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DOES WAR FOSTER COOPERATION OR PAROCHIALISM? EVIDENCE FROM A NATURAL EXPERIMENT AMONG TURKISH CONSCRIPTS

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Does War Foster Cooperation or Parochialism? Evidence from a Natural Experiment among Turkish Conscripts

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ABSTRACT

Exploiting a natural experiment and an innovative survey design, we study the causal impact of armed conflict exposure (ACE) on the sociopolitical attitudes and behaviors of the average male randomly picked from the population. Contrary to the arguments that war fosters cooperation, we find little evidence of social or political participation in exposed individuals. Instead, we document compelling evidence that ACE promotes opposition to peaceful means of conflict resolution, animosity towards minorities, and adherence to right-wing ideology. Further analyses show war-driven grievances, the normalization of violence in everyday life, and changes in conservative norms and preferences as the transmitting pathways.

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The Sociopolitical Legacies of War Exposure:
Evidence from a Natural Experiment among Turkish Conscripts

1. Introduction

What are the political and social legacies of armed conflict exposure? Existing literature offers compelling but contradicting answers. A line of inquiry finds that exposure to armed conflict induces individuals to exhibit prosocial behaviors including increased civic engagement, political participation, collective action, interpersonal trust, generosity and inequality aversion (Bellows and Miguel, 2009; Blattman, 2009; Voors et al., 2012; Gilligan et al., 2014; Bauer et al., 2014, Bauer et al., 2016, Bauer et al., 2018; Jha and Wilkinson, 2012). On the other hand, there is the observed persistence of conflicts and their tendency to recur, and the argument that this pattern can be explained by parochial responses to war exposure, such as increased nationalism, polarization, and reduced prosociality (Henrich, 2020; Collier et al., 2003; Rohner et al., 2013a, b; Grossman et al., 2015; Hager et al., 2019; Conzo and Salustri, 2019; De Juan et al., 2022; Vlachos, 2022).

This seeming contrast is attributed to the challenges in identifying the causal effects and isolating the complex psychological, economic, and social pathways that flow through (Blattman and Bauer, 2010; Bauer et al., 2016; Cederman and Vogt, 2017; Couttenier et al., 2019). Therefore, several vital questions remain intact, inviting suitable natural experiments and creative research designs to address them (Blattman and Bauer, 2010).

This study responds to that invitation by exploiting a population-level natural experiment, enabled by the strict universal military conscription in Turkey, staffing the Turkish Armed Forces during a deadly civil conflict in the southeast of the country. Within this setting, between 1984 and 2011, 97 percent of all men (roughly 14 million) reaching the age of induction were drafted, 93 percent of them served 15 to 18 months, and about one-fourth (nearly 3.5 million) were deployed to bases in conflict areas via a deployment lottery. We build on this natural experiment with a survey providing detailed information on military experiences, and political and social outcomes. We examine the causal impact of armed conflict exposure (ACE), measured by the intensity of armed conflict at deployment location during service, on social and political participation, attitudes towards conflict resolution and minorities, and general political inclination, and explore the potential mechanisms.

Our study design provides us with unique capabilities. First, exploiting the universality of the draft system in Turkey and the deployment lottery embedded in it (Official Gazette, 1927; 2019), we identify the impact of ACE for the randomly picked average male from the target population. Second, our empirical setup takes advantage of the geographical concentration of the conflict, and by sampling from provinces outside of and with negligible migration from the conflict areas, eliminates (i) any potential bias that may stem from unobserved exposure in civilian roles and (ii) confounding macroenvironmental effects of war. Consequently, because conscripts return to their peaceful hometowns upon discharge, we capture the impact of isolated and limited duration ACE during military service. Third, our data tracks actual experiences of armed violence as well, which not only allows us to demonstrate the validity of our exposure measure but also enables us to investigate the role of personal grievances on outcomes.

Equipped with these capabilities, we tackle major questions in the literature for which the verdict is still out. To begin, we contribute to the discussion regarding whether and to what extent conflict exposure is conducive to prosocial behaviors when environmental explanations, including the need for social insurance, security concerns, community-level paradigm shifts, and labor market fluctuations, which may all impact social cohesion, are minimized. Second, we test whether war exposure feeds the self-perpetuating dynamics of conflict. Third, we attempt to dissect war exposure into its military socialization and direct experiences of armed violence dimensions and explore the relative importance of these two main components of exposure in driving the outcomes. Finally, we explore the potential mechanisms, including human capital outcomes, psychological health, changes in norms and preferences, grievances, and normalization of violence.

Our main data source is the Exposure to Political Violence and Individual Behavior-Conscript Veterans (EXPOVIBE-CV) survey. The survey was conducted in 2019 in Turkey in 29 provinces outside of and with negligible in-migration from the conflict areas. 5,024 randomly selected adult males, conscripted in the 1984-2011 period, were interviewed.

We start our analysis by showing evidence on the orthogonality of ACE to pre-deployment characteristics. Then, examining direct combat experiences, we document that conflict intensity at the time and place of service substantially increases the likelihood of enemy firefight, injury, and witnessing casualties.

Upon showing evidence on our identification strategy's credibility and its potency in predicting direct combat experiences, we estimate the impact of ACE on our primary outcomes of interest. We do not find any significant favorable impact on the *Civic Participation Index*, a broad measure of prosociality constructed by using multiple indicators of participation in social and political life, including membership in various social and community organizations, voting in local and general elections, and political party membership.

We continue our analysis by examining the effects on attitudes towards conflict resolution and minorities, as well as self-placement on the left-right political spectrum. We find that service in an intense conflict environment increases the likelihood of being against multilateral deliberations for a peaceful solution to the ongoing conflict between the Turkish State and the PKK, supporting further military intervention as the best way to resolve it, feeling distant to minorities, being averse to living with minority neighbors, and favoring an iron-fist rule that would not hesitate to employ "even the harshest measures to destroy traitors and restore order in the country." Specifically, we find that service under intense conflict leads to a 0.3 standard deviation increase in the *Animosity Index*, summarizing the aforementioned variables, and also in the propensity to lean towards the right-hand side of the political spectrum.

These results largely persist years after discharge. We undertake an exhaustive set of sensitivity checks to test and confirm the robustness of our estimates.

We next tackle another significant void in the literature—unearthing the mediating pathways between ACE and prosociality. The findings provide strong evidence that the adverse effects of exposure flow through grievances, the normalization of violence, and subscription to conservative norms, highlighting how conflict exposure may contribute to the observed persistence of civil conflicts (Collier et al., 2003).

Our findings, therefore, indicate that societies with a history of conflict can risk further armed violence unless resolution efforts and reconstruction policies acknowledge grievances and involve measures to re-establish and strengthen inclusive and peaceful social and behavioral norms. These results also suggest that the employment of conscription armies and mass mobilization campaigns might add to the perpetuation risk by exposing civilians to the violence of armed conflicts, which then gets transmitted into social and political life through the deepening of social divides, accumulation of

grievances, tightening of conservative social norms, and normalization of violence as an acceptable way of resolving social problems.

2. Conflict Between the Turkish State and the Kurdistan Workers' Party (PKK)

The conflict between the Turkish state and the PKK has been raging since 1984. First founded as a separatist organization to establish an independent Kurdish state in southeastern Turkey, the PKK shifted its political agenda during the 90s towards a federational structure that would grant more autonomy to the region (Kıbrıs, 2011). Importantly, the conflict continued over the years with heavy casualties on both sides and remained geographically concentrated in the southeast of the country.

