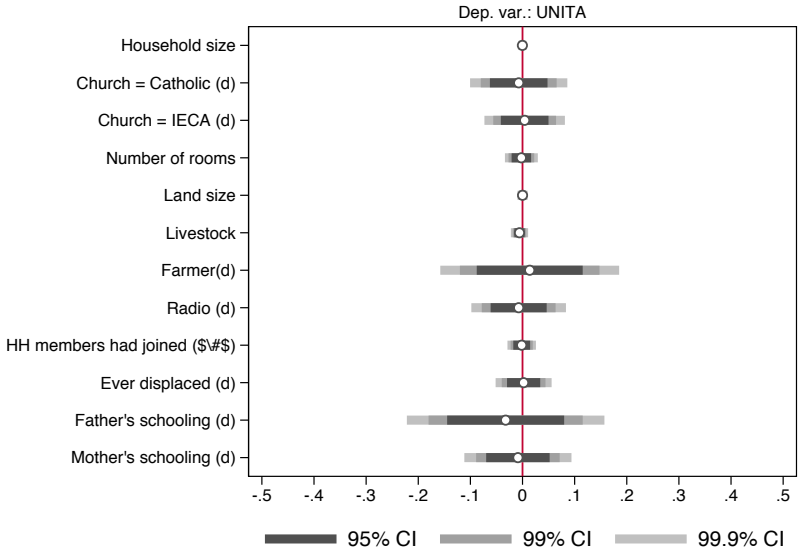
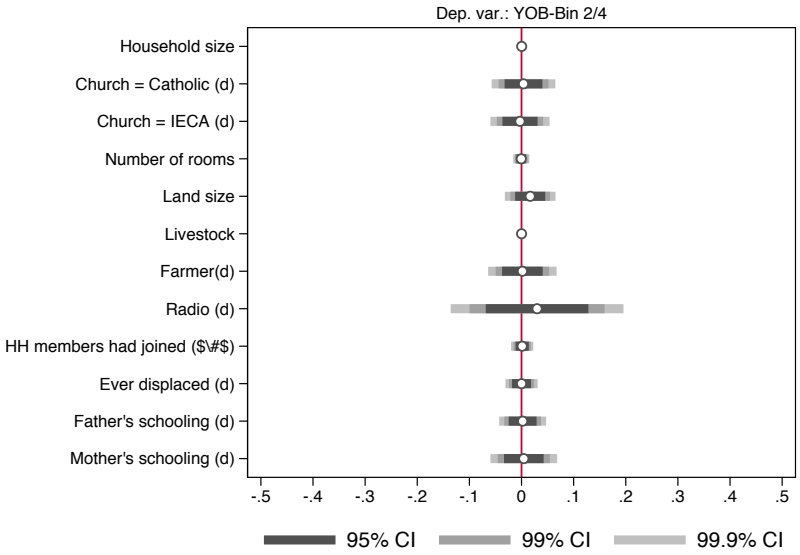


Figure A11: Predicting YOB-Bin 24 (Bayesian Model Averaging)

(a) Sub-sample: UNITA = 0



(b) Sub-sample: UNITA = 1

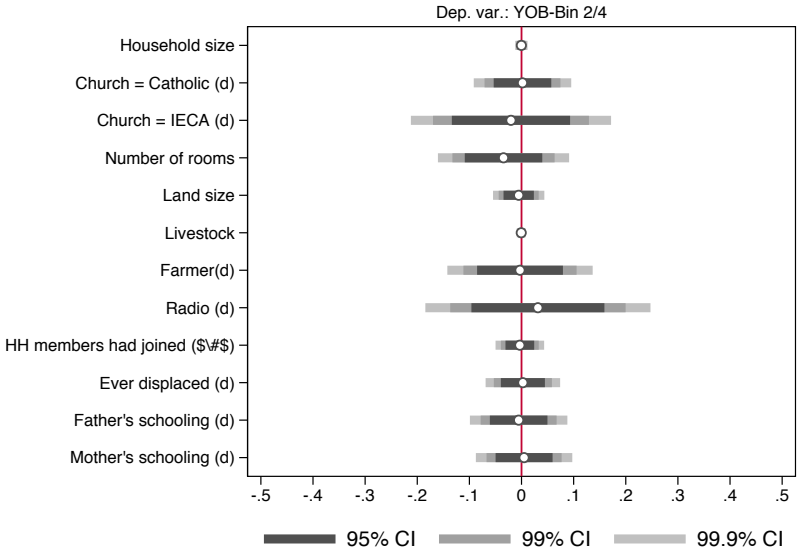
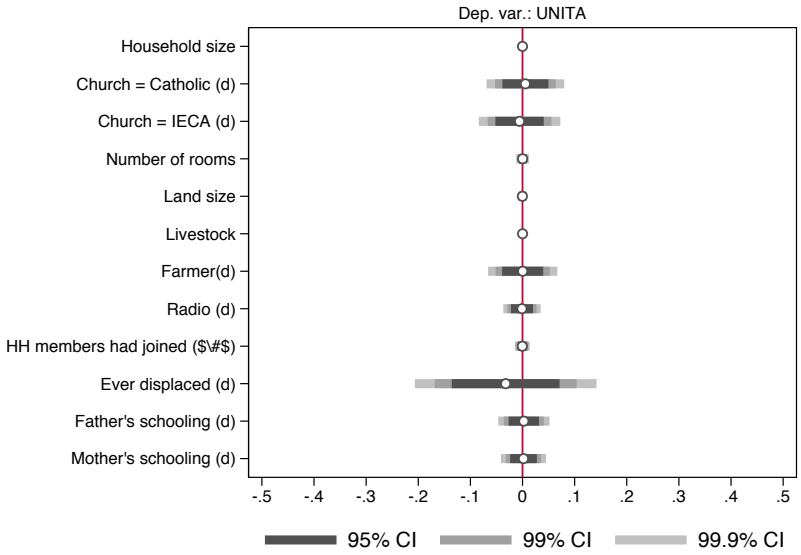


Figure A12: Predicting UNITA by date of entry (Bayesian Model Averaging)

(a) Sub-sample: YOE-Bin II/IV = 0



(b) Sub-sample: YOE-Bin II/IV = 1

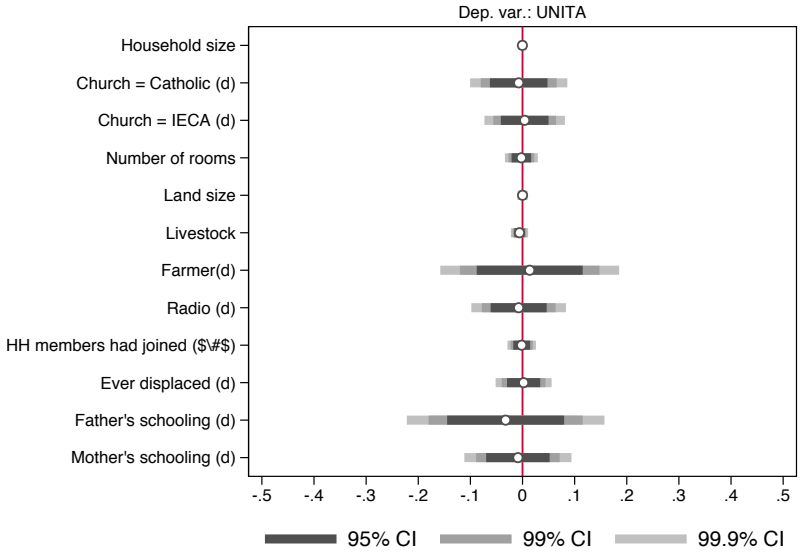
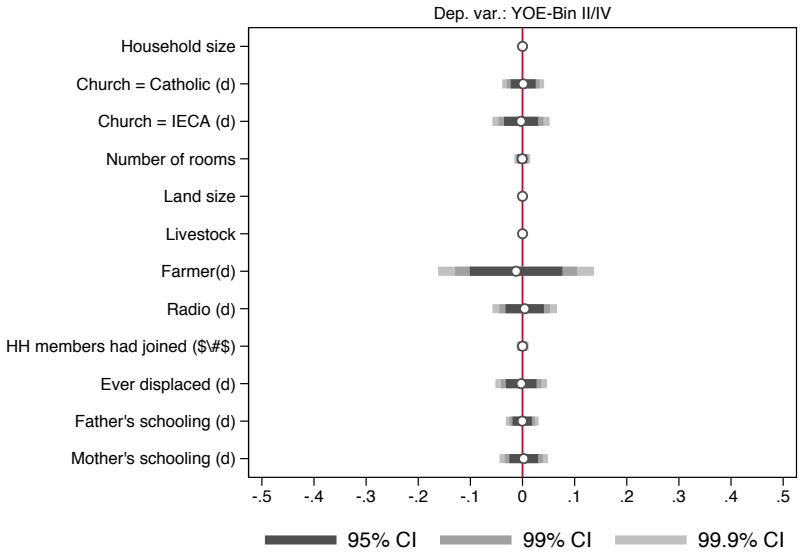


Figure A13: Predicting YOE-Bin II or IV (Bayesian Model Averaging)