The temporal evolution of the conflict is demonstrated in Figure I, with Panels A and B showing the trends in annual and cumulative combatant casualties on both sides over the 1984-2019 period. The armed clashes gradually intensified in early 90s peaking in 1994 with 3610 combatant deaths, and declined afterward, consistent with the PKK's shift away from a separationist agenda (Kıbrıs, 2011). As the Figures demonstrate, fatalities exhibited a slowdown during ceasefires: between 1999 and 2004, after the founding leader of the PKK was captured and jailed in February 1999; in 2009, when the PKK announced a one-sided ceasefire; and between 2013 and 2015 during the (failed) Peace Process (Köse, 2017). Notably, although the information on the casualties of the state forces and PKK rebels comes from different sources, the two series exhibit a very high correlation, confirming the validity of the data we employ.

3. Deployment Lottery as the Source of Identifying Variation

Turkey has a draft military system which mandates each Turkish male resident citizen to serve in the Armed Forces. In particular, a young man becomes draft eligible when he turns 20 and typically gets inducted into the military before he turns 22 (Official Gazette, 1927; 2019). The duration of service within the period we consider ranged between 15 to 18 months.[2]

The conscription procedure starts with the draft call. Those called must surrender to the Military Enrolment Services of the Turkish Defence Ministry, which then assigns them to branches, military occupation classifications, and training centers. Detailed information on this classification step can be found on the official instruction brochures for prospective draftees emphasizing that the classification

[2] While the required duration was 18 months in the 80s, it was taken down to 15 months in 1992, brought back up to 18 months in 1995, taken down to 15 months in 2003, and remained so up until 2014.

is conducted electronically on anonymized records and is conditional on educational qualifications to meet the needs of the Armed Forces across its branches and tasks.[3] We present a flowchart of these instructions in Data Appendix G.

An important point to note here is that regulations allow postponement of service until completion of formal education (Official Gazette, 1927; 2019). That is, although everyone gets the draft call at the age of 20, those who are in school are allowed to postpone enlistment until they complete their formal education (or until they are 29, whichever comes first). Therefore, induction largely takes place after formal schooling is completed (Akyürek, 2010; Yıldırım and Erdinç, 2007). The law also states that while draftees with less than a bachelor's degree serve full term as rank-and-file soldiers, college graduates serve either as full-term sub-lieutenants or half-term rank-and-file soldiers depending on the needs of the Armed Forces in that draft period. Having said that, our data indicates half-termers to constitute a small share with 93 percent of conscripts serving a full term. More importantly, college-educated individuals remain subject to the lottery-based assignment system regardless of their rank and service duration.

Inducted conscripts first go through a short training of up to three months in training centers all of which, for security reasons, are outside of conflict areas. Upon completion of training, they get sent to military bases all over the country, with the exception of those in their registration province, to serve their terms. Importantly, conditional on the branch of service and military occupation, the deployment assignment is done randomly via a lottery which is publicly known as the "base lottery" (Mater, 1999 pp.13,42,114,131, 136; Dündar and Anwar, 2021).[4] As they were conducted in public, the recordings of base-lottery ceremonies can still be found on social media outlets (see https://www.youtube.com/watch?v=D3w4i07_Wj4 as an example). Over the years, through this lottery-based assignment system, millions of conscripts were deployed to bases in the southeast and found themselves as combatants in the conflict that was raging around them.[5]

[3] https://www.msb.gov.tr/Askeralma/icerik/siniflandirma-islemleri. Last visited on February 7, 2023.
[4] Mater interviews 42 ex-conscripts who had been deployed to intense conflict areas during their service. The interviews contain frequent references to the "lottery". A recently published biography of the current president Erdogan mentions how relieved he felt when he had drawn a base in Istanbul in his base lottery (Dündar and Anwar, 2021, p. 98).
[5] For a bilingual emphasis on the random nature of base assignments see, https://www.takvim.com.tr/guncel/2020/05/28/kutuge-gore-askerlik-nereye-cikar-kutuge-gore-askerlik-yeri-sorgulama/2

While the regulatory information provided by the Armed Forces emphasizes that, conditional on the needs of the Armed Forces across its branches and tasks, service location is independent of individual pre-enlistment characteristics of conscripts, one may still ask if this official narrative reflects the actual practice.[6] To address this concern, we perform formal balance tests to confirm our identifying assumptions.

There also exist plausible explanations supporting the regulatory narrative of the military. Because those serving in the conflict zone face severe morbidity and mortality risks, the assignment system and its fairness have always been under close scrutiny by the general public and the media, especially during periods of intensified conflict as a significant number of conscripts lost their lives or got seriously injured in clashes (Yıldırımkaya, 2010; Kıbrıs, 2011). Consequently, the Turkish Ministry of Defense and the General Staff emphasize in all their communications with the public that the system does not discriminate based on conscripts' socioeconomic status or family and social networks (Turkish Ministry of Defense, 2015).[7] Anecdotal evidence also supports the argument regarding the non-discriminatory nature of the system. For example, as in the 2007 incident when the first cousin of the then Secretary of the State died in a PKK attack while serving in the southeast, it is not uncommon to observe close relatives of high-level politicians among the fallen soldiers.[8,9] Furthermore, the fact that the military has long been the most trusted institution in Turkey attests to the fairness perception of the public with regard to military practices (Esmer, 1999; Adaman et al., 2005).

A relevant question regarding the credibility of our identification strategy is whether citizens can avoid deployment to conflict zones by dodging the draft or influencing the timing of induction. Unlike other countries with universal conscription, like Israel or South Korea, where a significant share of eligible men can avoid active duty service, young Turkish men have negligibly limited options to circumvent the draft, and escaping induction is not a practical alternative for them, either.[10] The

[6] (https://static.turkiye.gov.tr/downloads/kurumlar/msb/ERBAS_VE_ERLERIN_YASAL_HAKLARI.pdf."

[7] Note that relatives and children of high-rank bureaucrats and military officials are significantly more likely to extend their education beyond high school. Therefore, they are more likely to serve half-term and, consequently, more likely to have lower exposure to the conflict. On the other hand, sons of military officials are more likely to pursue professional military careers and as such they are more likely to be excluded from our survey.

[8] https://worldbulletin.dunyabulteni.net/archive/turkish-fms-cousin-killed-in-pkk-attack-h10956.html.

[9] One of the highly exposed ex-conscripts Mater interviews (1999, p.224) is the son of a high-level military official.

[10] In the Israeli case, exemptions are made on religious, physical, psychological, or lawful grounds. Also, one can refuse to serve on the grounds of pacifism, antimilitarism, religious philosophy, or political disagreement with Israeli policies. The Israeli High Court of Justice ruled in 2002 that refusal to serve was legal (https://military-

likelihood of obtaining a fraudulent health-ailment exemption is slim because it is subject to close scrutiny and requires several approvals from multiple entities.[11] Moreover, evaders face legal consequences and are shunned by society (Altınay and Bora, 2002). The legal consequences include forfeiting paid employment because male employees are legally required to provide their employers with a military discharge certificate upon hiring.[12] Draft evaders and those who help them also risk imprisonment of up to three years.[13] Therefore, the conscription system in Turkey constitutes a rare exception as all Turkish men, except a small fraction who were pardoned due to incapacitating health ailments and those who illegally avoid induction, get drafted and complete their service.

One may also be worried that differential treatment due to formal education can threaten our identification. However, all conscripts are subject to the lottery-based assignment system. Moreover, induction mainly occurs upon the completion of formal schooling which is the key input for the military in determining the branch and occupation classifications. Therefore, incorporating schooling level, an observable characteristic available in our data, among conditional random assignment covariates addresses such concerns.

4. Data and Measures

The EXPOVIBE-CV survey was conducted in Turkey in late 2019 to collect information on the military service experiences and a wide array of economic, social, and political attitudes of Turkish conscript veterans. We provide a detailed account of the survey procedures in Data Appendix A. 5,024 randomly selected adult males who completed military service between 1984 and 2011 were interviewed at their residential addresses in 29 provinces outside of and with negligible in-migration from the conflict zone to separate conflict exposure during military service from that of civilian experiences.[14] The survey focused on this period because with new legislation enacted in late 2011

history.fandom.com/wiki/Refusal_to_serve_in_the_IDF). South Korean has a broader definition of compulsory service that includes social work, research, full-time reserve enlistment, and industrial technical service.

[11] Those with serious health problems are given exemption if the diagnosis is approved by a panel of military doctors. What constitutes "a serious health problem" is defined in regulations (Turkish Armed Forces, Health Capability Regulation, Official Gazette 29530, 12 November 2015).

[12] https://www.haberturk.com/e-devlet-ten-askerlik-durum-belgesi-sorgulama-islemi-nasil-yapilir-hts-2378941.

[13] The Military Penal Code states that evasion is punishable by up to three years in prison, and employing a fugitive is punishable by up to two years in prison. https://www.mevzuat.gov.tr/mevzuatmetin/1.3.1632.pdf.

[14] 41 respondents had served prior to 1984. We exclude them from our analyses. Their inclusion does not change our results and conclusions.

(Official Gazette, 2011), the Armed Forces started recruiting professional soldiers on fixed-term contracts to replace conscripts especially in the conflict zone as part of a move towards a professional military. And with enough professional soldiers in place, regulations were relaxed after 2018 to allow civilians to pay their way out of military service.

We measure ACE by combining the information on the dates and location of service from EXPOVIBE-CV with data on the geo-temporal evolution of the conflict from the Turkish State-PKK Conflict Event Dataset (TPCONED) (Kıbrıs, 2021). The TPCONED provides precise information on the timing and granular location of conflict events and combatant casualties on Turkish soil. It contains detailed information on 7,196 conflict events, with 17,532 PKK and 7,572 state casualties over the course of the conflict. Accordingly, for each respondent, ACE corresponds to the total number of combatant casualties at his base district (town) within his dates of service.

Figure II maps the sample distribution alongside the distribution of combatant casualties to visualize the clear separation between the sampling and conflict areas, isolating conflict exposure during military service from that of civilian experiences. Moreover, separating respondents' living environments from the conflict zone silences potential macroenvironmental mechanisms, such as the physical destruction of war, post-conflict reconstruction, threats to personal security and property after discharge, and community-level paradigm shifts in political and social attitudes.

Appendix Figure I displays the geographical distribution of combatant casualties and the military placements of survey respondents at the district level. In Appendix Figure II, we repeat the heat maps for the number of casualties and service location of respondents for the periods 1984 to 1990, 1991 to 1995, 1996 to 2000, 2001 to 2005, and 2006 to 2011. This exercise shows that while conflict intensity exhibits significant variation over time at the district level, military placements of respondents to different bases across the country remain fairly stable indicating a good deal of cross-sectional and longitudinal variation in conflict exposure.

The EXPOVIBE-CV also collected information on direct combat experiences. Our first batch of dependent variables track those experiences based on questions presented in Data Appendix B. In particular, *Armed Combat* and *Injured or Witnessed Casualties* are dichotomous variables indicating whether the respondent engaged in armed combat, was injured himself or witnessed others getting hurt or

killed in armed combat during service. The binary variable *Any Direct Combat* reflects indicating at least one such combat experience.

Appendix Table 1 displays our calculations of the number of Turkish men exposed to direct armed combat during service between 1984 and 2011, using exposure risk estimates from the EXPOVIBE-CV and information on the number of men conscripted during the period obtained from the TurkStat birth statistics.[15] The estimates reveal the extent of direct armed violence experiences among Turkish men, with more than 2 million directly engaged in firefight, and witnessed deaths and injuries, and roughly 3 million exposed to at least one direct combat incident. To the best of our knowledge, these statistics represent the highest war theatre exposure rates for the average male among middle- or higher-income nations worldwide.[16]

To measure cooperative behaviors, we employ variables reflecting participation in various community and social organizations, political party membership, and voter turnout, using survey items shown in Data Appendix C. *The Social Participation Index* focuses on membership in social organizations like alumni associations and social and sports clubs. *The Community Participation Index* provides the corresponding measure for membership in community organizations including compatriot, local community, and school parents' associations, religious and secular charities, and trade and worker unions. *The Political Participation Index* represents voting in local and general elections and political party membership. These indexes are constructed as normalized weighted averages (with mean zero and standard deviation one) of the listed measures, respectively, with lower (higher) weights for variables with higher (lower) correlation levels with the others (Anderson, 2008). The *Civic Participation Index* summarizes all these measures in the same way.

Next, we construct variables capturing political attitudes using the survey questions provided in Data Appendix D. *Against Peaceful Solution* is a binary indicator, coded one for those who are somewhat or entirely against peaceful solutions, and zero otherwise. *Pro-Military Solution* is set to one for respondents who consider intensified military operations and armed combat as the best solutions to

[15] As military rules prevent conscripts to be deployed to bases in their registration provinces, the exposure rates are expected to be somewhat lower for those drafted from conflict regions. Accordingly, we use the population distribution statistics from TurkStat and the distribution of home locations of the casualties of the Armed Forces we obtain from the TPCONED to adjust these rates for the veterans from the conflict areas.

[16] While exposure rates among conscripts may also be high in cases like Israel, the risk for the average male remains much lower as nearly half the population avoids conscription through exemptions or refusal (Jager, 2018).

ending the ongoing armed conflict, and zero otherwise. *Feel Distant to Minorities* is a binary indicator of feeling distant to ethnic minority identities. *Against Minority Neighbors* is one for those would be opposed to having someone from an ethnic minority as a neighbor, zero otherwise. *Tolerates Severe Measures* is coded one for those who are not opposed to using even the most severe measures to put the country on the right path and eradicate the traitors, and zero otherwise. The *Animosity Index* aggregates the answers to these five questions into a single indicator as described above. Finally, the *Political Spectrum Index* is the normalized value of respondents' self-reported positioning on a 10-point left-right political ideology scale.

To address the potential non-linearities in the effect of ACE on our outcomes of interest and facilitate the interpretation of coefficient estimates, we categorize those for whom ACE equals zero as having *Non-ACE Service*, and we rank those with positive ACE into thirds as Low*, Medium*, and *High-Intensity ACE*. We also explored finer exposure categories, such as quartiles and quintiles, as well as using ACE as it is with no categorization. These exercises lead us to conclude that the effects of ACE are nonlinear. Therefore, a linear specification is unlikely to capture the true extent of the effects of conflict exposure. Also, as three equal ACE categories fairly successfully capture the impact of conflict, there is no need to adopt a finer exposure category.

We also create binary indicators reflecting service with and without direct exposure for each ACE intensity. *Low, Medium, and Intense Conflict with DAC* (direct armed combat) identify those with *Low*, *Medium,* and *High-Intensity ACE* and direct combat experiences during their service, respectively. Similarly, the variables *Low, Medium,* and *Intense Conflict no DAC* are created for respondents with *Low*, *Medium*, and *High-Intensity ACE* but no direct combat experience during their service, in that order.

5. Econometric Model

The following equation specifies our baseline model:

(1) $Y_{cp} = \alpha + \pounds_1(\textit{Low-Intensity ACE})_{cp} + \pounds_2(\textit{Medium-Intensity ACE})_{cp} + \pounds_3(\textit{High-Intensity ACE})_{cp} + \beta\Psi_{cp} + \Phi\Pi_{cp} + \delta\Omega_{cp} + \varepsilon_{cp}$

Y_{cp} denotes the outcome variable for conscript *c*, who served in province *p*. Coefficients $\pounds_1$, $\pounds_2$, and $\pounds_3$ capture the impact of *Low, Medium, and High-Intensity ACE,* representing conflict intensity in the deployment district during service.

Ψ is a vector of conditional random assignment (CRA) covariates, including fixed effects for draft year, birth province, branch of service, military occupation, training province, half-term service indicator, and years of schooling.