(a) Sub-sample: UNITA = 0



(b) Sub-sample: UNITA = 1

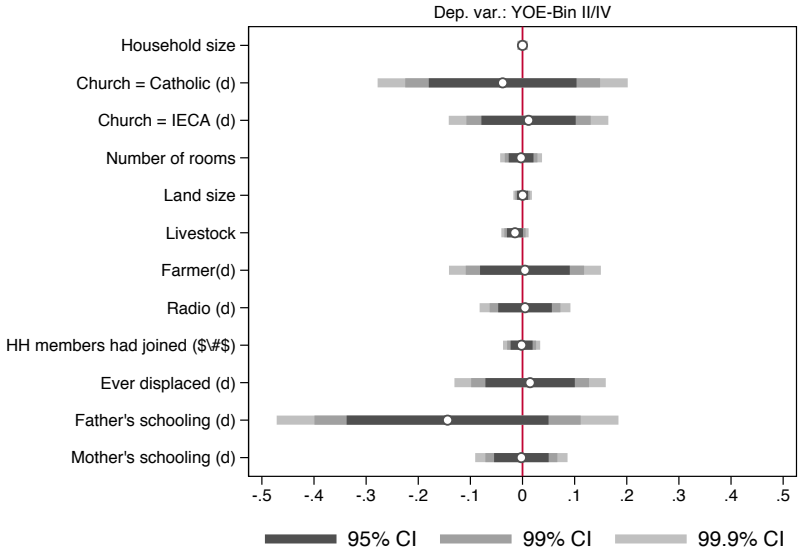


Figure A14: Histogram of share of high exposure respondents in village

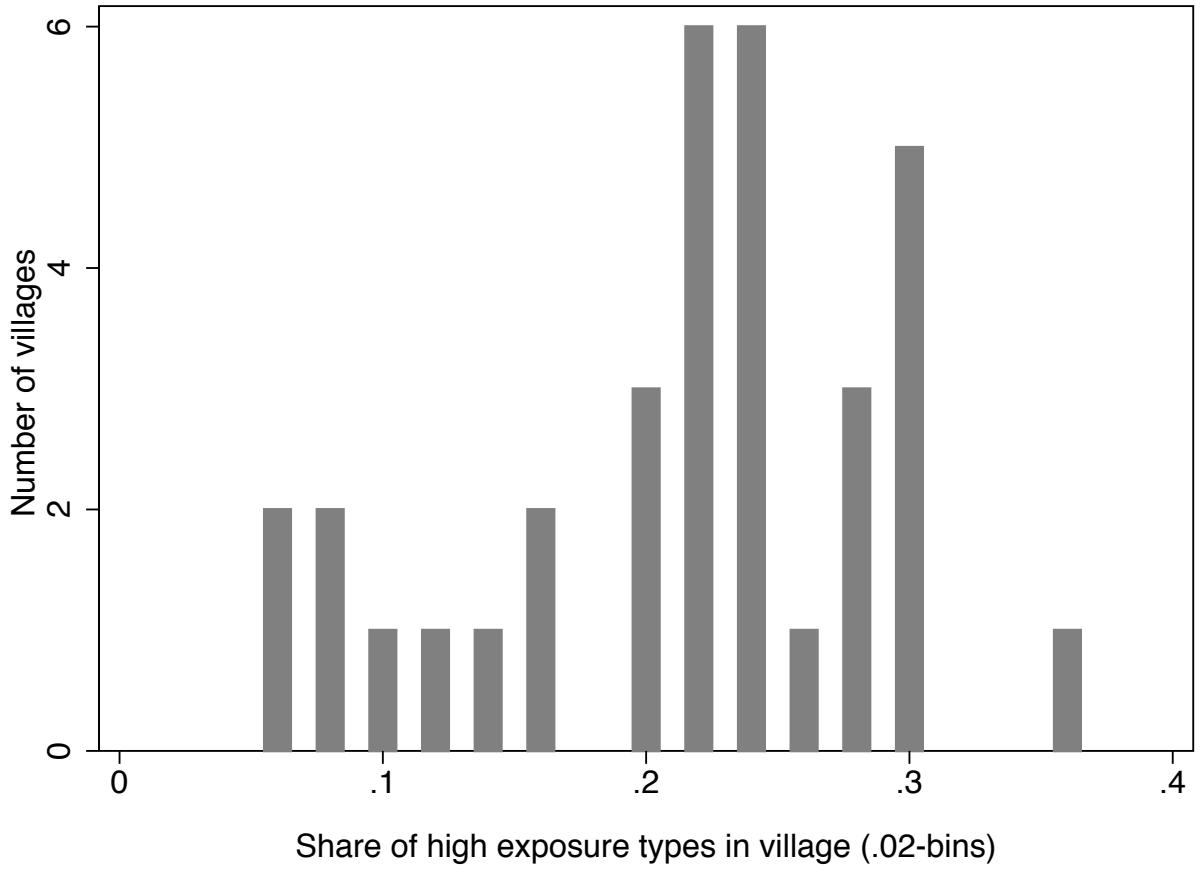
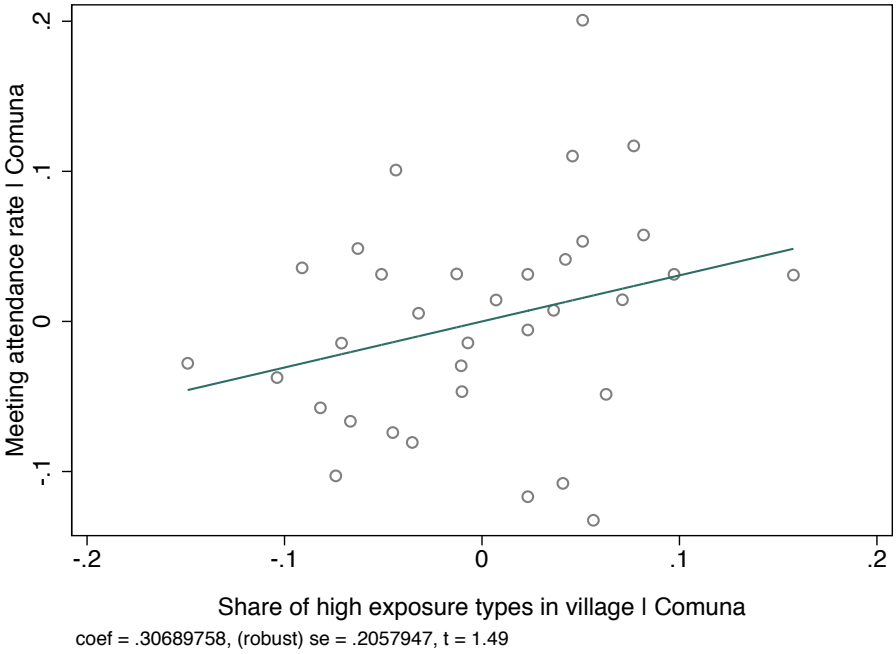
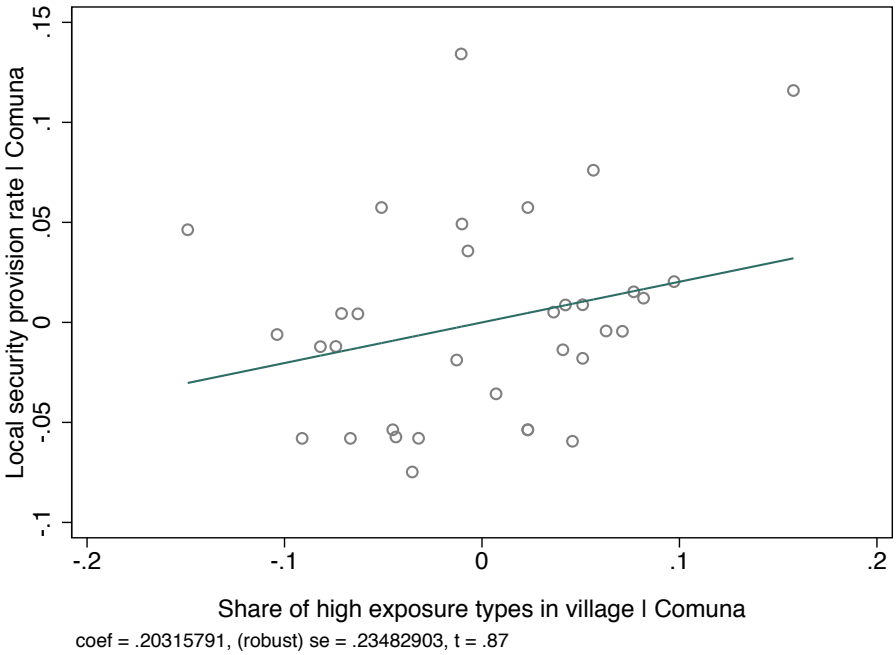