The Armed Forces declare that conditional on branch of service and military occupation, conscripts are randomly deployed to military bases across the country, excluding those in their registration province. Hence, the CRA variables include the branch of service, military occupation, and birth province fixed effects. Educational attainment is included because it is the main determinant of military branch and occupation. Given that the staffing needs of the military across locations vary over time, the service timing fixed effects are included as well. We also control for training province to ensure that any potential differentiation in the training content across locations based on branch or occupation does not bias our results.

Π contains plausibly exogenous pre-deployment characteristics, including height, ethnicity, draft age, conscript rank, training duration and service length in months. Note that the military rules state conscript rank to be unrelated to deployment assignment. Moreover, as they are determined prior to induction by the regulations in place, training and service durations should also be exogenous. Appendix Table 2 presents the summary statistics for these variables.

Additionally, to capture any service-location-specific unobserved heterogeneity, we control for service province fixed effects, represented by vector Ω.

ε_{cp} is the idiosyncratic error term. We cluster the standard errors at the service province level following the rule of thumb of clustering at the 'coarsest feasible level' when the number of clusters is sufficiently large (Cameron and Miller, 2015; MacKinnon et al., 2023).[17] We use ordinary least squares (OLS) to conduct our estimations.

6. Evidence on the Exogeneity of Deployment Assignment

The randomization of service location implies that the pre-deployment characteristics of draftees should be unrelated to ACE. We formally test this conjecture in Table 1 by conducting balance tests on pre-deployment covariates. Columns (1) to (4) present the means and standard deviations of pre-deployment variables by ACE intensity. Then, controlling for CRA variables, columns (5) to (7) show

[17] Clustering at the service province level yields 83 clusters with an average of 60 observations per cluster. As we demonstrate in the robustness section, our conclusions remain robust to adjusting standard errors for clustering at a number of different clustering levels.

the normalized differences and the associated p-values for Low, Medium, and High-Intensity ACE relative to Non-ACE Service.

The results support the orthogonality of exposure to pre-deployment characteristics. Moreover, the joint F-test p-values show that pre-deployment characteristics are jointly unrelated to ACE intensity. Appendix Table 3 presents balance tests on detailed exposure indicators based on which we conclude that our natural experiment enables us to distinguish the impact of exposure to the conflict environment with and without direct combat experiences. Then Appendix Tables 4 and 5 document evidence on the exogeneity of exposure among the at most high school educated, and college-educated men, respectively.

These results support our argument that our natural experiment identifies the causal impacts of ACE for the randomly picked male from the target population, rendering our findings generalizable to large segments of the population.

We next estimate the impact of ACE on direct combat experiences to gauge the potency of our exposure measure in capturing involvement in the war theatre.

First, in Appendix Figure III, we compare the annual combatant casualties obtained from the TPCONED to direct armed combat exposure rates by draft year in the EXPOVIBE-CV. This exercise shows that the trends in the prevalence of self-reported armed combat experiences closely track conflict intensity validating the accuracy and quality of both data sets.

Appendix Table 6 displays the descriptive statistics on direct combat experiences by ACE intensity. Table 2 then formally examines the impact of ACE on those experiences. In column (1), we find that *Low, Medium,* and *High-Intensity ACE* increase the likelihood of *Any Direct Combat* experience by 6.5, 11, and 17 percentage points, respectively. Column (2) and (3) show that the likelihoods of engaging the enemy in firefight and suffering or witnessing harm increase similarly with ACE intensity.

7. The Impact of Armed Conflict Exposure on Cooperation

In Table 3, we examine whether and to what extent ACE impacts the *Civic Participation Index* (Panel A), the *Animosity Index (Panel B),* and the *Political Spectrum Index* (Panel C), respectively.[18]

[18] As shown in Appendix Table 7, these outcome variables are weakly correlated.

In each case, column (1) shows the unadjusted estimate. Then, in columns (2), (3), and (4), we sequentially add CRA covariates, pre-deployment variables, and service province fixed effects to our models, respectively.

Panel A shows no positive impact of ACE on the *Civic Participation Index*. Instead, we find low to medium-intensity exposure to lower participation. While the inclusion of CRA covariates (column 2) increases the precision of estimates, the addition of neither the pre-deployment characteristics (column 3) nor service-province fixed effects make a meaningful change. In Appendix Table 8, we examine the impact of ACE on the components of the *Civic Participation Index*, including the *Social Participation Index*, *Community Participation Index*, and *Political Participation Index,* and document a similar pattern.

Next, in Panel B, we explore the effects on attitudes towards conflict resolution and minorities. We find *High-Intensity ACE* to cause a 0.3 standard deviation increase in the *Animosity Index*. Then in Appendix Table 9, we investigate the impacts on the components of the index, namely the likelihood of being against peace talks, supporting military measures for conflict resolution, feeling distant to ethnic minorities, being against having neighbors of minority ethnic origin, and not opposing the use of severe measures against traitors to put the country in the right path. Albeit less precise, estimates remain qualitatively similar to those on the *Animosity Index*.

Finally, in Panel C, we find that *High-Intensity ACE* causes a 0.3 standard deviation increase in the *Political Spectrum Index*, implying that conflict exposure boosts support for right-wing politics.

In Appendix Table 10, we show that the results are qualitatively similar across the two educational groups, even though less precisely estimated in the college-educated subsample.

In Table 4, we explore whether these effects dissipate over time. Panels I, II, and III reproduce the impact of ACE by limiting the sample to those discharged at least 10, 15, and 20 years ago, respectively. The stability of estimates across the panels suggests that the observed impacts of ACE are not transitory.[19]

Service in a conflict environment exposes conscripts to the military socialization of a war environment and to armed violence, both of which can trigger a number of mechanisms that can influence social and political outcomes. For instance, it is conceivable for the military indoctrination

[19] We also explored if these effects dissipate over time by interacting the exposure measures with years passed since discharge. This exercise, the results of which are available upon request, produces similar conclusions.

and socialization in combat environments to frame the adversary as an enemy that should be defeated militarily and to breed hostility and ethnocentrism (Grossman et al., 2015, Navajas et al., 2022). These findings, however, may also be explained by traumatic war experiences causing grief, boosting negative sentiments towards the out-group (Hirsch-Hoefler et al., 2014; Wood, 2008), and leading to "repeated decisions to fight" (Collier and Hoeffler, 2004; Cederman and Vogt, 2017).[20]

To explore the relative weights of military socialization and direct experiences of armed violence, we dissect our exposure indicators into ACE with and without direct combat experiences. The findings in the first column of Table 5 show that this dissection does not change our conclusion on the lack of any positive effects.

In column (2), we find that those who experienced direct combat in intense conflict environments drive the effects we observe on animosity. However, as the coefficient on *Intense Conflict no DAC* is not statistically distinguishable from the one on *Intense Conflict with DAC*, the potential role of military socialization cannot be completely overruled.

Finally, we repeat the same exercise in column (3) and find that while direct armed combat exposure during intense conflict has the largest impact (0.30 standard deviations) both military socialization and direct armed combat experiences matter in determining the impact of ACE on relative standing on the left-right political spectrum.

8. Robustness

We start our robustness tests by exploring whether a continuous measure (Appendix Table 12A) or a quartile categorization (Appendix Table 12B) of ACE better captures the impact of conflict exposure during service. As the effects usually emerge in the third and fourth quartiles of ACE intensity, we conclude that a linear measure fails to capture the true impact of conflict exposure.[21] Moreover, the quartile estimates suggest that our main specification captures the impact of ACE fairly well.

[20] https://forgehealth.com/the-impact-of-grief-and-loss-in-a-veterans-life-and-recovery-may-be-greater-than-people-think/
https://www.ocregister.com/2019/11/11/uci-study-examines-unseen-grief-of-soldiers-who-lost-friends-in-combat-or-by-suicide/

[21] Appendix Tables 11A and 11B display the associated balance tests for the linear and four-quartile ACE intensity specifications, respectively.