Figure A15: Village level

(a) Village level: participation in community meetings.



(b) Village level: participation in local security groups.



E Additional tables

Table A1: Wartime interactions with civilians (ever)

	UNITA	MPLA
Exposure to wartime governance (ever)		
Services	0.52	0.60
Infrastructure	0.61	0.61
Arms	0.23	0.16
Conflict resolution	0.49	0.52
Protection	0.81	0.90
Req: conflict resolution	0.36	0.34
Req: protection	0.39	0.31
Other (non-violent) interactions with civilians (ever)		
Collected taxes	0.36	0.31
Attended social events together	0.50	0.51
Taught political ideas	0.70	0.63
Observations	226	534

Notes: Entries indicate the share of respondents who ever experienced the specific interaction when they were in the military.

Table A2: Selection into UNITA

	MPLA				
	(1)	(2)	(3)	(4)	(5)
Household size (#)	–0.001	–0.001	–0.002	–0.002	0.001
	(0.849)	(0.801)	(0.686)	(0.775)	(0.841)
Church = Catholic (d)	–0.023	–0.031	–0.034	–0.066	–0.052
	(0.700)	(0.583)	(0.544)	(0.239)	(0.349)
Church = IECA (d)	–0.032	–0.049	–0.053	–0.069	–0.050
	(0.627)	(0.427)	(0.388)	(0.256)	(0.401)
Number of rooms (#)	–0.001	0.015	0.013	0.006	0.019
	(0.945)	(0.292)	(0.346)	(0.634)	(0.150)
Land size (#)	–0.001	–0.004	–0.004	–0.005	–0.005
	(0.880)	(0.592)	(0.594)	(0.483)	(0.512)
Livestock (#)	–0.002	–0.001	–0.002	–0.002	–0.003
	(0.331)	(0.480)	(0.235)	(0.278)	(0.149)
Farmer (d)	0.061	0.082	0.064	0.058	0.082
	(0.381)	(0.215)	(0.325)	(0.372)	(0.199)
Radio (d)	–0.028	–0.027	–0.020	–0.014	–0.043
	(0.411)	(0.404)	(0.528)	(0.653)	(0.174)
HH members had joined (#)	–0.014	–0.015	–0.013	–0.015	–0.014
	(0.329)	(0.269)	(0.312)	(0.243)	(0.235)
Ever displaced (d)	–0.044	–0.031	–0.017	–0.014	–0.024
	(0.219)	(0.357)	(0.607)	(0.666)	(0.450)
Father's schooling (d)	–0.023	–0.011	–0.021	–0.002	0.002
	(0.557)	(0.761)	(0.571)	(0.966)	(0.961)
Mother's schooling (d)	–0.004	–0.023	–0.043	–0.054	–0.023
	(0.922)	(0.536)	(0.240)	(0.136)	(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	759	759	759	759	759
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 A3: Key summary statistics

	Mean	SD	MIN	MAX
Main outcome: Participation in public good production				
Community meetings	0.22	0.417	0	1
Local security provision	0.06	0.246	0	1
Main explanatory variable: Exposure to wartime governance				
Exposure to wartime governance	1.16	0.772	0	3
... (in top quartile)	2.35	0.366	2	3
Armed group and pooled birth cohorts				
UNITA (main)	0.30	0.457	0	1
YOB $\leq$ 1962 (Bin 1)	0.43	0.496	0	1
1963 $<$ YOB $\leq$1971 (Bin 2)	0.38	0.485	0	1
1972 $<$ YOB $\leq$1975 (Bin 3)	0.09	0.282	0	1
YOB $>$1975 (Bin 4)	0.10	0.304	0	1
War control variables				
Length of service	11.15	6.806	1	40
Violence witnessed	1.49	0.853	0	4
Violence received	0.84	0.746	0	4
Violence perpetrated	2.13	1.415	0	4
Socio-economic control variables				
Radio	0.74	0.440	0	1
Wealth	0.00	2.501	-2	26
Education	0.85	0.361	0	1
Born in this comuna	0.74	0.439	0	1
Age	49.73	9.847	25	86
Wartime governance components				
Services	1.29	1.349	0	4
Infrastructure	1.52	1.526	0	4
Arms	0.40	0.949	0	4
Conflict resolution	1.07	1.274	0	4
Protection	2.46	1.329	0	4
Civilian requests for conflict resolution	0.70	1.137	0	4
Civilian requests for protection	0.67	1.092	0	4
Other non-violent interactions with civilians				
Taxes on economic activity	0.73	1.231	0	4
Attended social events together	0.92	1.128	0	4
Taught political ideas	1.54	1.372	0	4
Observations	760			

Notes: Binary measures: Community meetings, Local security, UNITA (main), Radio, Education, and Born here (1 = Yes, 0 = No). *Index measures*: Wartime governance (7 items), Violence witnessed (5), Violence received (9), Violence perpetrated (3), Wealth (20). *In years*: Length of service, Age. *Frequency measures*: Services, Infrastructure, Arms, Conflict, Resolution, Protection, Civilian requests for conflict resolution, Civilian requests for protection, Collected taxes, Attended social events together, Taught political ideas (4 = Extremely often,..., 0 = Never.)

Table A4: Robustness: Alternative standard errors

	(1) CGM 1-Way	(2) Moulton	(3) WCB	(4) Huber-White	(5) Classical
Panel A: Community meeting attendance					
WG high coefficient	0.173				
p-val	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Local security provision					
WG high coefficient	0.153				
p-val	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760

Notes: Estimation of standard errors: one-way clustered by *bairro* of residence (col. 1), Moulton-corrected (col. 2), wild-cluster bootstrapped (col. 3), Huber-White-corrected (col. 4), classical (col. 5). *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Post-Location FE*: full set of *comuna* fixed effects.

Table A5: Non-linear model specifications

	Community Meetings				Local Security			
	(1) OLS	(2) OLS	(3) AME	(4) AME	(5) OLS	(6) OLS	(7) AME	(8) AME
WG high	0.161***	0.159***	0.165***	0.161***	0.152***	0.151***	0.199***	0.196***
	(0.039)	(0.039)	(0.039)	(0.039)	(0.030)	(0.030)	(0.037)	(0.036)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	No	Yes	No	Yes	No	Yes	No
Post-Location FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	760	760	760	760	760	760	760	760

Notes: Estimation: OLS (cols. 1, 2, 5, 6), logit (cols. 3, 4, 7, 8; average marginal effect). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location*: full set of recruitment region fixed effects. *Post-Location*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Huber-White standard errors in parentheses, fixed effects as in standard specifications (logit results to be analyzed with caution).

Table A6: Alternatively constructed indices

	Community Meetings				Local Security			
	(1) Ind1	(2) Ind2	(3) Ind3	(4) Ind4	(5) Ind1	(6) Ind2	(7) Ind3	(8) Ind4
WG index	0.063***	0.068***	0.067***	0.061***	0.054***	0.059***	0.058***	0.044***
	(0.015)	(0.019)	(0.017)	(0.013)	(0.010)	(0.009)	(0.011)	(0.012)
UNITA+Cohort	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760

Notes: Standardized coefficients; all indices standardized to zero mean and unit standard deviation. Ind1: standard index (cols. 1, 5). Ind2: index based on principal component analysis (cols. 2, 6). Ind3: index based on the method described in Anderson (2008) (cols. 3, 7). Ind4: standard index adjusted for length of service (cols. 4, 8). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Length of Service*: length of wartime military service (in years). *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A7: Wartime governance index components: correlation

	Services	Infra-structure	Arms	Conflict resolution	Pro-tection	Req: conflict resolution	Req: protection
Services	1.000						
Infrastructure	0.503	1.000					
Arms	0.171	0.316	1.000				
Conflict resolution	0.180	0.278	0.383	1.000			
Protection	0.354	0.403	0.201	0.247	1.000		
Req: conflict resolution	0.118	0.164	0.302	0.374	0.183	1.000	
Req: protection	0.102	0.141	0.410	0.414	0.195	0.591	1.000

Notes: Req: conflict resolution: civilians requested help with conflict resolution. *Req: protection*: civilians requested protection.