In Appendix Table 13, we use conflict intensity in service location prior to deployment, which we measure via the standardized total number of combatant casualties in service district up until deployment date, as a placebo exposure measure. Results confirm that our results are not biased by unobserved confoundment of ACE.

As previously discussed, educational attainment is instrumental in determining the military occupation and branch of service allocations which then condition deployment. Because those in higher education can apply for a deferral until the completion of formal schooling, induction largely takes place after the completion of formal schooling. Nonetheless, we further investigate whether potential reverse causation from ACE to education can bias our estimates. For this purpose, we first eliminate from the sample those who had postponed induction to complete their schooling to focus on those with the potential to continue their education after discharge. This elimination leaves us with those who served full term (i.e., at least 15 months) without postponement (i.e., inducted prior to their twenty-second birthday) as they might have continued on to higher education after discharge. However, results displayed in Appendix Table 14A do not show a significant impact of ACE on their educational attainment, recommending the absence of reverse causation from ACE to schooling.

Second, we create a predicted school completion age (PSCA) indicator based on the expected level of attainment at given ages if there are no interruptions in school attendance. Then, using the PSCA, we estimate our models by restricting our sample to those whose draft age and educational attainment are consistent with the assumption that their formal schooling ended prior to induction. In Appendix Table 14B, this exercise produces similar results to our main findings indicating no bias in our findings due to potential reverse causation from ACE to education.

In Appendix Table 15, we control for birth province by draft year fixed effects. This exercise relies on variation in conflict exposure across contemporary draftees sharing the same pre-enlistment environment and reveals no bearing on our main findings.

In Appendix Table 16, we show that ACE has no effect on the likelihood of non-response to questions pertaining to our outcome measures.

In Appendix Table 17, we limit ourselves to arguably more homogenous subsamples. Because of the landbound and treacherous nature of the terrain, soldiers and gendarmes are more likely to

serve in conflict areas and be exposed to direct combat in our case. Hence, in Panel I, we reproduce our estimates by excluding sailors and airmen.

Because of their shorter service duration, those who serve less than full-term may not be preferred to serve their duties in conflict zones. Accordingly, in Panel II we test whether the presence of less than full-termers (roughly seven percent of our sample) biases our findings by excluding them.

In Panel III, we limit our estimation sample to those inducted before turning 22 to test whether our findings are resilient to dropping individuals who delayed their draft. Finally, given the ethnic nature of the conflict, in Panel IV, we exclude individuals with non-Turkish ethnicity. Results show that these exercises have no bearing on our conclusions.

We also subject our findings to additional scrutiny by undertaking Oster's (2019) omitted variables bias test. As displayed in Appendix Table 18, Oster's beta values are nearly identical to our baseline estimates, suggesting that our natural experiment identifies unbiased estimates of the impact of ACE.

To test whether our results are driven by a small number of influential observations, in Appendix Figure IV columns (1) and (2), we drop one military-service province and one draft year at a time and plot the associated coefficient estimates and 95% confidence intervals. Estimates remain remarkably similar.

As we examine the impact of ACE on different outcomes, one potential concern is the problem of multiple inferences, which implies that statistically significant estimates may emerge by chance. While we already use summary indexes to circumvent this possibility, we nonetheless test the robustness of the standard errors to four different multiple-hypothesis-testing procedures including the Simes (1986) and the Hochberg (1988) methods, and those accounting for the familywise error rate (FWER) (Westfall and Young, 1993) and the false discovery rate (FDR) (Anderson, 2008), respectively.[22] In Appendix Table 19, we present the associated p-values using these approaches along with the baseline estimates and show that our results are robust.

As noted, standard errors in the main analysis are corrected for clustering on the military service province, which is arguably the 'coarsest feasible level' within which errors are correlated, as the conflict activity within the service province is likely to be correlated (Cameron and Miller, 2015;

[22] https://blogs.worldbank.org/en/impactevaluations/updated-overview-multiple-hypothesis-testing-commands-stata

MacKinnon et al., 2023). In Appendix Table 20, we check the sensitivity of the precision of our estimates to clustering at different levels. Specifically, we adjust standard errors at training province; service province and induction year (i.e., two-way clustering); service district; service district and induction year (i.e., two-way clustering); birth province; branch by draft year; branch by occupation; and branch by occupation by draft year, respectively. The results show that inference based on adjusting for clustering at different levels produces identical conclusions to our main analysis.

Finally, in Appendix Table 21, we control for interviewer-fixed effects. Results remain similar recommending that interviewer-specific idiosyncrasies do not influence our findings.

9. Mechanisms

Theory suggests that ACE may shape political and social attitudes and behaviors through several different channels, which may be broadly grouped under war-induced macroenvironmental transformations; human capital pathways; and psychological mechanisms (Galovski and Lyons, 2004; Grosjean, 2014; Bauer et al., 2016; Henrich, 2020; Blattman, 2009; Blattman and Annan, 2010).

The macroenvironmental effects of war concern the differential incentives and payoffs created by the conflict ecology, such as demand for social insurance due to risks to personal security and property, and community-level paradigm shifts (Bauer et al., 2016). The EXPOVIBE-CV survey was implemented outside the conflict zone to eliminate such effects. As participants did not live in areas where the conflict took place and caused any physical destruction or societal change, did not bear any risks to their personal security, property rights, or local social networks, nor took part in any form of post-conflict reconstruction, ecological explanations are irrelevant in our case by design. Importantly, this aspect of our empirical framework sets us apart from other well-known cases of conscription armies deployed in conflict, such as the case of Israel, where personal safety concerns have been offered among the explanatory pathways (Hirsch-Hoefler et al., 2014; Grossman et al., 2015). As further evidence that community-level effects of conflict are irrelevant in explaining our findings, we also show that our results are robust to exclusion from the sample of those who live in the provinces that experienced a few conflict events within their borders. As shown by the results in Appendix Table 22, there exists no reason to believe that our findings are contaminated by the potential social-ecological effects of conflict, including civilian exposure.

Conflict experience can also influence cooperative attitudes and behavior via the accumulation of social skills. Those who fight in conflict may need to hone their prosocial skills and attitudes to maximize social support and their likelihood of survival under scarce resources (Kalyvas, 2006). Such incentives, however, are either not relevant or play a minimal role in our case. First, as the Turkish military has the resources and the institutional structure to provide the conscripts with the social and technical support they need on the field, they may not feel the need to invest in their prosociality. Second, because induction takes place after formal schooling, conflict exposure is unlikely to affect human capital formation via educational attainment, as shown by our analysis in Section 7 above.

As the potential human capital formation effects of ACE may also be reflected in labor market outcomes and financial wellbeing, we explore the impact of ACE on such outcomes, as well. We show in Table 6 that ACE has no sizable impact on the likelihood of being unemployed (column 1) or family income (column 2). These results suggest that the impacts of ACE do not work through labor market success. Relatedly, it is worth noting that there is no financial compensation for service in conflict zones (excluding those who were disabled due to severe injury during service) or any favorable treatments directed to conscripts in their civilian lives upon discharge (Açiksöz, 2015).[23]

Conflict-induced migration could be another channel related to human capital. Although the EXPOVIBE-CV was administered in provinces with low levels of in-migration from conflict areas, one may still be concerned if ACE causes subsequent migration and whether and to what extent such migration explains our findings. To address such concerns, we show in Appendix Table 23A that ACE does not impact the likelihood of currently living in a province different from birthplace. Then, in Appendix Table 23B, we re-estimate our models by excluding such migrants from the sample and find that none of our conclusions change. Thus, we infer that potential conflict-induced migration does not explain our findings.

Armed conflict exposure can also operate through its effects on general preferences and psychological channels. On the one hand, a group of studies show evidence that exposure to war may lead to post-traumatic growth inciting positive change (Tedeschi and Calhoun, 2004; Blattman, 2009;

[23] Disability status due to injury during service is only provided to those with more than 40% impairment in accordance with Article 52 of the 5434 Law on the Pension Fund (https://www.mevzuat.gov.tr/MevzuatMetin/2.3.41053.pdf). Accordingly, only 0.5 per thousand of the conflict area veterans receive compensation. Unlike the veterans in the USA, psychological ailments, such as PTSD, do not usually quality towards disability status (Güloğlu, 2016).