Table A8: Wartime governance index components: impact

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Community meetings attendance							
Services	0.009						
	(0.009)						
Infrastructure		0.026*					
		(0.014)					
Arms			0.047**				
			(0.021)				
Conflict resolution				0.050***			
				(0.013)			
Protection					0.043***		
					(0.008)		
Req: conflict resolution						0.042**	
						(0.019)	
Req: protection							0.041**
							(0.016)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760
R^2	0.10	0.10	0.11	0.11	0.11	0.10	0.10
Panel B: Participation in local security groups							
Services	0.022***						
	(0.008)						
Infrastructure		0.022**					
		(0.010)					
Arms			0.051***				
			(0.015)				
Conflict resolution				0.036***			
				(0.011)			
Protection					0.023***		
					(0.004)		
Req: conflict resolution						0.055***	
						(0.009)	
Req: protection							0.037***
							(0.013)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760
R^2	0.07	0.08	0.11	0.09	0.08	0.11	0.09

Notes: Standardized measures, with mean zero and unit standard deviation. *Std. controls*: Same specification as in column 1 of Table 5. *Req: conflict resolution*: civilians requested help with conflict resolution. *Req: protection*: civilians requested protection. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A9: Correlations of treatment with military or background traits

	Sample			Difference	
		High exposure			
	Full	Yes	No	Diff	p
Background					
Farming household (d)	0.94	0.94	0.93	0.00	0.87
Catholic household (d)	0.65	0.64	0.65	–0.01	0.90
Father any schooling (d)	0.57	0.54	0.57	–0.03	0.49
Mother any schooling (d)	0.39	0.40	0.38	0.01	0.81
Household size (#)	7.48	7.73	7.41	0.33	0.22
HH members recruited (#)	0.90	0.94	0.88	0.05	0.62
HH members killed (#)	0.15	0.17	0.14	0.03	0.30
Subjective health (1-10)	6.15	6.43	6.08	0.35	0.14
Any training (d)	0.06	0.04	0.07	–0.03	0.18
Any schooling (d)	0.64	0.73	0.61	0.12***	0.00
Schooling (yrs)	2.43	2.82	2.32	0.50**	0.02
Armed group and pooled birth cohorts					
UNITA (main)	0.30	0.30	0.30	0.01	0.87
YOB $\leq$ 1962	0.43	0.46	0.43	0.03	0.51
1963 $<$ YOB $\leq$1971	0.38	0.40	0.37	0.02	0.58
1972 $<$ YOB $\leq$1975	0.09	0.07	0.09	–0.02	0.34
YOB $>$1975	0.10	0.08	0.11	–0.03	0.29
Military					
Any training (d)	0.97	0.96	0.97	–0.00	0.82
Training (weeks)	15.38	19.35	14.31	5.04***	0.00
Role: infant (d)	0.43	0.36	0.44	–0.08*	0.07
Rank: private (d)	0.47	0.44	0.48	–0.04	0.40
Combat zone (d)	0.77	0.86	0.75	0.11***	0.00
Battallions (#)	2.14	2.40	2.07	0.33*	0.05

Notes: Binary measures of background and military variables are indicated by '(d)'. All other measures of background and military variables are measured as a quantity. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: UNITA sample: predictive power of YOB-Bins 2 and 4

	First-stage		Reduced form			
	(1)	(2)	(3)	(4)	(5)	(6)
	Wartime governance		Community meetings		Local security	
YOB-Bin 2	–0.205**	–0.230**	–0.102***	–0.110***	–0.045**	–0.048***
	(0.087)	(0.102)	(0.014)	(0.031)	(0.020)	(0.017)
YOB-Bin 4	–0.257***	–0.300***	–0.056	–0.077*	–0.073*	–0.082*
	(0.053)	(0.046)	(0.044)	(0.040)	(0.039)	(0.042)
Post-Controls	No	Yes	No	Yes	No	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	No	Yes	Yes	Yes	Yes	Yes
Observations	225	225	225	225	225	225
R^2	0.23	0.32	0.17	0.24	0.26	0.27
F(joint)	43.52	59.36				

Notes: *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses; in columns 1 and 2 clustered by municipality of recruitment, in columns 3–7 two-way clustered by *bairro* of residence and municipality of recruitment.

Table A11: Correlations of the instrument with military or background traits

	Sample			Difference	
		UNITA x YOB-Bin2/4 = 1			
	Full	Yes	No	Diff	p
Background					
Farming household (d)	0.94	0.95	0.93	0.02	0.54
Catholic household (d)	0.65	0.68	0.64	0.04	0.39
Father any schooling (d)	0.57	0.52	0.58	–0.06	0.24
Mother any schooling (d)	0.39	0.38	0.39	–0.01	0.86
Household size (#)	7.48	7.28	7.51	–0.23	0.45
HH members recruited (#)	0.90	0.75	0.92	–0.17	0.16
HH members killed (#)	0.15	0.17	0.14	0.03	0.41
Subjective health (1-10)	6.15	5.98	6.18	–0.20	0.45
Any training (d)	0.06	0.09	0.05	0.04*	0.08
Any schooling (d)	0.64	0.59	0.65	–0.05	0.29
Schooling (yrs)	2.43	2.14	2.48	–0.34	0.15
Military					
Any training (d)	0.97	0.97	0.97	–0.00	0.99
Training (weeks)	15.38	14.19	15.60	–1.41	0.20
Role: infant (d)	0.43	0.47	0.42	0.05	0.35
Rank: private (d)	0.47	0.41	0.48	–0.07	0.16
Combat zone (d)	0.77	0.78	0.77	0.01	0.89
Battallions (#)	2.14	2.09	2.15	–0.06	0.76