Bauer et al., 2016). On the other hand, war traumas can lower prosociality by hindering a person's desire and ability to establish and maintain healthy social relationships (Ehlers and Clark 2000; Galovski and Lyons 2004). To gauge the role of mental health and risky health behaviors in explaining our negative findings, we explore depressive symptomology via a 6-question brief depression scale (Derogatis, 1975) that required respondents to indicate on a 5-point Likert scale from 0 to 4 how frequently they had felt sadness, loneliness, hopelessness, withdrawn, worthless, and suicidal within the past week.

In column (3) of Table 6, we examine the total score on these questions as a binary 90^{th} percentile indicator, capturing the upper end of this scale, and find no evidence that ACE triggers such symptoms. Relatedly, in column (4), we find that ACE does not lead to personal security concerns, a proxy measure we have to explore anxiety. Our measure, the *Personal Safety Index*, is derived from an 8-item insecurity scale (Vélez et al., 2016), listed in Data Appendix F, that questions participants' perceptions of risks to personal safety in daily life.

As evidenced by the literature, mental health ailments are significant risk factors for substance abuse, and relatedly, they are also likely to hinder labor market prospects (Kulka et al., 1990; Zatzick et al., 1997). In columns (5) and (6) of Table 6, we estimate the effect of ACE on smoking and daily drinking and document that it does not have a positive impact on these risky health behaviors, either. Coupled with our null findings on employment and income, these results, do not recommend that the impact of ACE on our outcomes operates through mental distress.

Finally, exposure to an environment where resorting to violence is socially acceptable may trigger a learning process that leads to the normalization of violence (Bandura, 1973; Horowitz and Solomon, 1978; Wood, 2008; Navajas et al., 2022). Moreover, evolutionary accounts argue that intergroup competition (i.e., war exposure) has been responsible for solidifying interdependent groups and strengthening norms that promote group cohesion (Henrich and Boyd, 2001; Richerson and Boyd, 2001; Henrich, 2020).

In columns (7) and (8) of Table 6, we explore whether norm tightness and normalization of violence can explain our findings. Our tightness measure is the standardized answers to a question that requires respondents to indicate on a 5-point Likert scale how much they agree with the statement that "the key to living well is obedience, discipline, and compliance with ethical behavior." Then, in

column (7), using Anderson's methodology (2008) as described above, we construct an *Aggression Index* based on the 12-question brief version of the Buss-Perry Aggression Scale (Buss and Perry, 1992) developed by Webster et al. (2013).[24] Estimated coefficients show norm tightness and normalization of violence to be a likely pathway. In particular, we show that direct armed combat exposure is the primary driver of such social conservatism and aggression, with effect sizes increasing in ACE intensity.

We also investigate if and how much controlling for these potentially endogenous outcomes affects our results. In Appendix Table 24 we investigate the resilience of our findings for each summary index outcome measure. In column (1), we show the baseline estimate. We control for unemployment status and family monthly income in column 2; mental health and risky health behaviors in column 3, and aggression and norm following in column 4. Then, column (5) jointly specifies these potential mediators. As the estimates indicate, controlling for these covariates has no bearing on our findings, regardless of whether we specify them individually or jointly.

As exposure during service can make veterans more sensitive to news on their service location, post-deployment conflict intensity may constitute another channel through which ACE can impact veterans. We explore this channel in Appendix Table 25 to find that conflict intensity after discharge does not meaningfully impact our results.

10. Conclusions

In this study, we examine the causal impact of armed conflict exposure on the social and political attitudes and behaviors of the average adult male randomly picked from a population inhabiting a peaceful environment where conflict-induced demand for cooperation is absent. We identify the effect of exposure to armed violence in and of itself and decipher the potential explanatory channels these impacts work through without confoundment by the macroenvironmental effects of war.

We provide answers to long-standing questions in the literature. First, we show that when neoclassical explanations, including the need for social insurance, security concerns, community-level paradigm shifts, and labor market outcomes that may boost demand for cohesion, are minimized, conflict exposure in and of itself does not foster cooperation. Our ability to silence such mediating channels not only solves an important puzzle in the literature but also underlines the importance of

[24] The questions are listed in Data Appendix E.

the conflict-induced boost in demand for cooperation in explaining increased prosociality due to war exposure as we do not see any such effect in its absence.

Instead, once such channels are neutralized, animosity and intransigence take over and the adverse effects of exposure flow through grievances, normalization of violence and conservatism. Accordingly, these dynamics may play significant roles in the formation of the infamous *conflict trap* and be highly relevant for understanding why "once a country stumbles into civil war, its risk of further conflict soars" (Collier et al., 2003, pp. 4).

To deepen the understanding of our findings, it is worth discussing the differences between the war experience of conscripts and those who participate in the conflict in different roles. Our subjects are ex-conscript civilians randomly picked from the general population. While they are exposed to conflict as combatants, they do not self-select into this role. Therefore, their involvement in the conflict is not ideological and does not necessarily involve any sociopolitical concerns. Moreover, unlike insurgent combatants, conscripts do not need to establish local support for survival. As part of the state military, they are already embedded in a well-defined, well-organized, and dense support system. Therefore, they face much lesser incentives to develop prosocial skills and attitudes during or after service. Finally, upon discharge, they return to their peaceful home environments, where there is no reason to expect war-induced paradigm shifts. Note that this description fits the experience of the members of most modern-day armies. In particular, our conclusions are highly relevant for cases where similar institutional setups draft civilians to participate in armed combat away from their homes. A timely example is the recent mobilization of Russian conscripts with minimal military training to be deployed in the invasion of Ukraine away from their peaceful homes (Roth, 2022).[25] The mass conscription campaign in Eritrea as part of its involvement in neighboring Ethiopia's civil war and the universal draft in Iran alongside the ongoing armed conflict with the PJAK insurgents in the northwest of the country constitute other current cases in which civilian conscripts are exposed to armed violence.[26] Armenia and Azerbaijan have resorted to mass mobilization campaigns and deployed conscripts to armed combat in the latest episodes of their conflict over Nagorno-Karabakh (Mejlumyan, 2020). Similarly, the border conflict between Kyrgyzstan and Tajikistan risks exposing

[25] https://www.theguardian.com/world/2022/sep/22/russia-mobilisation-ukraine-war-army-drive.
[26] https://www.bloomberg.com/news/articles/2022-10-18/eritrea-goes-for-broke-in-ethiopian-civil-war-to-crush-old-foe

conscripts to an armed conflict environment as the border guards of both countries are staffed largely by conscripts.[27,28]

There are also limitations to our study that originate from the structure and content of the EXPOVIBE-CV survey. First is the absence of information on parental characteristics like education, occupation, religiosity, or political views; neither do we observe whether respondents grew up in rural or urban areas. While such background characteristics would have enriched our analysis, the survey does not include these measures.

Another caveat is the absence of information on post-traumatic stress disorder (PTSD), which has been shown to play a central role in explaining the links between combat exposure and subsequent outcomes. We partially address this limitation by using depressive symptoms, which usually co-emerge with PTSD (Dekel et al., 2014; Campbell et al., 2007) and can be considered as a reasonable proxy for traumatic stress (O'Donnell et al., 2004; Sher, 2005). Furthermore, as suggested by the associated literature, PTSD and psychological health problems are important risk factors for substance use and can adversely impact labor market outcomes (Kulka et al., 1990; Zatzick et al., 1997). Based on our null findings on substance use and labor market outcomes, we infer that psychological health is unlikely to be a lead explanatory pathway.