Notes: Binary measures of background and military variables are indicated by '(d)'. All other measures of background and military variables are measured as a quantity. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Correlations of the instrument with pre-service variables

	UNITA x YOB-Bin 24		UNITA first army			
	(1)	(2)	(3)	(4)	(5)	(6)
			Bin 24	Bin 24	Bin 24	Bin 24
Household size	0.000	0.000	–0.003	0.002	–0.005	0.003
	(0.959)	(0.996)	(0.746)	(0.780)	(0.457)	(0.560)
Church = Catholic (d)	–0.040	–0.052	–0.079	0.032	–0.106	0.066
	(0.407)	(0.353)	(0.363)	(0.707)	(0.381)	(0.250)
Church = IECA (d)	–0.074	–0.087	–0.093	0.031	–0.114	0.086
	(0.154)	(0.166)	(0.338)	(0.727)	(0.417)	(0.391)
Number of rooms	–0.020*	–0.021	–0.025	0.016	–0.023	0.017
	(0.082)	(0.218)	(0.251)	(0.411)	(0.376)	(0.233)
Land size	–0.003	–0.003	–0.014	0.019	–0.008	0.014
	(0.683)	(0.564)	(0.217)	(0.166)	(0.136)	(0.220)
Livestock	–0.001	–0.002	–0.003	–0.001	–0.003	–0.004
	(0.444)	(0.148)	(0.295)	(0.666)	(0.105)	(0.169)
Farmer (d)	0.009	–0.014	0.024	0.080	–0.057	0.118
	(0.869)	(0.850)	(0.818)	(0.404)	(0.692)	(0.144)
Radio (d)	0.019	0.030	–0.024	–0.055	–0.017	–0.065
	(0.482)	(0.322)	(0.649)	(0.246)	(0.589)	(0.259)
Members joined (#)	–0.013	–0.012	–0.030	0.009	–0.028	0.010
	(0.233)	(0.143)	(0.146)	(0.657)	(0.121)	(0.404)
Ever displaced (d)	–0.013	–0.004	–0.032	–0.057	–0.011	–0.046
	(0.634)	(0.919)	(0.550)	(0.251)	(0.872)	(0.227)
Father's schooling (d)	–0.030	–0.036	–0.071	0.022	–0.069	0.020
	(0.328)	(0.342)	(0.215)	(0.686)	(0.283)	(0.722)
Mother's schooling (d)	0.017	0.005	0.014	–0.018	–0.000	–0.047
	(0.574)	(0.868)	(0.809)	(0.738)	(1.000)	(0.296)
Pre-Location FE	No	Yes	No	No	Yes	Yes
Observations	760	760	364	396	364	396
R^2	0.02	0.06	0.04	0.02	0.14	0.09

Notes: Pre-Location FE: full set of recruitment region fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Standard errors in parentheses; in columns 1, 3 and 4 classical; in columns 2, 5 and 6 robust, clustered at the pre-service location level.

Table A13: Correlations of the instrument with experiences of violence

	Sample			Difference	
		UNITA x YOB-Bin 24 = 1			
	Full	Yes	No	Diff	*p*
Specific experiences					
Battle: In line of fire	4.64	4.23	4.72	–0.49	0.67
Battle: Severely injured	0.80	0.67	0.83	–0.16	0.21
Battle: Many lose life	2.02	2.68	1.90	0.78	0.11
Civilians: Looting	1.63	1.88	1.58	0.30	0.43
Civilians: Massacres	0.96	1.03	0.95	0.08	0.74
Civilians: Sexual violence	0.65	0.54	0.67	–0.13	0.70
Index measures					
Fighting experience	1.57	1.60	1.56	0.04	0.68
Battle-field violence received	1.32	1.28	1.33	–0.05	0.57
Violence received (full)	0.84	0.78	0.85	–0.08	0.32
Violence perpetrated (full)	2.15	2.21	2.14	0.07	0.63
Wartime governance (index)	1.16	0.96	1.19	–0.23***	0.00

Notes: All measures were based on survey instruments calibrated by a team of psychologists. Specific measures of battle-field and one-sided violence: *In the line of fire*, *Severely injured*, *Many (companions) lose life*, *Looting*, *Massacres*, *Sexual violence (against civilian women)* denote the self-reported, total number of such situations experienced. For measure of violence against civilians, respondents were asked to report exposure to such situations, irrespective of their particular role in these acts, and they were informed that they would not be asked to reveal their role. Index measures: *Fighting experience (6 acts)*, *Battle-field violence received (3 acts) Violence received (full) (9 acts)*, *Violence perpetrated (full) (3 acts)* are based on a set of specific acts and situations, which were all measured via a five-point frequency scale, ranging from never (= 0) to extremely often (= 4). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *YOB-Bin 2/4* collapses *YOB-Bin 2* and *YOB-Bin 4* into a joint bin. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Results for full index of exposure