Finally, as the questions on direct combat experiences were asked earlier in the survey, priming may occur if recalling direct combat experiences causes the under- or over-reporting of our outcomes of interest. Two pieces of evidence we provide suggest that priming does not influence our findings. First, as more recent traumatic experiences may be remembered more intensely, one would expect that the magnitudes of the coefficient estimates would diminish as time since discharge increases (Moya, 2018.) As presented in Table 4, the magnitudes of the impact of ACE do not exhibit any such decline. Second, if being asked about combat experiences were to cause a priming impact, that would be a psychological consequence of conflict exposure itself, and we would expect it to reflect on the psychological health outcomes we explore. However, we do not observe any measurable impact of exposure on mental health outcomes. Therefore, it is unlikely that our findings are driven by potential

[27] https://www.globalsecurity.org/military/world/centralasia/tajik-personnel.htm

[28] In the Israeli case, security concerns are not eliminated upon returning home. Similarly, because a soldier is usually drafted into the division in his home region, Colombian conscripts do not have isolated conflict exposure during service (Sacquety, 2006).

priming effects. Furthermore, evidence suggests that the effects of trauma on behavioral and attitudinal outcomes operate independent of any priming effects (Moya, 2018; American Psychiatric Association, 2013; Kessler et al., 1995; Mcewen and Sapolsky, 1995; Yehuda, 2002).

FIGURES and TABLES

Figure IA. Annual Fatalities Among the Turkish Security Forces and PKK Recruits

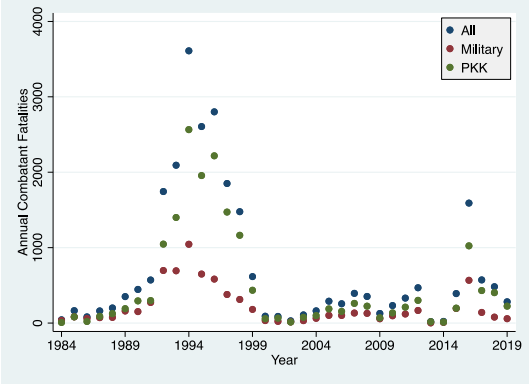
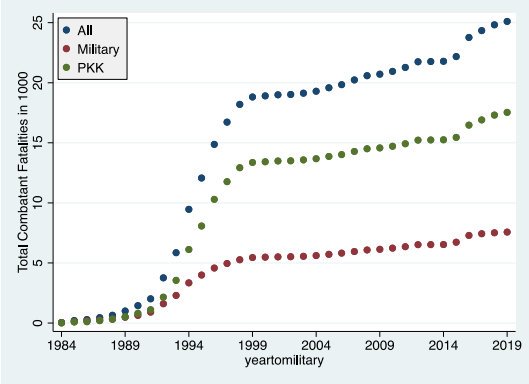


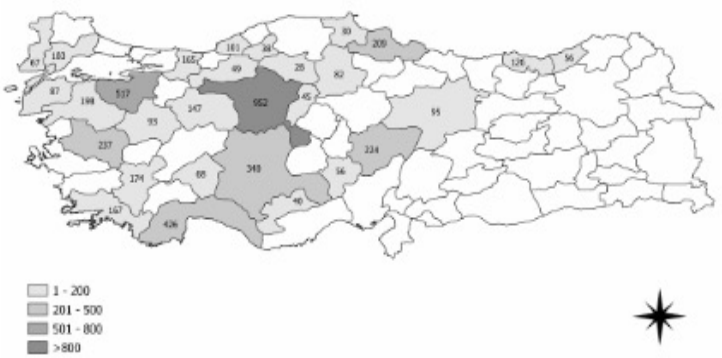
Figure IB. Cumulative Fatalities Among the Turkish Security Forces and PKK Recruits



Notes: The estimates were obtained from the TPCONED.

Figure II. Sampling distribution versus the distribution of combatant casualties

Sampling Distribution



Distribution of Combatant Casualties, 1984-2019

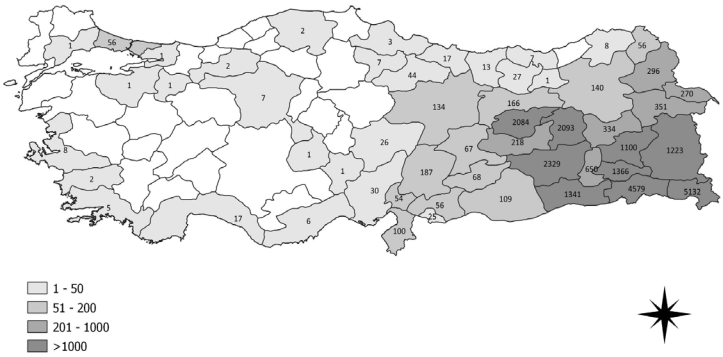


Table 1. Evidence on the Exogeneity of ACE, Full Sample

	(1) *No-ACE* Mean/SD	(2) *Low-Intensity ACE* Mean/SD	(3) *Medium-Intensity ACE* Mean/SD	(4) *High-Intensity ACE* Mean/SD	(5) Normalized Difference & P-value (1)-(2)	(6) Normalized Difference & P-value (1)-(3)	(7) Normalized Difference & P-value (1)-(4)
Height (cm)	175.816	175.082	175.650	176.667	0.105	0.024	-0.122
	[15.170]	[12.260]	[6.736]	[19.342]	(0.062)	(0.487)	(0.850)
Turkish Ethnicity	0.916	0.907	0.910	0.933	0.029	0.019	-0.060
	[0.429]	[0.336]	[0.252]	[0.297]	(0.570)	(0.623)	(0.579)
Kurdish Ethnicity	0.062	0.059	0.054	0.047	0.013	0.033	0.060
	[0.305]	[0.228]	[0.244]	[0.276]	(0.606)	(0.452)	(0.892)
Other Ethnicity	0.022	0.034	0.036	0.020	-0.077	-0.090	0.015
	[0.243]	[0.206]	[0.173]	[0.099]	(0.259)	(0.094)	(0.334)
Conscription Age	20.675	20.452	20.421	20.357	0.125	0.140	0.179
	[2.711]	[1.374]	[1.890]	[1.155]	(0.793)	(0.254)	(0.197)
Rank: Private	0.800	0.786	0.811	0.859	0.034	-0.026	-0.145
	[0.593]	[0.460]	[0.509]	[0.367]	(0.336)	(0.924)	(0.696)
Rank: Corporal	0.062	0.063	0.062	0.038	-0.004	-0.001	0.097
	[0.360]	[0.220]	[0.330]	[0.150]	(0.863)	(0.854)	(0.915)
Rank: Sergeant	0.131	0.138	0.122	0.098	-0.019	0.027	0.097
	[0.446]	[0.422]	[0.299]	[0.346]	(0.748)	(0.981)	(0.706)
Rank: Sub-Lieutenant	0.007	0.013	0.005	0.005	-0.069	0.017	0.021
	[0.081]	[0.107]	[0.069]	[0.084]	(0.073)	(0.823)	(0.725)
Training Duration in Months	2.617	2.713	2.777	2.773	-0.122	-0.203	-0.199
	[1.667]	[1.306]	[0.917]	[1.460]	(0.329)	(0.892)	(0.422)
Service Length in Months	16.457	16.779	16.904	17.215	-0.118	-0.163	-0.278
	[4.879]	[3.093]	[2.852]	[3.825]	(0.868)	(0.524)	(0.080)
F-test of joint significance (p-value)					*0.442*	*0.834*	*0.311*
	3874	423	318	356			

Notes: These estimates are obtained using sampling weights. In columns (1) to (4), means and standard deviations [in brackets] by ACE are presented. In columns (5) to (7), normalized differences are obtained by controlling for conditional random assignment (CRA) covariates including draft year, birth province, training province and service province fixed effects, branch of service indicators, military occupation dummies, a half-term service indicator, and dichotomous educational attainment indicators. P-values, adjusted for clustering on the province of military service, for normalized differences are in parenthesis.

Table 2. The Impact of ACE on Direct Combat Experiences

Dependent Variable:	(1) Any Direct Combat Experience	(2) Armed Combat	(3) Injured or Witnessed Casualties
Low-Intensity ACE	0.065*	0.065**	0.026
	(0.037)	(0.026)	(0.034)
Medium-Intensity ACE	0.110**	0.127***	0.113***
	(0.045)	(0.039)	(0.037)
High-Intensity ACE	0.169**	0.195***	0.139**
	(0.066)	(0.059)	(0.062)
Observations	4,968	4,968	4,967
R-squared	0.406	0.400	0.331

Notes: Estimates are obtained using OLS with sampling weights. Robust standard errors corrected for clustering on the province of military service are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control for CRA covariates and pre-deployment characteristics (height, birth quarter dummies, land ownership status, ethnic minority indicators, draft age, military rank dummies, training, and service duration).

Table 3. The Impact of ACE on Political and Social Attitudes and Behavior

	(1)	(2)	(3)	(4)
Panel A. Civic Participation Index				
Low-Intensity ACE	-0.046	-0.110**	-0.112***	-0.112**
	(0.045)	(0.042)	(0.041)	(0.050)
Medium-Intensity ACE	-0.117*	-0.117**	-0.120**	-0.093
	(0.069)	(0.058)	(0.057)	(0.069)
High-Intensity ACE	-0.013	-0.007	-0.009	0.010
	(0.042)	(0.055)	(0.055)	(0.068)
Observations	4,971	4,971	4,971	4,971
R-squared	0.001	0.072	0.079	0.097
Panel B. Animosity Index				
Low-Intensity ACE	0.164**	0.063	0.055	0.050
	(0.073)	(0.056)	(0.053)	(0.059)
Medium-Intensity ACE	0.130**	0.042	0.035	0.043
	(0.055)	(0.052)	(0.054)	(0.062)
High-Intensity ACE	0.296***	0.271***	0.278***	0.300***
	(0.070)	(0.076)	(0.075)	(0.092)
Observations	4,969	4,969	4,969	4,969
R-squared	0.007	0.196	0.208	0.225
Panel C. Political Spectrum Index				
Low-Intensity ACE	0.022	0.050	0.054	0.075
	(0.047)	(0.046)	(0.045)	(0.056)
Medium-Intensity ACE	-0.064	-0.060	-0.058	-0.029
	(0.068)	(0.063)	(0.060)	(0.067)
High-Intensity ACE	0.221***	0.235***	0.243***	0.299***
	(0.055)	(0.050)	(0.048)	(0.091)
Observations	4,499	4,499	4,499	4,499
R-squared	0.004	0.147	0.155	0.181
Controls For				
Conditional Random Assignment Variables	No	Yes	Yes	Yes
Pre-deployment characteristics	No	No	Yes	Yes
Service Province Fixed Effects	No	No	No	Yes

Notes: Estimates are obtained using OSL with sampling weights. Robust standard errors corrected for clustering on the province of military service are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Effects Over Time

Dependent Variable:	(1) Civic Participation Index	(2) Animosity Index	(3) Political Spectrum Index
Panel I. Discharged >=10 Years Ago			
Low-Intensity ACE	-0.168***	0.027	0.074
	(0.048)	(0.057)	(0.063)
Medium-Intensity ACE	-0.120*	0.053	-0.012
	(0.061)	(0.060)	(0.066)
High-Intensity ACE	-0.014	0.299***	0.282***
	(0.067)	(0.089)	(0.090)
Observations	4,590	4,588	4,148
R-squared	0.098	0.227	0.184
Panel II. Discharged >=15 Years Ago			
Low-Intensity ACE	-0.151**	0.055	0.065
	(0.059)	(0.068)	(0.069)
Medium-Intensity ACE	-0.108	0.049	-0.044
	(0.077)	(0.060)	(0.074)
High-Intensity ACE	-0.019	0.341***	0.297***
	(0.085)	(0.085)	(0.085)
Observations	3,650	3,648	3,306
R-squared	0.113	0.235	0.191
Panel III. Discharged >=20 Years Ago			
Low-Intensity ACE	-0.108	0.085	0.072
	(0.075)	(0.085)	(0.098)
Medium-Intensity ACE	-0.124*	0.016	0.070
	(0.073)	(0.074)	(0.093)
High-Intensity ACE	-0.077	0.291**	0.350***
	(0.096)	(0.111)	(0.097)
Observations	2,532	2,531	2,302
R-squared	0.141	0.247	0.230

Notes: Estimates are obtained using OLS with sampling weights. Robust standard errors corrected for clustering on the province of military service are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control for CRA covariates and pre-deployment characteristics.

Table 5. The Impact of Detailed ACE on Political and Social Attitudes and Behavior

Dependent Variable:	(1) Civic Participation Index	(2) Animosity Index	(3) Political Spectrum Index
Low Conflict no DAC	-0.098	-0.008	0.047
	(0.066)	(0.063)	(0.069)
Low Conflict with DAC	-0.122**	0.131	0.117
	(0.059)	(0.107)	(0.095)
Medium Conflict no DAC	-0.212**	0.075	-0.013
	(0.095)	(0.083)	(0.084)
Medium Conflict with DAC	0.014	0.021	-0.038
	(0.060)	(0.080)	(0.082)
Intense Conflict no DAC	0.041	0.320	0.294**
	(0.082)	(0.214)	(0.136)
Intense Conflict with DAC	0.014	0.295***	0.304***
	(0.069)	(0.075)	(0.107)
Observations	4,971	4,969	4,499
R-squared	0.098	0.225	0.181

Notes: Estimates are obtained using OLS with sampling weights. Robust standard errors corrected for clustering on the province of military service are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control for CRA covariates and pre-deployment characteristics.

Table 6. The Impact of ACE on Potential Mediators

Dependent Variable:	(1) Unemployed	(2) Log Family Income	(3) Depressed	(4) Personal Safety Index	(5) Smoker	(6) Daily Drinker	(7) Aggression Index	(8) Norm Tightness
Low Conflict no DAC	0.016	0.022	0.004	-0.014	-0.046	-0.002	0.097	0.067
	(0.013)	(0.035)	(0.022)	(0.061)	(0.035)	(0.013)	(0.060)	(0.081)
Low Conflict with DAC	0.003	-0.049	0.005	-0.037	0.062*	0.010	0.151	0.184**
	(0.015)	(0.049)	(0.032)	(0.084)	(0.036)	(0.016)	(0.111)	(0.077)
Medium Conflict no DAC	-0.019	0.047	-0.013	0.059	-0.021	-0.013	0.091	0.048
	(0.012)	(0.067)	(0.032)	(0.085)	(0.054)	(0.012)	(0.137)	(0.108)
Medium Conflict with DAC	0.008	-0.061	0.015	-0.052	0.044	-0.025**	0.256***	0.174**
	(0.019)	(0.043)	(0.031)	(0.095)	(0.040)	(0.010)	(0.080)	(0.081)
Intense Conflict no DAC	0.044	-0.362*	0.122*	-0.019	0.022	-0.033	0.067	0.078
	(0.032)	(0.187)	(0.070)	(0.099)	(0.059)	(0.021)	(0.158)	(0.185)
Intense Conflict with DAC	-0.002	-0.059	0.045	-0.058	0.066	-0.024	0.261**	0.131
	(0.014)	(0.050)	(0.034)	(0.089)	(0.041)	(0.016)	(0.122)	(0.079)
Observations	4,967	4,338	4,971	4,970	4,971	4,965	4,962	4,920
R-squared	0.098	0.210	0.162	0.168	0.140	0.075	0.160	0.223

Notes: Estimates are obtained using OLS with sampling weights. Robust standard errors corrected for clustering on the province of military service are in parentheses. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. All models control for CRA covariates and pre-deployment characteristics.

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