	Community Meetings		Local Security	
	(1) OLS	(2) IV	(3) OLS	(4) IV
WG index	0.057***	0.152**	0.054***	0.113*
	(0.015)	(0.075)	(0.010)	(0.060)
Std. controls	Yes	Yes	Yes	Yes
Observations	760	760	760	760
F		12.96		12.96

Notes: WG index: full index measure of exposure to wartime governance, standardized to zero mean and unit standard deviation. *Std. controls*: same specification as in column 1 of Table 5. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A15: IV estimates from alternative specifications of the instrument

	Community meetings			Local security		
	(1) 24	(2) 2+4	(3) 2+3+4	(4) 24	(5) 2+4	(6) 2+3+4
WG high	0.338***	0.336***	0.343***	0.255**	0.251**	0.248**
	(0.140)	(0.140)	(0.138)	(0.119)	(0.119)	(0.116)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes
Post-Controls	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760
R^2	0.04	0.05	0.05	0.06	0.06	0.06
F-stat(IV)	48.59	30.81	21.71	48.59	30.81	21.71

Notes: WG high: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Length of Service*: length of wartime military service (in years). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Post-Location FE*: full set of *comuna* fixed effects. Pre-service controls omitted from model on grounds of statistical power (point estimates are not noticeable different when these are included). Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A16: Violence received: First-stage mechanics

	Violence received			
	(1)	(2)	(3)	(4)
UNITA x YOB-Bin 3	0.335***	0.383**	0.347**	0.388**
	(0.123)	(0.151)	(0.138)	(0.161)
UNITA x YOB-Bin 24			0.022	0.008
			(0.081)	(0.100)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes
Post-Controls	No	Yes	No	Yes
Pre-Controls	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes
Post-Location FE	No	Yes	No	Yes
Observations	760	760	760	760
R^2	0.08	0.11	0.08	0.11
F(UNITA x YOB-Bin 3 = 0)	7.46	6.41	6.29	5.79

Notes: UNITA (main): indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin x*: indicator of year of birth bin (1 = Yes, 0 = No). The sample is grouped into four bins (1 – 4), as explained in the text. *YOB-Bin 1* is the oldest group and the reference bin, YOB-Bin 4 is the youngest group. *YOB-Bin 24* collapses *YOB-Bin 2* and *YOB-Bin 4* into a joint bin. *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Length of Service*: length of wartime military service (in years). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.

Table A17: Robustness check: Violence received (OLS and IV)

	Community Meetings				Local Security			
	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) OLS	(6) OLS	(7) IV	(8) IV
Violence received	0.052**	0.030	0.105	–0.014	0.046***	0.024	0.096	0.022
	(0.021)	(0.023)	(0.255)	(0.230)	(0.015)	(0.016)	(0.154)	(0.159)
WG high		0.139***		0.349**		0.140***		0.214*
		(0.043)		(0.169)		(0.037)		(0.122)
UNITA+YOB-Bin	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Length of Service	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post-Location FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	760	760	760	760	760	760	760	760
R^2	0.13	0.15	0.12	0.11	0.09	0.13	0.07	0.12
F- VR			5.85	2.92			5.85	2.92
F-SW VR			5.85	4.92			5.85	4.92
F-AP VR			5.85	5.31			5.85	5.31
F- WG				153.61				153.61
F-SW WG				35.62				35.62
F-AP WG				198.74				198.74

Notes: *Violence received*: index of violence received, standardized to zero mean and unit standard deviation. *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *UNITA (main)*: indicator of main armed group (1 = UNITA, 0 = MPLA). *YOB-Bin*: full set of indicators of year of birth bin (1 = Yes, 0 = No). *Post-Controls*: radio ownership (1 = Yes, 0 = No), assets (index), years of schooling, born in this comuna (1 = Yes, 0 = No). *Pre-Controls*: vector of eight pre-service family background characteristics. *Pre-Location FE*: full set of recruitment region fixed effects. *Post-Location FE*: full set of *comuna* fixed effects. *F-VR*: 1st stage F-statistic "Violence received", *F-SW VR*: Sanderson-Windmeijer F-statistic "Violence received", *F-AP VR*: Angrist-Pischke F-statistic "Violence received". *F-WG*: 1st stage F-statistic "WG high", *F-SW WG*: Sanderson-Windmeijer F-statistic "WG high", *F-AP WG*: Angrist-Pischke F-statistic "WG high". *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). Significance levels: * p < 0.1, ** p < 0.05, *** p < 0.01. Robust standard errors in parentheses, in columns 1 and 3 clustered by municipality of recruitment, in columns 2 and 4 two-way clustered by *bairro* of residence and municipality of recruitment.

Table A18: Mechanisms: heterogeneity in age at entry

	Community Meetings			Local Security		
	(1)	(2)	(3)	(4)	(5)	(6)
Age at military entry	<18	18-24	>24	<18	18-24	>24
WG high	0.110*	0.227***	0.152**	0.134***	0.158***	0.220***
	(0.063)	(0.055)	(0.059)	(0.031)	(0.050)	(0.065)
Std. controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	319	277	163	319	277	163
R^2	0.18	0.22	0.15	0.14	0.17	0.25

Notes: *WG high*: indicator of high exposure to wartime governance (1 = Yes, 0 = No). *Std. controls*: same specification as in column 1 of Table 5. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses, two-way clustered by *bairro* of residence and municipality of recruitment.